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ERRATA.

Page 6, l. 14 from top, for "1847," read "1845."—P. 7, l. 7 from top, for "Regent," read "Honorary Member."

P. 76, l. 27 from top, for "produced," read "protected."

P. 53, line 13 from bottom, for "$t = 1, 2, 3$," read "$t = 0, 1, 2, 3$."

P. 136, foot-note 3, insert "om" before Stoffernes.

P. 146, line 18 from bottom, in table, for "92·3," read "72·3."

p. 148, in formula (47), for $\frac{de}{dt}$ read $\frac{dc}{dt}$.

p. 149, l. 19 from bottom, for γ' read γ^t.

P. 312, foot-note 1, for "this memoir," read "the memoir of which this is an abstract."

THE

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[SECOND SERIES.]

BENJAMIN SILLIMAN.

OUR honored associate, Professor BENJAMIN SILLIMAN, the founder of this Journal, whose name has appeared upon the title page of every number, from the first until the present, is with us no more. He died at his residence in New Haven, early Thursday morning, Nov. 24, 1864 (the day set apart for a national thanksgiving), having reached the age of 85 years.

It becomes our duty to place on record in these pages, as an inscription to the monument which he has himself erected, an outline of his career and a tribute to his memory. Few men enter life with such promise as he; fewer still sustain themselves so evenly, and die so widely lamented.

Instruction in natural science has been his great work; and in it he was emphatically a man of the times. Beginning when almost nothing was known in this country of the departments to which he was especially devoted, he lived to see them carried forward to a high degree of progress, and their importance everywhere acknowledged. His life, which was one of few marked incidents, was passed in his native State, in connection with Yale College, the institution that early selected him as one of its faculty. Two or three times he was invited to become the president of colleges elsewhere, but New Haven continued his chosen home. Twice he visited Europe, first in 1805–6, in order to qualify himself for his work in life by attendance upon lectures in London and Edinburgh, and by observation of foreign institutions of learning; and again, near the close of his life, in 1851, when he was accompanied by his son, and made a more extended tour of observation and inquiry. Frequent journeys in his own country made him acquainted personally with the institutions and the men of every State, while his habits of prompt and friendly correspondence perpetuated the intimacies which he formed at home and abroad.

Without attempting a formal biography (which the late day of his decease renders impossible at this time), we propose to speak briefly of Professor Silliman's career as an officer of Yale College, and as a man of science, and then of his personal character and influence in the community.

The Silliman family has resided in Fairfield, Conn., since the early colonial days. Tradition says that Claudio Sillimandi, their earliest known ancestor, was driven, in 1517, from Lucca, Italy, to Switzerland, by religious persecution. The descendants resided in Berne, and afterward in Geneva, whence they emigrated through Holland to this country about the middle of the seventeenth century. A worthy pastor of the name, living with his family near Neuchatel, was visited by Prof. Silliman in 1851.

Ebenezer Silliman, the grandfather of Benjamin, graduated at Yale College in 1727, and Gold Selleck, the father, in 1752. The latter was a Brigadier General of militia in the Revolution, and was entrusted for a time with the defence of the Long Island coast. In 1775 he was married to Mary, the daughter of Rev. Joseph Fish of Stonington, and the widow of Rev. John Noyes. The two children of this marriage, Gold Selleck and Benjamin, became members of the same class in college, and have maintained through life an intimacy peculiarly fresh and cordial. The younger brother, Benjamin, was born in North Stratford, Conn. (now the town of Trumbull), August 8, 1779. The elder, who was born in 1777, is still living in Brooklyn, N. Y.[1]

Throughout his active life, Professor Silliman has been identified with Yale College. He entered the institution in 1792, graduated in 1796, became a Tutor in 1799, was appointed Professor of Chemistry and Natural History in 1804; and in 1853, having been relieved at his own request from further service as an instructor, he was designated, by the Corporation, Professor *emeritus*. Thus, during a period of nearly three-quarters of a century, his name has appeared as a student and teacher successively on the catalogues of the college. He was a pupil both of Dr. Stiles and Dr. Dwight, and the colleague of the latter during eighteen years. With President Day and Professor Kingsley he was associated for half a century and more in the government of the institution.

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[1] Prof. Silliman was twice married: first, in 1809, to Harriet, daughter of the second Gov. Trumbull of Connecticut, the mother of his nine children; and again, in 1851, to Mrs. Sarah Webb, daughter of John McClellan. Five children survive him, one son and four daughters. All are married, the eldest daughter to J. B. Church, the second to Prof. O. P. Hubbard, the third to Prof. J. D. Dana, and the fourth to Rev. E. W. Gilman. His descendants include twenty-three grandchildren, besides five deceased, and two great-grandchildren.

In the capacity of a college officer, he was preëminent as a teacher. The professor's chair, in the laboratory or the lecture-room, was the place above all others in which his enthusiasm, his sympathy with youthful aspirations, his varied acquisitions, his acquaintance with the world of Nature and of Art, and his graceful utterance, exerted their highest and most enduring influence. The minds which he aroused to the study of Nature have become investigators and teachers in every portion of the country, and all his pupils, whether devoted to science or to letters, will bear testimony to the interest which he awakened in these pursuits. They will never forget the admirable tact with which the manipulations of the laboratory were performed, or the brilliant experiments in chemistry which the lecturer seemed to enjoy as if, like the class, he had never witnessed them before. The course in chemistry, in early years, extended through one hundred and twenty lectures. In later days it was not so long, but was followed by a course in mineralogy and another in geology. Here, too, Prof. Silliman had the same magnetic influence on his students, sending them off on long walks about New Haven and at home to search for specimens, or to study the phenomena of geology. The third of these annual courses, that on geology, he gave with peculiar zest and eloquence. He delighted to depict the catastrophes of geological history and to clothe the world with the plants and animals of former days.

Professor Silliman was less concerned in the government of the students than some of his associates; but questions were continually arising in which his counsel was of weight. He was prompt in rebuking every form of youthful delinquency, yet was never harsh or inconsiderate. No student ever left his presence feeling wronged or indignant. He would much rather sacrifice a rule than injure an offender. If he seemed sometimes to be lenient, it was the leniency of a father, for his mind regarded the improvement of his scholars rather than the enforcement of routine and discipline. His paternal lectures to the Freshman class on morals and manners were admirable in their influence, and many a graduate of the college will acknowledge that his habits for life were affected by the judicious hints which he received from his kind and sympathising teacher.

Mr. Silliman's labors began with instruction; but they did not end there. His active and versatile disposition led him to become interested in and to help forward whatever would contribute to the welfare of Yale College. When he went abroad, in 1805, to fit himself for the duties of his professorship, the purchase of books for the library was one of the duties with which he was especially charged. He was one of the library committee until his retirement. In his own departments, not only the Chemical Laboratory, but also the Cabinet of Minerals owed its existence to his energy. This collection is indeed so important, that something more than the mere mention of it seems due. About the time when Mr. Silliman was appointed a professor, the entire mineralogical and geological collection of Yale College was transported to Philadelphia in one small box, that the specimens might be named by Dr. Adam Seybert, then fresh from Werner's School at Freiberg, the only man in this country who could be regarded as a mineralogist scientifically trained. From this small beginning grew the present cabinet. In 1810, owing to personal regard for Prof. Silliman, Col. George Gibbs deposited with Yale College his valuable collection of minerals, and, after it had remained open to the public fifteen years, various friends of the college, chiefly through the instrumentality of Prof. Silliman, subscribed for its purchase the sum of $20,000. Other important accessions were also secured through his influence, not only from college graduates and other American gentlemen, but from various foreign collectors.

The Clark telescope is another of the donations to Yale College due to Prof. Silliman. This excellent glass, the best in the country at the time of its purchase, was the means of exciting among the students of the college unusual attention to astronomical pursuits for many years after its reception. The liberal donor, a farmer near New Haven, by this and other more important gifts, placed himself foremost among all the benefactors of the college up to that time, and Mr. Silliman was the medium through whom his benefactions were bestowed. The Trumbull Gallery of Paintings, a collection of priceless value, not only as works of art but also as illustrations of American history and biography, was secured to the college through the same enlightened instrumentality.[1] The Medical Institution of Yale College and the Sheffield School of Science, important branches of the University, were both greatly aided in their beginnings by the influential exertions put forth by Professor Silliman. He was one of the chief founders of the Alumni association of the college, and at their anniversaries and on other occasions, he was, as another has said, "the standing 'orator' of the college, the principal medium between those who dwelt in the academic shade and the great public." Not unfrequently he was the college solicitor, asking funds for the expansion of the institution, and never asking in vain.

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[1] It is an interesting fact that as early as 1842, Prof. Silliman, in his Alumni address, pointed out the need of another edifice for the Fine Arts, or an extension of the Trumbull Gallery, at no distant day. This want is about to be supplied by the liberality of an alumnus of the college. Prof. Silliman was prevented by illness, a week before his decease, from taking a public part in the exercises of laying the corner stone of the proposed structure.

Although his services as a college officer were great, Prof. Silliman's strongest claim to the gratitude of men of science rests upon the establishment, and the maintenance, often under very discouraging circumstances, of the "American Journal of Science." The history of this undertaking has already been given, in his own words, in the introduction to the 50th or Index volume of the First series of the Journal; and it is for others, rather than for us, to give an estimate of his editorial services. It is but just, however, to call attention to a few circumstances, which all will regard as creditable to its founder.

He had the sagacity to foresee, as long ago as 1818, the scope which such a magazine should take. The prospectus which he then wrote is applicable almost exactly to our pages to-day. Experience has established the wisdom of the course which he marked out.

He maintained the Journal, from the beginning, at his own pecuniary risk. Its publication has often been a serious financial burden, and in its most prosperous days has not yielded a fair return for editorial labor. But it has been continued, at this personal inconvenience, for the sake of American science, that the labors of our countrymen might be made known abroad, and the labors of Europeans understood in this country.

The Journal has never been used for the benefit of any party or individual, but solely for the advancement and diffusion of scientific truth. Its pages have been always open to free scientific discussion, with truth as the single end in view.

The original investigations of Prof. Silliman are not numerous. In the early part of his career he began with energy some important experiments and researches. He undertook a geological survey of Connecticut; he published a paper in conjunction with Prof. Kingsley on the famous Weston meteorite; he applied the newly invented blowpipe of his friend, Dr. Hare, to the fusion of a variety of bodies, which were before regarded as infusible; he demonstrated in the galvanic battery the transfer of particles of carbon from one charcoal point to the other; he made scientific examinations of various localities interesting in their geological or mineralogical aspects. But he was too much needed elsewhere to be allowed to remain a close student in the laboratory, or to engage with constancy as an explorer in the field of geological research. He has probably been a more useful man in the wider spheres of influence to which he was called than he could have been in a life devoted to scientific investigation.

During a considerable part of his life, he was one of the few men in the country who could hold a popular audience with a lecture on science. The public early knew of his capabilities, and for many years he yielded to invitations from various parts of the country to deliver lectures on Geology and Chemistry. In 1833 he gave his first popular course on Geology at New Haven, which was repeated in 1834 at Hartford and Lowell, and in 1835 at Boston and Salem. At Boston, the audience desiring to attend was so much larger than the largest hall would hold, that each lecture was given twice for the accommodation of the public. From 1840 to 1843 inclusive, he gave four successive courses of the "Lowell Lectures" in Boston. Besides various other engagements in the Northern and Eastern states, he went in 1847 by invitation to New Orleans, and on his way appeared before crowded audiences in other cities of the South; and five years after the resignation of his professorship in college, when he had passed his 75th year, he made the long journey to St. Louis, in obedience to a call for a course of lectures from the citizens of that place.

In lecturing, his language was simple—his flow of words easy, generous and appropriate—his style animated, abounding in life-like and well-adorned description, often eloquent, and sometimes varied with anecdote running occasionally into wide digressions. His manner was natural, and every feature spoke as well as his mouth; his noble countenance and commanding figure (he was nearly six feet in height, with a well-built frame) often called forth, as he entered the lecture hall, the involuntary applause of his audience.

In his popular courses he often lectured on the subject of "Geology and Genesis," and as he was widely known not only as a man of science, but as a sincere believer in the sacred Scriptures, he greatly aided in removing from the religious world the apprehension that science and religion were hostile in their teachings.

Mr. Silliman found great pleasure in helping forward other men of science. He rejoiced heartily in their progress; his house and his laboratory were always open to receive them, and if a friendly word or letter from him could advance their interests, he was ever ready to bestow it. He also felt a deep concern for the advancement of scientific investigations in every part of the country, and whenever, in halls of legislation, or before the public, the name of Benjamin Silliman would advance a useful project, it was not withheld. In more than one instance, the foreigner, or the exile, remembers his kindness with almost filial devotion.

Prof. Silliman's scientific publications, apart from his contributions to this journal, were chiefly text-books. He edited

Henry's Chemistry and Bakewell's Geology, for the use of his pupils, and also published a work on Chemistry, in two volumes.

His long labors for science brought him honors from all parts of the world. His name is on the roll of several of the principal scientific Academies or Societies of Europe, and of those of his own country. He was one of the original members of the National Academy of Sciences, and a Regent of the Smithsonian Institution.

Aside from Professor Silliman's influence as an officer of Yale College, and as a well known man of science, his personal hold upon the community at large was remarkably strong. This was due somewhat to the favor with which his popular lectures were received, and to the wide circuit over which he had journeyed. It was also owing in part to the pleasure and instruction which were afforded by his books of travel. Twice, as we have stated, Professor Silliman visited Europe, the interval between his journeys being nearly fifty years. Both these visits led to the publication of his observations in volumes which were widely read. The narrative of his earlier journey especially was received by the public with great delight. Few Americans then went abroad; and hardly any had published narratives of what they had seen. Mr. Silliman's volumes were fascinating to young and old,—and many were the testimonials which he received of the interest thus awakened in European institutions and manners. His Journal of a Tour to Canada was another contribution to the literature of the day.

But the general influence of Mr. Silliman must be attributed to his personal character rather than to any of what may be termed the accidental circumstances of his life. He was a man of vigorous understanding and sound judgment, led on, but never carried away, by an enthusiastic disposition, glowing and constant. With this was associated sterling integrity, which never harbored a selfish or dishonorable purpose, but rejoiced in doing and encouraging whatever was right. Every one could trust him. These fundamental traits were adorned by the outward qualities of affability and courtesy, or rather were expressed in manners at once so dignified and so kind that all with whom he came in contact were charmed at once, and on closer intercourse were bound to him as friends for life. Such friendships he never neglected or forgot. Even the sons and the grandsons of his early associates inherited a share in the regard which he had bestowed upon their parents. Blending with and ennobling all these virtues, was the child-like simplicity of his Christian faith.

A character like this shines the brighter the nearer it is seen. In his own family circle, Mr. Silliman has moved for years as a patriarch, surrounded by his descendants to the third and fourth generation. The very house which he occupied has become historic, reflecting in its arrangements, its family portraits, its interesting mementos of absent friends, and its long shelves of books, the controlling mind which has dwelt there.

In the neighborhood and town where he resided, Mr. Silliman was peculiarly beloved and respected. "New Haven will not be New Haven without him," said more than one of his associates, as he heard of his death. His hand was always open to the needy. He was given to hospitality. He frequently took part in public meetings, and was actively concerned in all questions of local improvement. He rarely, if ever, failed to discharge his duties as a citizen at the polls, and was always ready to express his opinions on questions of public policy.

A whole-souled patriot, he viewed with the deepest interest the complications brought into the affairs of the country by the system of slavery. His general benevolence ever led him to sympathize with the oppressed, and the wrongs of the African touched him deeply. We cannot better indicate his feelings on this subject than by quoting a few sentences from his private journal under the date of April, 1850. After mentioning the death of the champion of what have been called "Southern rights," John C. Calhoun, his former pupil and friend, he gives a brief sketch of his character, concluding as follows:

> "His public career has been highly distinguished. It is, however, very much to be regretted that he, many years ago, narrowed down his great mind to sectional views, and that he became morbidly sensitive and jealous of encroachment as regards the South, especially in reference to the protective tariff and to slavery. The former prompted his efforts for nullification, and the latter excited him to a vindication of slavery in the abstract. He, in a great measure, changed the state of opinion and the manner of speaking and writing upon this subject in the South, until we have come to present to the world the mortifying and disgraceful spectacle of a great republic—and the only real republic in the world—standing forth in vindication of slavery, without prospect of, or a wish for, its extinction. If the views of Mr. Calhoun, and of those who think with him, are to prevail, slavery is to be sustained on this great continent forever. I will not occupy my pages with any extended remarks upon this subject which is now agitating the national councils, and to a degree the nation itself. * * * It [the great question] is in better hands than man's; and I trust ultimately the colored men of all races on this continent will be received into the great human family as rational beings and as heirs of immortality."

As soon as the atrocities in Kansas revealed the determination of the advocates of slavery to perpetuate and extend that institution, even if they dissevered or destroyed the nation, Mr. Silliman came out with all his youthful ardor, and with the influence of his years and reputation, as the opponent of the slave-power. He thus became the object of personal defamation, even in the Senate chamber at Washington; but he still remained firm, for he recognized in this war a slaveholder's rebellion. All the lofty sentiments of patriotism which were awakened in childhood, as he witnessed the commencement of national life, were intensified by this struggle to maintain the Union. He was sure that the nation would be purified by the conflict, and liberty established through all the land.

Mr. Silliman has always been remarkable for uniform good health, and in his later years manifested but slightly the encroachments of age. To the last, his form was as erect, his brow as serene, and his features as full of life and cheerfulness as in his earlier days; and his gait was only a little slower and more cautious.

He continued as usual until the middle of November just past, when he was for a few days quite unwell, probably as an immediate consequence of exposure to cold in attending an evening meeting in behalf of the Sanitary Commission. He had gradually, to appearance, regained nearly his former strength during the following week, and, on Wednesday, was intending to join the family Thanksgiving festival the next day at the house of his son-in-law, Prof. Dana. On the morning of that day, Nov. 24th, he awoke early, after a night of quiet rest, feeling stronger, as he said, than he had done for some days. He spoke with his wife of the many reasons there were for thankfulness, both public and private, dwelling at length upon the causes for national gratitude, especially in the recent re-election to the Presidency of a man who had proved himself so true, so honest, so upright in conducting the affairs of the government as Mr. Lincoln. As was his custom, while still in his bed, he offered up a short prayer, and repeated a familiar hymn of praise. In resuming his conversation, before rising, he spoke of the possibility of his attending the public services of the day, of the happiness of his home, of the love of his children, and, in strong terms of endearment, of his wife. Just as these his last words of love were uttered, there was a sudden change of countenance, a slightly heavier breath and he was gone. At the advanced age of 85, life to him was still beautiful; and not less so was its close. His sun set in the blessedness of the Christian's faith, to rise on a brighter morrow.

ART. II.—*Notice of the Explorations of the Geological Survey of California, in the Sierra Nevada, during the summer of* 1864.

IN the September number of this Journal (vol. xxxviii, page 298) a notice was inserted giving a sketch of the explorations of the Geological Survey of California in the High Sierra about the heads of King's and Kern rivers (latitude 36° 30′ to 37°). It was mentioned that Mr. King had, on the 6th of July, ascended a peak over 14,000 feet high, and that he was about to make an attempt to reach a point still higher, which was one of two in sight from, and higher than, the peak he had just been on. Since the last number of the Journal was issued, some further particulars have been received with regard to the peak ascended on the 6th of July, to which the name of Mt. Tyndall was given, and also a brief account of the attempt made to get on to the highest points.

The ascent of Mt. Tyndall was a very difficult and dangerous one; but the summit was reached without accident either to Mr. King or his companion, although the trip required that several nights should be spent at an elevation of over 12,000 feet, of course without fire, and with but scanty covering. From the summit of Mt. Tyndall, which is considerably over 14,000 feet high, there appeared two other peaks of equal elevation and two still higher, all within a distance of seven miles of this. Mr. King says, "of the two highest, one rose close by, being hardly a mile away; it is an inaccessible bunch of needles. The other was equally inaccessible from any point on the north or west side. The first-mentioned was about 150 feet above us, the other was six or seven miles distant and I should think fully 350 feet higher than the peak we were on. Within our field of view were five mountains over 14,000 feet and about fifty over 13,000. The five highest peaks are all in the eastern ridge. Owen's valley, a brown sage plain, lies 10,000 feet below on the one side, and Kern cañon, once the rocky bed of a grand old glacier, 4000 feet down on the other. About fifteen miles north of here, King's river cuts through the western ridge and turns at a right angle toward the plain. North of this point, again, the two great ridges unite in a grand pile of granite mountains, whose outlines are all of the most rugged and fantastic character. Twenty-five miles south, the high group ends, there (certainly for a breadth of sixty miles) forming one broad, rolling, forest-covered plateau, 8000 to 9000 feet in elevation."

"From Mt. Brewer to Kaweah Peak, the two culminating points of the western ridge, for a distance of fifteen miles, there is nothing that can be called a separate mountain: it is, rather, a great mural ridge, capped by small sharp cones and low ragged domes, all covered with little minarets. At one place the ridge forms a level table; upon this lies an unbroken cover of snow. To the eastward, all this range, from King's river gateway to Kaweah Peak, presents a series of blank, almost perpendicular precipices, broken every mile or so by a bold granite buttress. Between these are vast snow fields, and also numberless deep lakes, of which the most elevated are frozen."

"The few *Pinus contorta*, and the groves of our new pine[1] have a peculiar black color, or, rather, dark bluish-green, which rather augments than relieves the desolate, naked aspect of things." "The only bits of bright color to break the solemn monotony of granite and snow are the blue lakes, which lie everywhere in the ancient glacier beds."

"Far away in the north there is a broad red band in the granite; other than that, all is gray. Beyond Owen's valley is a low desert range. The Coso and Inyo mountains are in plain sight."

"To the eastward, parallel ridges, one beyond another, lie stretched before you, rigid and stony. They have the same aspect as the mountains near Washoe, the same brown color, with red and yellow shadings."

"Nearly due north, fifty or sixty miles distant, rises a lofty group of mountains, which culminates in a white cone. This must be very high, for unbroken snow covers fully two thousand feet on the south side of the peak, while even the highest mountains in our group have no snow except on the north flank. These are probably the 'White Mountains.' I venture 14,600 feet as a guess at the probable elevation of the highest point in the group."

The White mountains, of which Mr. King speaks, in the extract cited above, lie just on the borders of California and Nevada, in about lon. 118°, lat. 37° 30′; they were distinctly visible to us from Mt. Dana and the other high peaks near Mono lake. It is doubtful whether the highest points are within the State of California; but they are probably very near the line on one side or the other. As it is by no means impossible, although I do not consider it probable, that some points of this range of mountains are higher than any yet measured or ascended by our parties, we still have to remain for some time in uncertainty as to whether California can claim the highest elevations in the country as within her borders; we can, at least, say that the highest measured ones are.

The ascent of Mt. Tyndall was a successful one, the party returning to camp at the end of the fifth day, with bones and barometers unbroken, although several very narrow escapes are briefly mentioned in Mr. King's notes.

It appears from Mr. Brewer's notes, that Mr. King's barometrical measurements of Mt. Tyndall would make that mountain above the height of Mt. Shasta, or over 15,090 feet, Mt. Shasta being 14,440. The height of Visalia, which is the plane of reference for all our observations this year in the High Sierra, has not yet been as accurately determined as it will be after a longer series of observations has been worked up, this place having been, during the past summer, and still continuing to be, one of Major Williamson's stations.

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[1] A new and not yet named species discovered by the Survey during this trip.

Not satisfied with the former attempt to reach the highest peak of the Sierra, Mr. King made another, starting from Visalia, July 14th, with the intention of following the Owen's lake and Visalia trail, and endeavoring to reach the culminating point from somewhere on this route. This trail leads up the Kaweah river, following up the South fork, from its junction with the middle fork of the same river. Full particulars of this reconnoissance have not yet come to hand; but from letters lately received, it appears that the base of the high point toward which his efforts were directed was, with difficulty, reached; but that the actual summit of the peak was found to be inaccessible—at least from any direction except that of Owen's valley, and probably not to be scaled from that side. The highest point attained by Mr. King on this trip was 14,369 feet above Visalia, which place is approximately 360 feet above the sea level, making the total height 14,729 feet, which is considerably above Mt. Shasta. But Mr. King was not, by estimate, within 300 or 400 feet of the summit, so that it appears that the elevation above the sea-level of this, so far as known, the culminating point of the Sierra Nevada cannot fall short of 15,000 feet.[1]

In the mean time, the main party, under Professor Brewer, made an attempt to work up the topography of this portion of the Sierra north to the Merced river, by keeping along at a high altitude, on the west side of the main ridge; but, after many perilous and laborious attempts, they found the ridges between the north and south forks of King's river to be impassable, so that they were obliged to cross the summit of the Sierra and descend into Owen's valley, in order to get north in that valley, and by crossing back on some other pass to reach the region of the head-waters of the San Joaquin. The pass by which the Sierra was crossed, going into Owen's valley, was about 11,600 feet high, all the region about the summit being exceedingly rough, sharp peaks of granite rising 2,000 feet or more above the pass on both sides. The party travelled for three days up Owen's valley, then turned and crossed the Sierra, by an old Indian foot-trail, to the head-waters of the San Joaquin, the summit of the pass being just about 12,000 feet high, and the mountains on each side 1,000 to 1,200 feet higher

From here the party made their way to Clark's ranch, between the Mariposa estate and the Yo-semite, through many privations and some perils, but without serious accident to life or limb.

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[1] To this peak the name of Mount Whitney was given by Messrs. Brewer, King and Hoffmann.

The result of the summer's reconnoissance has been that a general idea is secured of the topography of a region about as large as Massachusetts, lying wholly within the State of California, and of which nothing whatever was known previously to this. That mountain peaks should be found in this part of the State, higher than any known to exist in the United States, is a discovery equally interesting and unexpected. The details of this exploration will furnish many facts of great geological and geographical interest. J. D. W.

Northampton, Mass., Oct. 15, 1864.

ART. III.—*On the Mineral Waters of Bath and other hot springs, and their Geological effects;* by Sir CHARLES LYELL, Bart.[1]

* * * * What renders Bath a peculiar point of attraction to the student of natural phenomena is its thermal and mineral waters, to the sanatory powers of which the city has owed its origin and celebrity. The great volume and high temperature of these waters render them not only unique in our island, but perhaps without a parallel in the rest of Europe, when we duly take into account their distance from the nearest region of violent earthquakes or of active or extinct volcanos. The spot where they issue, as we learn from the researches of the historian and antiquary, was lonely and desert when the Romans first landed in this island, but in a few years it was converted into one of the chief cities of the newly conquered province. On the site of the hot springs was a large morass, from which clouds of white vapor rose into the air; and there first was the spacious bath-room built, in a highly ornamental style of architecture, and decorated with columns, pilasters, and tessellated pavements. By its side was erected a splendid temple dedicated to Minerva, of which some statues and altars, with their inscriptions, and ornate pillars are still to be seen in the Museum of this place. To these edifices the quarters of the garrison, and in the course of time the dwellings of new settlers, were added; and they were all encircled by a massive wall, the solid foundations of which still remain.

A dense mass of soil and rubbish, from 10 to 20 feet thick, now separates the level on which the present city stands from the level of the ancient Aquæ Solis of the Romans. Digging through this mass of heterogeneous materials, coins and coffins of the Saxon period have been found; and lower down, beginning at the depth of from 12 to 15 feet from the surface, coins have been disinterred of Imperial Rome, bearing dates from the reign of Claudius to that of Maximus in the fifth century. Beneath the whole, are occasionally seen tesselated pavements still retaining their bright colors, one of which, on the site of the Mineral-water Hospital, is still carefully preserved, affording us an opportunity of gauging the difference of level of ancient and modern Bath.

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[1] From the inaugural address at the opening of the meeting of the British Association at Bath, Sept. 14, 1864.

One of our former Presidents, Dr. Daubeny, has remarked that nearly all the most celebrated hot springs of Europe, such as those of Aix-la-Chapelle, Baden-Baden, Naples, Auvergne, and the Pyrenees, have not declined in temperature since the days of the Romans; for many of them still retain as great a heat as is tolerable to the human body, and yet when employed by the ancients they do not seem to have required to be first cooled down by artificial means. This uniformity of temperature, maintained in some places for more than 2000 years, together with the constancy in the volume of the water, which never varies with the seasons, as in ordinary springs, the identity also of the mineral ingredients which, century after century, are held by each spring in solution, are striking facts, and they tempt us irresistibly to speculate on the deep subterranean sources both of the heat and mineral matter. How long has this uniformity prevailed? Are the springs really ancient in reference to the earth's history, or, like the courses of the present rivers and the actual shape of our hills and valleys, are they only of high antiquity when contrasted with the brief space of human annals? May they not be like Vesuvius and Etna, which, although they have been adding to their flanks in the course of the last 2000 years many a stream of lava and shower of ashes, were still mountains very much the same as they now are in height and dimensions from the earliest times to which we can trace back their existence? Yet although their foundations are tens of thousands of years old, they were laid at an era when the Mediterranean was already inhabited by the same species of marine shells as those with which it is now peopled; so that these volcanos must be regarded as things of yesterday in the geological calendar.

Notwithstanding the general persistency in character of mineral waters and hot springs ever since they were first known to us, we find on inquiry that some few of them, even in historical times, have been subject to great changes. These have happened during earthquakes which have been violent enough to disturb the subterranean drainage and alter the shape of the fissures up which the waters ascend. Thus, during the great earthquake at Lisbon in 1755, the temperature of the spring called La Source de la Reine at Bagnères de Luchon, in the Pyrenees, was suddenly raised as much as 75° F., or changed from a cold spring to one of 122° F., a heat which it has since retained. It is also recorded that the hot springs at Bagnères de Bigorre, in the same mountain-chain, became suddenly cold during a great earthquake which, in 1660, threw down several houses in that town.

It has been ascertained that the hot springs of the Pyrenees, the Alps, and many other regions, are situated in lines along which the rocks have been rent, and usually where they have been displaced or "faulted." Similar dislocations in the solid crust of the earth are generally supposed to have determined the spots where active and extinct volcanos have burst forth; for several of these often affect a linear arrangement, their position seeming to have been determined by great lines of fissure. Another connecting link between the volcano and the hot spring is recognizable in the great abundance of hot springs in regions where volcanic eruptions still occur from time to time. It is also in the same districts that the waters occasionally attain the boiling-temperature, while some of the associated stufas emit steam considerably above the boiling-point. But in proportion as we recede from the great centres of igneous activity, we find the thermal waters decreasing in frequency and in their average heat, while at the same time they are most conspicuous in those territories where, as in Central France or the Eifel in Germany, there are cones and craters still so perfect in their form, and streams of lava bearing such a relation to the depth and shape of the existing valleys, as to indicate that the internal fires have become dormant in comparatively recent times. If there be exceptions to this rule, it is where hot springs are met with in parts of the Alps and Pyrenees which have been violently convulsed by modern earthquakes.

To pursue still further our comparison between the hot spring and the volcano, we may regard the water of the spring as representing those vast clouds of aqueous vapor which are copiously evolved for days, sometimes for weeks, in succession from craters during an eruption. But we shall perhaps be asked whether, when we contrast the work done by the two agents in question, there is not a marked failure of analogy in one respect —namely a want, in the case of the hot spring, of power to raise from great depths in the earth voluminous masses of solid matter corresponding to the heaps of scoriæ and streams of lava which the volcano pours out on the surface. To one who urges such an objection it may be said that the quantity of solid as well as gaseous matter, transferred by springs from the interior of the earth to its surface, is far more considerable than is commonly imagined. The thermal waters of Bath are far from being conspicuous among European hot springs, for the quantity of mineral matter contained in them in proportion to the water which acts as a solvent; yet Professor Ramsay has calculated that if the sulphates of lime and soda, and the chlorids of sodium and magnesium, and the other mineral ingredients which they contain, were solidified, they would form in one year a square column 9 feet in diameter, and no less than 140 feet in height. All this matter is now quietly conveyed by a stream of limpid water, in an invisible form, to the Avon, and by the Avon to the sea; but if, instead of being thus removed, it were deposited around the orifice of eruption, like the siliceous layers which encrust the circular basin of an Icelandic geyser, we should soon see a considerable cone built up, with a crater in the middle; and if the action of the spring were intermittent, so that ten or twenty years should elapse between the periods when solid matter was emitted, or (say) an interval of three centuries, as in the case of Vesuvius between 1306 and 1631, the discharge would be on so grand a scale as to afford no mean object of comparison with the intermittent outpourings of a volcano.

Dr. Daubeny, after devoting a month to the analysis of the Bath waters, in 1833, ascertained that the daily evolution of nitrogen gas amounted to no less than 250 cubic feet in volume. This gas, he remarks, is not only characteristic of hot springs, but is largely disengaged from volcanic craters during eruptions. In both cases, he suggests that the nitrogen may be derived from atmospheric air, which is always dissolved in rain-water, and which, when this water penetrates the earth's crust, must be carried down to great depths, so as to reach the heated interior. When there, it may be subjected to deoxydating processes, so that the nitrogen, being left in a free state, may be driven upward by the expansive force of heat and steam, or by hydrostatic pressure. This theory has been very generally adopted, as best accounting for the constant disengagement of large bodies of nitrogen, even where the rocks through which the spring rises are crystalline and unfossiliferous. It will, however, of course be admitted, as Professor Bischof has pointed out, that in some places organic matter has supplied a large part of the nitrogen evolved.

Carbonic acid gas is another of the volatilized substances discharged by the Bath waters. Dr. Gustav Bischof, in the new edition of his valuable work on chemical and physical geology, when speaking of the exhalations of this gas, remarks that they are of universal occurrence, and that they originate at great depths, becoming more abundant the deeper we penetrate. He also observes that, when the silicates which enter so largely into the composition of the oldest rocks are percolated by this gas, they must be continually decomposed, and the carbonates formed by the new combinations thence arising must often augment the volume of the altered rocks. This increase of bulk, he says, must sometimes give rise to a mechanical force of expansion capable of uplifting the incumbent crust of the earth; and the same force may act laterally, so as to compress, dislocate, and tilt the strata on each side of a mass in which the new chemical changes are developed. The calculations made by this eminent

German chemist of the exact amount of distention which the origin of new mineral products may cause, by adding to the volume of the rocks, deserve the attention of geologists, as affording them aid in explaining those reiterated oscillations of level—those risings and sinkings of land—which have occurred on so grand a scale at successive periods of the past. There are probably many distinct causes of such upward, downward, and lateral movements, and any new suggestion on this head is most welcome; but I believe the expansion and contraction of solid rocks, when they are alternately heated and cooled, and the fusion and subsequent consolidation of mineral masses, will continue to rank, as heretofore, as the most influential causes of such movements.

The temperature of the Bath waters varies in the different springs from 117° to 120° F. This, as before stated, is exceptionally high, when we duly allow for the great distance of Bath from the nearest region of active, or recently extinct, volcanos and of violent earthquakes. The hot springs of Aix-la-Chapelle have a much higher temperature, viz. 135° F., but they are situated within forty miles of those cones and lava-streams of the Eifel which, though they may have spent their force ages before the earliest records of history, belong, nevertheless, to the most modern geological period. Bath is about 400 miles distant from the same part of Germany, and 440 from Auvergne—another volcanic region, the latest eruptions of which were geologically coëval with those of the Eifel. When these two regions in France and Germany were the theaters of frequent convulsions, we may well suppose that England was often more rudely shaken than now; and such shocks as that of October last, the sound and rocking motion of which caused so great a sensation as it traversed the southern part of the island, and seems to have been particularly violent in Herefordshire, may be only a languid reminder to us of a force of which the energy has been gradually dying out.

If you consult the geological map of the environs of this city, colored by the Government surveyors, you will perceive that numerous lines of fault or displacement of the rocks are there laid down, and one of these has shifted the strata vertically as much as 200 feet. Mr. Charles Moore pointed out to me last spring, when I had the advantage of examining the geology of this district under his guidance, that there are other lines of displacement not yet laid down on the Ordnance Map, the existence of which must be inferred from the different levels at which the same formations crop out on the flanks of the hills to the north and south of the city. I have therefore little doubt that the Bath springs, like most other thermal waters, mark the site of some great convulsion and fracture which took place in the crust of the earth at some former period—perhaps not a very remote one, geologically speaking. The uppermost part of the rent through which the hot water rises is situated in horizontal strata of Lias, and Trias, 300 feet thick; and this may be more modern than the lower part, which passes through the inclined and broken strata of the subjacent Coal-measures, which are unconformable to the Trias. The nature and succession of these rocks penetrated by the Bath waters was first made out by the late William Smith, in 1817, when a shaft was sunk in the vicinity in search for coal. The shock which opened a communication through the upper rocks may have been of a much later date than that which fractured the older and underlying strata; for there is a tendency in the earth's crust to yield most readily along lines of ancient fracture, which constitute the points of least resistance to a force acting from below.

If we adopt the theory already alluded to, that the nitrogen is derived from the deoxydation of atmospheric air carried down by rain-water, we may imagine the supply of this water to be furnished by some mountainous region, perhaps a distant one, and that it descends through rents or porous rocks till it encounters some mass of heated matter by which it is converted into steam and then driven upward through a fissure. In its downward passage, the water may derive its sulphate of lime, chlorid of calcium, and other substances from the decomposition of the gypseous, saline, calcareous and other constituents of the rocks which it permeates. The greater part of the ingredients are common to sea-water, and might suggest the theory of a marine origin; but the analysis of the Bath springs by Merck and Galloway shows that the relative proportion of the solid matter is far from agreeing with that of the sea, the chlorid of magnesium being absolutely in excess, that is, 14 grains of it per gallon for 12 of common salt; whereas in sea-water there are 27 grains of salt, or chlorid of sodium, to 4 of the chlorid of magnesium. That some mineral springs, however, may derive an inexhaustible supply through rents and porous rocks, from the leaky bed of the ocean, is by no means an unreasonable theory, especially if we believe that the contiguity of nearly all the active volcanos to the sea is connected with the access of salt water to the subterranean foci of volcanic heat.

Professor Roscoe, of Manchester, has been lately engaged in making a careful analysis of the Bath waters, and has discovered in them three metals which they were not previously known to contain—namely, copper, strontium, and lithium; but he has searched in vain for cæsium and rubidium, those new metals, the existence of which has been revealed to us in the course of the last few years by what is called spectrum analysis. By this new method, the presence of infinitesimal quantities, such as would have wholly escaped detection by ordinary tests, are made known to the eye by the agency of light. Thus, for example, a solid substance such as the residue obtained by evaporation from a mineral water is introduced on a platinum wire into a colorless gas-flame. The substance thus volatilized imparts its color to the flame, and the light, being then made to pass through a prism, is viewed through a small telescope or spectroscope, as it is called, by the aid of which one or more bright lines or bands are seen in the spectrum, which, according to their position and color, indicate the presence of different elementary bodies.

Professor Bunsen, of Heidelberg, led the way, in 1860, in the application of this new test to the hot waters of Baden-Baden and of Dürkheim in the Palatinate. He observed in the spectrum some colored lines of which he could not interpret the meaning, and was determined not to rest till he found out what they meant. This was no easy task, for it was necessary to evaporate fifty tons of water to obtain 200 grains of what proved to be two new metals. Taken together, their proportion to the water was only as one to three million. He named the first cæsium, from the bluish-gray lines which it presented in the spectrum; and the second rubidium, from its two red lines. Since these successful experiments were made, thallium, so called from its green line, was discovered in 1861 by Mr. Crooks; and a fourth metal, named indium, from its indigo colored band, was detected by Professor Richter, of Freiberg, in Saxony, in a zinc ore of the Hartz. It is impossible not to suspect that the wonderful efficacy of some mineral springs, both cold and thermal, in curing diseases, which no artificially prepared waters have as yet been able to rival, may be connected with the presence of one or more of these elementary bodies previously unknown; and some of the newly found ingredients, when procured in larger quantities, may furnish medical science with means of combating diseases which have hitherto baffled all human skill.

While I was pursuing my inquiries respecting the Bath waters, I learned casually that a hot spring had been discovered at a great depth in the copper mine near Redruth, in Cornwall, having about as high a temperature as that of the Bath waters, and of which, strange to say, no account has yet been published. It seems that, in the year 1839, a level was driven from an old shaft so as to intersect a rich copper-mine at the depth of 1350 feet from the surface. This lode or metalliferous fissure occurred in what was formerly called the United Mines, and which have since been named the Clifford Amalgamated Mines. Through the contents of the lode a powerful spring of hot water was observed to rise, which has continued to flow with undiminished strength ever since. At my request, Mr. Horton Davey, of Redruth, had the kindness to send up to London many gallons of this water, which have been analyzed by Professor William Allen Miller, F.R.S., who finds that the quantity of solid matter is so great as to exceed by more than four times the proportion of that yielded by the Bath waters.[1] Its composition is also in many respects very different; for it contains but little sulphate of lime, and is almost free from the salts of magnesium. It is rich in the chlorids of calcium and sodium, and it contains one of the new metals—cæsium, never before detected in any mineral spring in England; but its peculiar characteristic is the extraordinary abundance of lithium, of which a mere trace had been found by Professor Roscoe in the Bath waters; whereas in this Cornish hot spring this metal constitutes no less than a twenty-sixth part of the whole of the solid contents, which, as before stated, are so voluminous. When Professor Miller exposed some of these contents to the test of spectrum analysis, he gave me an opportunity of seeing the beautiful bright crimson lines which the lithium produces in the spectrum.

Lithium was first made known in 1817 by Arfvedson, who extracted it from petalite; and it was believed to be extremely rare, until Bunsen and Kirchhoff, in 1860, by means of spectrum analysis, showed that it was a most widely diffused substance, existing in minute quantities in almost all mineral waters, and in the sea, as well as in milk, human blood, and the ashes of some plants. It has already been used in medicine, and we may therefore hope that, now that it is obtainable in large quantities, and at a much cheaper rate than before the Wheal-Clifford hot spring was analyzed, it may become of high value. According to a rough estimate which has been sent to me by Mr. Davey, the Wheal-Clifford spring yields no less than 250 gallons per minute, which is almost equal to the discharge of the King's Bath or chief spring of this city. As to the gases emitted, they are the same as those of the Bath water—namely carbonic acid, oxygen, and nitrogen.

Mr. Warington Smyth, who had already visited the Wheal-Clifford lode in 1855, re-examined it in July last, chiefly with the view of replying to several queries which I had put to him; and, in spite of the stifling heat, ascertained the geological structure of the lode, and the exact temperature of the water. This last he found to be 122° F. at the depth of 1350 feet; but he scarcely doubts that the thermometer would stand two or three degrees higher at a distance of 200 feet to the eastward, where the water is known to gush up more freely. The Wheal-Clifford lode is a fissure varying in width from six to twelve feet, one wall consisting of elvan or porphyritic granite, and the other of killas or clay-slate. Along the line of the rent, which runs east and west, there has been a slight throw or shift of the rocks. The vein-stuff is chiefly formed of cellular pyrites of copper and iron, the porous nature of which allows the hot water to percolate freely through it. It seems, however, that in the continuation upward of the same fissure, little or no metalliferous ore was deposited, but in its place, quartz and other impermeable substances, which obstructed the course of the hot spring, so as to prevent its flowing out on the surface of the country. It has been always a favorite theory of the miners that the high temperature of this Cornish spring is due to the oxydation of the sulphurets of copper and iron, which are decomposed when air is admitted. That such oxydation must have some slight effect is undeniable; but that it materially influences the temperature of so large a body of water is out of the question. Its effect must be almost insensible; for Professor Miller has scarcely been able to detect any sulphuric acid in the water, and a minute trace only of iron and copper in solution.

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[1] See for the analysis, the last volume of this Journal, p. 447.

When we compare the temperature of the Bath springs, which issue at a level of less than 100 feet above the sea, with the Wheal-Clifford spring found at a depth of 1350 feet from the surface, we must of course make allowance for the increase of heat always experienced when we descend into the interior of the earth. The difference would amount to about 20° F., if we adopt the estimate deduced by Mr. Hopkins from an accurate series of observations made in the Monkwearmouth shaft, near Durham, and in the Dukinfield shaft near Manchester, each of them 2000 feet in depth. In these shafts, the temperature was found to rise at the rate of only 1° F. for every increase of depth of from 65 to 70 feet. But if the Wheal-Clifford spring, instead of being arrested in its upward course, had continued to rise freely through porous and loose materials so as to reach the surface, it would probably not have lost anything approaching to 20° F., since the renewed heat derived from below would have warmed the walls and contents of the lode, so as to raise their temperature above that which would naturally belong to the rocks at corresponding levels on each side of the lode. The almost entire absence of magnesium raises an obvious objection to the hypothesis of this spring deriving its waters from the sea; or if such a source be suggested for the salt and other marine products, we shall be under the necessity of supposing the magnesium to be left behind in combination with some of the elements of the decomposed and altered rocks through which the thermal waters may have passed.

Hot springs are, for the most part, charged with alkaline and other highly soluble substances, and, as a rule, are barren of the precious metals, gold, silver, and platinum, as well as of tin, copper, lead, and many others, a slight trace of copper in the Bath waters being exceptional. Nevertheless, there is a strong presumption that there exists some relationship between the action of thermal waters and the filling of rents with metallic ores. The component elements of these ores may, in the first instance, rise from great depths in a state of sublimation or of solution in intensely heated water, and may then be precipitated on the walls of a fissure as soon as the ascending vapors or fluids begin to part with some of their heat. Almost everything, save the alkaline metals, silica, and certain gases, may thus be left behind long before the spring reaches the earth's surface. If this theory be adopted, it will follow that the metalliferous portion of a fissure, originally thousands of feet or fathoms deep, will never be exposed in regions accessible to the miner until it has been upheaved by a long series of convulsions, and until the higher parts of the same rent, together with its contents and the rocks which it had traversed, have been removed by aqueous denudation. Ages before such changes are accomplished, thermal and mineral springs will have ceased to act; so that the want of identity between the mineral ingredients of hot springs and the contents of metalliferous veins, instead of militating against their intimate relationship, is in favor of their being the complementary results of one and the same natural operation.

But there are other characters in the structure of the earth's crust more mysterious in their nature than the phenomena of metalliferous veins, on which the study of hot springs has thrown light: I allude to the metamorphism of sedimentary rocks. Strata of various ages, many of them once full of organic remains, have been rendered partially or wholly crystalline. It is admitted on all hands that heat has been instrumental in bringing about this re-arrangement of particles, which, when the metamorphism has been carried out to its fullest extent, obliterates all trace of the imbedded fossils. But as mountain-masses many miles in length and breadth, and several thousands of feet in height, have undergone such alteration, it has always been difficult to explain in what manner an amount of heat capable of so entirely changing the molecular condition of sedimentary masses could have come into play without utterly annihilating every sign of stratification, as well as of organic structure.

Various experiments have led to the conclusion that the minerals which enter most largely into the composition of the metamorphic rocks have not been formed by crystallizing from a state of fusion, or in the dry way, but that they have been derived from liquid solutions, or in the wet way—a process requiring a far less intense degree of heat. Thermal springs, charged with carbonic acid and with fluohydric acid (which last is often present in small quantities), are powerful causes of decomposition and chemical reaction in rocks through which they percolate.

If, therefore, large bodies of hot water permeate mountain-masses at great depths, they may in the course of ages superinduce in them a crystalline structure; and in some cases strata in a lower position and of older date may be comparatively unaltered, retaining their fossil remains undefaced, while newer rocks are rendered metamorphic. This may happen where the waters, after passing upward for thousands of feet, meet with some obstruction, as in the case of the Wheal-Clifford spring, causing the same to be laterally diverted so as to percolate the surrounding rocks. The efficacy of such hydro-thermal action has been admirably lilustrated of late years by the experiments and observations of Sénarmont, Daubrée, Delesse, Scheerer, Sorby, Sterry Hunt, and others.

The changes which Daubrée has shown to have been produced by the alkaline waters of Plombières, in the Vosges, are more especially instructive. These thermal waters have a temperature of 160° F., and were conveyed by the Romans to baths through long conduits or aqueducts. The foundations of some of their works consisted of a bed of concrete, made of lime, fragments of brick and sandstone. Through this and other masonry the hot waters have been percolating for centuries, and have given rise to various zeolites—apophyllite and chabazite besides others; also to calcareous spar, aragonite, and fluor spar, together with siliceous minerals, such as opal,—all found in the interspaces of the bricks and mortar, or constituting part of their rearranged materials. The quantity of heat brought into action in this instance in the course of 2000 years has, no doubt, been enormous, although the intensity of it developed at any one moment has been always inconsiderable.

The study, of late years, of the constituent parts of granite has in like manner led to the conclusion that their consolidation has taken place at temperatures far below those formerly supposed to be indispensable. Gustav Rose has pointed out that the quartz of granite has the specific gravity of 2·6, which characterizes silica when it is precipitated from a liquid solvent, and not that inferior density, namely 2·3, which belongs to it when it cools and solidifies in the dry way from a state of fusion.

But some geologists, when made aware of the intervention on a large scale, of water, in the formation of the component minerals of the granitic and volcanic rocks, appear of late years to have been too much disposed to dispense with intense heat when accounting for the formation of the crystalline and unstratified rocks. As water in a state of solid combination enters largely into the aluminous and some other minerals, and therefore plays no small part in the composition of the earth's crust, it follows that, when rocks are melted, water must be present, independently of the supplies of rain-water and sea-water which find their way into the regions of subterranean heat. But the existence of water under great pressure affords no argument against our attributing an excessively high temperature to the mass with which it is mixed up. Still less does the point to which the melted matter must be cooled down before it consolidates or crystallizes into lava or granite afford any test of the degree of heat which the same matter must have acquired when it was melted and made to form lakes and seas in the interior of the earth's crust.

We learn from Bunsen's experiments on the Great Geyser in Iceland, that at the depth of only seventy-four feet, at the bottom of the tube, a column of water may be in a state of rest, and yet possess a heat of 120° Centigrade, or 248° F. What, then, may not the temperature of such water be at the depth of a few thousand feet? It might soon attain a white heat under pressure; and as to lava, they who have beheld it issue, as I did in 1858, from the southwestern flanks of Vesuvius, with a surface white and glowing like that of the sun, and who have felt the scorching heat which it radiates, will form a high conception of the intense temperature of the same lava at the bottom of a vertical column several miles high, and communicating with a great reservoir of fused matter, which, if it were to begin at once to cool down, and were never to receive future accessions of heat, might require a whole geological period before it solidified. Of such slow refrigeration, hot springs may be among the most effective instruments, abstracting slowly from the subterranean molten mass that heat which clouds of vapor are seen to carry off in a latent form from a volcanic crater during an eruption, or from a lava-stream during its solidification. It is more than forty years since Mr. Scrope, in his work on volcanos, insisted on the important part which water plays in an eruption, when intimately mixed up with the component materials of lava, aiding as he supposed, in giving mobility to the more solid materials of the fluid mass. But, when advocating this igneo-aqueous theory, he never dreamed of impugning the Huttonian doctrine as to the intensity of heat which the production of the unstratified rocks, those of the plutonic class especially, implies.

The exact nature of the chemical changes which hydrothermal action may effect in the earth's interior will long remain obscure to us, because the regions where they take place are inaccessible to man; but the manner in which volcanos have shifted their position throughout a vast series of geological epochs—becoming extinct in one region and breaking out in another—may, perhaps, explain the increase of heat as we descend toward the interior, without the necessity of our appealing to an original central heat, or the igneous fluidity of the earth's nucleus.

ART. IV.—*On the Nebular Hypothesis;* by DAVID TROWBRIDGE, A.M.

[Concluded from vol. xxxviii, p. 360.]

1. *The Breaking-up of the Rings.*

34. The process of cooling would still continue after the rings had separated. The loss of caloric from radiation would cause the rings to contract their dimensions; and this, the unequal density of different parts, and the extraordinary perturbations to which they would necessarily be subject in their motions, would cause a separation of the rings in certain weak places. When once broken into parts while the density of the ring was very small, even if the parts were not many, the case would be extraordinary in which the parts would be prevented from re-uniting into a single planet, owing to the very great perturbations to which their motions would be subject, as separate bodies. It is impossible at present to tell just how a ring would be resolved into a planet. A system of waves would probably be developed, owing to perturbations; and by an accumulation in one part, owing to the nature of the disturbing force, such as calculation shows might exist under certain conditions in the case of the rings of Saturn, and thus a considerable portion of the matter of the ring would be driven into one side, and this accumulation would be the center of attraction around which the planet would be formed.

35. We may now ask in what direction the planet thus formed would rotate? The direction of rotation would depend on circumstances. Let us take the breadth of the ring from which Uranus was formed, the same as the diameter of his sphere of attraction in Kirkwood's Analogy. The diameter of the sphere of attraction of Uranus, as given by Professor Kirkwood,[28] is 7·438. The inner radius is 2·558, and the outer one, 4·879. If we suppose the inner and the outer parts of the ring each to have the velocity due it according to Kepler's third law, then, the velocity of the inner part being called 1, that of the outer will be 0·8287; and the angular velocities of the same parts will be to each other as 1 to 0·4715. If every part of the ring have the same angular velocity, and that of the inner part be 1, the angular velocity of the outer part will be 1·457. If the inner and the outer parts have the same velocity, the angular velocity of the inner part being called 1, that of the outer part will be 0·6866. But none of these cases can obtain in any of the rings, but one or another will be approximated to, accord-

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[28] This Journal, [2], xiv, p. 213.

ing to circumstances. The outer rings would approximate to the first case, and the inner ones, to the second case; while intermediate rings would approximate more or less, according to their position in the System, to the last case. In confirmation of this, we may refer to the case of Saturn's rings. It is demonstrated by mathematicians that the stability of the motions of those rings cannot exist unless the parts at different distances from the center of Saturn, have different angular velocities.[29]

But observation[30] shows that the stability of the motion of the Rings exists. We hence conclude that those parts of the Rings situated at different distances from the center of Saturn do have different angular velocities. But it is concluded from observation that the rings of Saturn are liquid;[31] we hence conclude that there is still greater necessity for the different parts of a gaseous ring situated at different distances from the center of attraction, to have different angular velocities; just as we have concluded in speaking of the separation of the rings from the equatorial parts of the great solar spheroid.

36. In the case of the first two or three rings abandoned by the primitive spheroid, their density would be relatively so small that the velocity of the outer parts would be regulated approximately by Kepler's third law. Such being the case, it is probable that the velocity of the inner parts will exceed that of the outer parts. When the rings break up to form planets, the parts will in a great measure retain the velocity which they had in the complete rings, and the consequence will be that the rotation of such planets will, at first, be performed in a direction opposite to that of their motions around the central body.[32] In consequence of the great friction of the different strata of the fluid planetary rings abandoned by the solar spheroid, the outer strata would, in most cases, probably, have a greater absolute velocity than the inner strata; and the planets formed from them would naturally at first take the direct motion of rotation, because the external parts having a greater velocity than the inner parts, the excess of velocity would give the planet a direct rotatory velocity. Even if some of the outer planets should prove to have a retro-

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[29] Maxwell, *On the Stability of the Motion of Saturn's Rings*, pp. 3 and 45; Peirce in Gould's *Astronomical Journal*, ii. 17–18.

[30] Observation seems to show that the Rings of Saturn are gradually approaching the body of the planet; but it by no means follows that they will ever come in contact with the body of the planet. They have existed too long for us to suppose there is any probability that the Rings are to be precipitated upon the body of the planet before our eyes, as it were.

[31] See *Annual of Scientific Discovery* for 1852, p. 377.

[32] Professor Kirkwood has pointed this out in his article in this Journal, [2], xxxviii, 2–4; and he has also attempted to show that in certain cases the rotation will become direct from the action of other members of the System. See also, volume xxxvii, page 51, where Prof. Hinrichs has given a formula indicating the same thing that we have arrived at in the text.

grade rotatory motion, our reasoning would not lead us to conclude that such would be the case with many of them.

37. But are we certain that all the rings would break up; or if broken up, that each would form a single planet? In this part of our enquiry we cannot guess at it; nor will it do to be guided by the phenomena of the Solar System; for it is the phenomena of such a system of which we are to render a rational account. We must here be guided by strictly mathematical and philosophical deductions. Prof. Peirce, of Harvard University, in his investigation of the problem of the stability of the motions of Saturn's Rings, arrived at the remarkable conclusion that the dynamical equilibrium of the rings is preserved by the sustaining effect of the satellites in the very act of perturbation. He then makes the remark that the only place in the Solar System, among the primary planets, where we could, from the above conclusions, expect a permanent ring, is just within the powerful masses of Jupiter and Saturn.[33] But even here, he says, the ring could not exist, but must ultimately be destroyed. He says, "But had there been a ring at this part of the system, it must have been subject to such extraordinary perturbations that it would, in the course of time, have been vibrated up against the next inferior planet, *Mars;* and in this way have been broken into the asteroids. The orbits of planets, formed under such circumstances, must have been characterized by great eccentricity."

The above view of the formation of the Asteroids, needs some modification. According to Peirce's conclusions, drawn from his investigation of the problem of Saturn's Rings, a fluid ring might, perhaps, exist for some considerable length of time, within the orbit of Jupiter. Granting this to be true, it is difficult to escape the conclusion that the process of cooling, to which the ring would be subject, even if it were gaseous, would gradually reduce it to a condition approximating to that of the rings of Saturn at the present time; that is, a liquid state. The various conclusions, then, at which mathematicians have arrived, respecting the perturbations of those rings, will apply with greater or less force, according to circumstances, to the supposed ring within the orbit of Jupiter. Prof. Maxwell,[34] in his investigation of the problem of the motion of Saturn's Rings, discussed the case of a solid ring, a ring of disconnected particles, and a fluid (or more definitely a liquid,) ring. The first case he found to be one of unstable equilibrium. In the last two cases he found that if the perturbations to which the ring would be subject could be propagated around the ring in the form of waves, subject to particular conditions, the ring would be permanent; but if these particular conditions were not fulfilled, the ring would be destroyed. In the case of a fluid ring, (incompressible,) he found that in general a wide ring could not revolve every part of which had the same angular velocity.[35] In his recapitulation[36] he says, "We next took up the case of a flattened ring, composed of incompressible fluid, and moving with uniform angular velocity. The internal forces here arise partly from attraction and partly from fluid pressure. We began by taking the case of an infinite stratum of fluid affected by regular waves. I found the accurate values of the forces in this case. For long waves, the resultant force is in the same direction as the displacement, reaching a maximum for waves whose length is about ten times the thickness of the stratum. For waves about five times as long as the stratum is thick, there is no resultant force; and for shorter waves, the force is in the opposite direction to the displacement.

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[33] Gould's Ast. Journal, vol. ii, p. 18. Also Annual of Scientific Discovery for 1852, p. 379.

[34] On the stability of the motion of Saturn's rings. An essay which obtained the Adams Prize for the year 1856, in the University of Cambridge, Eng. By J. Clerk Maxwell, M. A.

39. "Applying these results to the case of the ring, we find that it will be destroyed by the long waves unless the fluid is less than $\frac{1}{12}$d of the density of the planet, and that in all cases the short waves will break up the ring into small satellites.

40. "Passing to the case of *narrow* rings, we should find a somewhat larger maximum density, but we should still find that very short waves produce forces in the direction opposite to the displacement, and that therefore, as already explained, these short undulations would increase in magnitude without being propagated along the ring, till they had broken up the fluid filament into drops. These drops may or may not fulfill the condition formerly given [in the essay] for the stability of a ring of equal satellites. If they fulfill the conditions, they will move as a permanent ring. If they do not, short waves will arise and be propagated among the satellites, with ever increasing magnitude, till a sufficient number of drops have been brought into collision, so as to unite and form a smaller number of larger drops, which may be capable of revolving as a permanent ring."

41. Basing our reasoning on the preceding results, we are led to the conclusion that under certain conditions—such as probably exist within the orbits of Jupiter and Saturn in the Solar System—the abandoned fluid ring may preserve its form for immense ages, and thus have time to cool down somewhat and approximate to the condition of an incompressible fluid, and then, with the changes already taken place, undergo the necessary transformation, to convert it into small, separate bodies. These bodies would continue to come into collision until the conditions of dynamical equilibrium were fully established, and thus en-

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[35] Maxwell, p. 45.

[36] Ib., p. 64.

able each body to move in its own independent orbit. Bodies so formed must vary in size from the largest of them to bodies too minute to be visible through our most powerful telescopes.[37] The original ring would necessarily have considerable width, and since it would be prevented from uniting into a single planet, any such zone of small planets would also be of considerable width, and the planets would range in mean distance from one limit of the broken ring to the opposite limit. The various disturbing causes would very probably cause the small planetary bodies to move in very eccentric (planetary) orbits, and the action of the short waves which would break up the ring, might, perhaps, cause the resulting planets to move in orbits considerably inclined to each other, and to the plane of the ecliptic.

42. The heterogeneousness of the materials composing the outer rings, and the great rarity of their physical condition, would, perhaps, cause some of the less dense portions, at the time of the breaking-up of the rings, owing to the action of the great disturbing forces to which their motions would be subject, to be projected to a considerable distance from either the outer or the inner parts of the rings, and such detached portions might never return to the parent masses, but would move around the central solar body in an elliptical orbit, having, perhaps, in some cases, considerable inclination to the plane of the equator of the rings from which they were projected. Such bodies would revolve around the sun as *Comets.* The theory of Central Forces[38] shows us that, when the distances from the central body and the initial velocity are given, the eccentricity is dependent on the angle of projection; that is, the angle formed by the radius-vector and the line of projection. Supposing comets to have been formed as we have just described, we see that the great eccentricity of their orbits results from the direction with respect to the radius-vector, in which the mass is thrown from the ring. Such comets would very probably move around the sun in the same direction as the planets. Those rings abandoned nearer the centre of the solar spheroid, would, very likely, be more symmetrical in the disposition of their materials, and consequently less likely to have any large collection of rare materials that could be projected from the rings. We hence conclude that those comets which have a comparatively short period of revolution, will not be very large and conspicuous objects when they approach their perihelia. We may further conclude that the last three or four rings would not throw off any comets. We hence conclude that periodic comets having a direct motion should be found in groups, perhaps, with periods of revolution a little greater or a little less than the planets to which they

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[37] May we not in this way account for the existence of meteoric rings?

[37] See Math. Monthly, ii, 160.

nearest correspond in their motions. Retrograde comets, and a portion of those which have a direct motion, cannot very well be accounted for upon this hypothesis of the breaking-up of the rings, nor should we expect to find retrograde comets with short periods. If, as Laplace supposed, and which, according to our hypothesis, is not improbable, comparatively small quantities of nebulous matter exist in different regions of the universe, the Solar System may, in its motion through space, come into their immediate neighborhood at different times, and such nebulous matter would be drawn, by the attractive influence of the sun and planets, into the system, and it would appear in the form of comets—either retrograde or direct, according to circumstances—and meteors. All comets so formed would necessarily have a comparatively long period of revolution, unless (which might be the case in some instances) the perturbative influence of the planets should change their orbits. We may hence conclude that all, or nearly all, comets having a short period, will have a direct motion; and of the comets of long periods, the direct and retrograde should, considering all time, be nearly equally divided, so far as we can determine.

2. *The Planets and their Satellites.*

43. After the rings from which the planets were formed were broken up, and all the parts of any one ring reduced to a planet, such planet, as we have shown, would have a rotation on an axis. This axis would, necessarily, be a *natural* or *principal axis*. It would be a delicate adjustment to give a solid body, differing from a sphere, a rotation around a principal axis; but a fluid body would readily adapt itself to such an axis. We do not doubt but that Infinite Wisdom and Power could cause any solid body to rotate around a natural axis; but we have not the least evidence that Infinite Wisdom ever works in that special manner. God always adapts means to ends, so that all things are produced under the action of fixed natural laws. We, therefore, conclude that the planets, formed as required by the Nebular Hypothesis, should all be found to rotate on *natural axes*.

44. After the parts of the broken-up rings had united, and the resulting bodies commenced their rotatory motion, the cooling of the masses, in consequence of the radiation of heat, would cause their rotatory velocities to be increased, and this process would continue till, in most instances, a secondary ring would be abandoned by such planetary bodies.[30] The outer planets being larger and much less dense than the inner ones, would abandon *several* secondary rings. In reference to the number of these secondary rings cast off by each of the planets, all that we can

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[30] We have attempted here to be guided by strictly reasonable conclusions, without any reference to the phenomena of the Solar System.

at present conclude on, with any degree of certainty, is, that the number should, upon the whole, increase from the sun outward. We cannot, at present, say that the outermost planet ought to have abandoned the greatest number of rings. These secondary rings would, in general, break up and form secondary planets, or satellites. Under certain conditions—such as Prof. Peirce has found to exist in the System of Saturn—a ring, or rings might remain entire, or at least, not break up into a single satellite. According to this view, rings, if they exist, must be found interior to several satellites. In every case—unless changed by disturbing forces—the planet should rotate on its axis in less time than is required for any ring or satellite to revolve around the primary. The same must hold in the case of the sun and the planets.

45. When the last ring has been abandoned by any one of the planets, the remaining part[*] must cool down and thus form a primary planet. The outer portions being exposed to the low temperature of space—at least 50° below zero Fah.—they will cool much more rapidly than the inner parts, and after the whole is reduced to a liquid, a crust would, comparatively soon, form around the liquid planet, and further radiation would take place very slowly compared with its former rate. A certain amount of heat would be received from the other bodies, particularly the sun, (whatever might be its condition), and finally a balance would be reached beyond which the planet would not cool,—or at most very slowly,—and thus the inner parts would remain liquid. The outer planets being from the beginning less dense than the others, a crust would, perhaps, be formed so comparatively early as to leave the mean density of the planet comparatively small. We should, therefore, look for an increase in mean density from the outermost planet to the innermost. We see no reason, however, to believe that any simple law regulates this variation of mean density from one planet to another.

46. It would at first seem as if the satellites of the primaries should follow the same law of rotation that the primaries themselves do; but we must recollect that the numbers representing the distances of the former, expressed in radii of the latter, may differ very considerably from the numbers representing the distances of the latter expressed in radii of the sun. Again, the rings from which the satellites were formed, were abandoned when the primaries were much reduced in temperature, and condensed, when compared with the condition of the primary rings

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[*] "In the Nebular Hypothesis, after a fluid body has abandoned all the rings possible, there *must* remain a central body of comparatively large dimensions, and of a mass far greater than the sum of the masses of all the rings separated. How would the author of the meteoric theory as given in the 204th number of the North American Review, account for the fact that our Solar System is constructed upon this principle?

when they were separated from the primitive solar spheroid. All the satellites will be much smaller than the primaries; and, being comparatively small bodies, they will cool down and become solid much sooner than the primaries will. In consequence of the forces acting on the planets, both primaries and secondaries, they will become spheroidal in shape as soon as the rings from which they are made are broken up and the parts united. The attraction of the primaries on the fluid satellites, will raise the fluid particles into great tidal-waves; and as the waves on opposite sides are slightly unequal, the result will be a retardation of the rotatory velocity of the secondaries. William Ferrel, Esq., of the Nautical Almanac Office, Cambridge, Mass., has given[41] a mathematical discussion of the retarding effect of the tides of the earth on its rotatory velocity. According to his conclusions, the retarding effect of the earth upon the moon's rotatory velocity, both acting under similar circumstances, would be between 500 and 600 times as great as that of the moon on the earth's. According to his results, it would amount to an entire circumference of the moon, or one period of rotation of the moon in a hundred years. Even if these numbers be wide of the truth, the investigation shows us that, in general, the retarding effect of the attraction of the primaries on their secondaries, when in a fluid state, is sufficient, in the course of immense ages, to reduce the periods of revolution and rotation of the latter to isochronism. We have, therefore, great reason to suppose that as a general rule the satellites turn on their axes but once during a revolution around their primaries.

47. If there exists a cosmical ether, as is at present pretty generally admitted, in order that it may remain spread throughout universal space, it is only necessary for it to possess an elasticity so great that the action of luminous bodies is sufficient to produce a mechanical action in it that will enable it to maintain its temperature and fluid condition under all circumstances. This cosmical ether being material in its nature, it would necessarily partake of the motion of those bodies with which it remains in contact for immense ages of time. In the Solar System, the motion of the ether around the sun would be in the general direction of all the planets. It would also be regulated in its motion

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[41] Gould's Ast. Journal, iii, 138–141. Professor Ferrel informs me that some numbers which he employed he now knows to be about ten times too large, and hence his results should be diminished in nearly the same ratio. He found that in one century the moon retarded the rotatory motion of the earth's equator 37·44 miles. Dividing this by 10, to correspond more nearly with more recent data, we have about 3¾ miles for the retarding effect of the moon in a century. The effect of the earth on the moon he found to be 562¼ times as great as that of the moon on the earth; and this will give—since the diameter of the moon is only about one-fourth of the diameter of the earth—between 500 and 600 miles for the retarding effect of the earth upon the moon's equater, if it be only partly fluid. If it be wholly fluid, it would be somewhere near 1000 miles per century.

partly according to the third law of Kepler; and thus the planets, whatever might be their distances from the sun, would scarcely be retarded by it in their annual revolutions."[*] Since the ether is supposed to maintain its temperature, and therefore elastic force, it would not gather into separate collections, leaving some parts without it; but it would only be slightly condensed around the different bodies of the universe.

48. If we calculate the principal radii of gyration of Jupiter, at the time when each secondary ring was abandoned, upon the same principle as that adopted in the case of the sun, viz: by neglecting the masses of the satellites, we shall find such radii, at the time when each satellite ring was separated, commencing with the nearest and taking them in the order of distance—the equatorial radius of Jupiter being the unit—to be as follows:

TABLE IV.

$k_1 = 1{\cdot}308$; $a_1 = 6{\cdot}048$;
$k_2 = 1{\cdot}832$; $a_2 = 9{\cdot}623$;
$k_3 = 2{\cdot}630$; $a_3 = 15{\cdot}350$;
$k_4 = 4{\cdot}018$; $a_4 = 26{\cdot}998$;

a_1, &c., being the distances of the satellites. If we compare the radii of gyration, k, with the mean distance of the satellites, a, we see that in every instance the latter is not far from six times the former. If we make a similar comparison for the primary planets, using Table II, we see that the radius of gyration is only about the one-hundredth of the mean distance of the planet from the center of the sun. We hence conclude from this that the density of the primitive solar spheroid increased much more rapidly toward the center, than the primitive Jovian spheroid. In other words, the solar spheroid was relatively much more dense about the central parts than Jupiter. This we had reason to expect, since, according to the hypothesis, the ring from which the Jovian system was formed was already somewhat condensed when it was abandoned, and the general density much more nearly uniform than that of the solar spheroid; and hence the secondary rings were much denser relatively to the Jovian spheroid than was the ring from which Jupiter was formed relatively to the solar spheroid. This, it would seem, is a strong proof of the truth of the nebular hypothesis. The mass of Jupiter is 6000 times as great as the sum of the masses of his satellites; so that neglecting the masses of the satellites in comparison with the mass of Jupiter we neglected only the small fraction $\frac{1}{6000}$th. I supposed Jupiter homogeneous in finding his principal radius of gyration.

The periods of rotation of Uranus and of Neptune being unknown, we cannot compute the radii of gyration of the planets at the time when the satellite rings were abandoned, as we have done in the case of Jupiter. And if Bessel's mass of the rings of Saturn ($\frac{1}{118}$th of that of Saturn) be admitted, it is probable that this, and the sum of the masses of the satellites, will make a fraction too great to be neglected. The same is true of the earth and moon.

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[*] This conclusion differs from the basis assumed by Prof. Hinrichs in his article on the Age of the Planets, this Journ., [2], xxxvii, 36-56.

49. Let us now collect together in this article the various facts which ought to be found to exist in the Solar System, according to the preceding view of the nebular hypothesis.

1st. The planets should all move in the same direction around the sun. 2nd. Some comets whose distances correspond with those of the planets within moderate limits, should have the same direction as the planets. 3d. The orbits of the planets should not be much inclined to each other. 4th. All the planets, except, perhaps, a few of the outer ones, should turn on their axes in the same direction as that in which they move around the sun. 5th. The satellites should revolve around their primaries in the same direction in which the latter turn on their axes. 6th. The periods of revolution and rotation of the satellites, should, in many cases, at least, be equal. 7th. The sun must rotate on an axis in the same direction as that in which the planets revolve around him. 8th. The outer planets of the system should be larger than the inner ones. 9th. The outer planets should have the greater number of satellites. 10th. The outer planets should rotate on their axes in less time than the inner ones. 11th. The outer planets should in general have greater masses than the inner ones. 12th. The planets should, upon the whole, increase in density as they are found nearer the sun. 13th. The satellites should be less than their primary, and the planets less than the sun, both in bulk and mass. 14th. All the members of the Solar system should be composed of similar materials.

50. We have thus, in the preceding pages, attempted to give a connected view of the nebular hypothesis, uncompared, except in a few cases, with the real phenomena of nature. In what follows, we shall show that the phenomena of the sidereal heavens, and of the Solar System, agree very closely with the preceding deductions.

3. *A Comparison of the preceding Theory with the Phenomena of Nature.*

51. The origin of material existence is at present a mystery to the human mind. To say that it was created by the Deity is an assertion that conveys to the understanding something of which we have no definite conception. On the other hand, to say that it has always existed is equally unsatisfactory, since the human mind always looks toward a beginning of everything.

But with such speculations, we have here nothing to do. The nebular hypothesis supposes matter and all physical forces to exist; and we have only to carry back our speculations to the most ancient and chaotic state in which it is possible for us to suppose matter to exist. I have supposed matter in its earlier chaotic condition to have been necessarily heterogeneous in structure. Others have different views.[43] Even if it[44] were at first, or at any time, perfectly homogeneous, but not of symmetrical form, it would soon become heterogeneous, because various centers of attraction would be established, around which matter would accumulate; and thus the change in the different parts would generate a rotatory motion. Thus, under almost any conceivable—certainly probable—circumstances, the nebulous mass must be considered as having sufficient potential energy to give it a motion of rotation. But we must not be confined to these speculative views. "Every well-trained philosophical judgment is accustomed to observe illustrations of the most sublime phenomena of creation in the most minute and familiar operations of the Creator's laws, one of the most characteristic features of which consists in the absolute and wonderful integrity maintained in their action whatsoever be the range as to magnitude or distance of the objects on which they operate. For instance, the minute particles of dew which whiten the grass blade in early morn, are, in all probability, moulded into spheres by the identical law which gives to the mighty sun its globular form."[45] "It is remarkable of physical laws that we see them operating on every kind of scale as to magnitude, with the same regularity and perseverance. . . . . . Two eddies in a stream fall into a mutual revolution at the distance of a couple of inches, through the same cause that makes a pair of suns link in mutual revolution at the distance of millions of miles. There is, we might say, a sublime simplicity in this indifference of the grand regulations to the vastness or the minuteness of the field of their operations."[46] We thus may learn from the minuter operations of nature of those grand revolutions which we have reason to conclude have taken place in past ages of duration. Binary, ternary, and multiple stars, in some, perhaps many, cases, may be the resulting motion of two or more stars—I should more properly say primitive stellar spheroids, coming so near together that their atmospheres came in contact and thus gave them a motion around their common center of gravity.[47]

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[43] "I regard the gaseous as not the *ultimate* form of matter, but that in its ultimate state it was *absolutely* imponderable and perfectly elastic, and therefore it completely filled, absolutely, infinite space, and that the myriads of Solar vortices were developed out of this perfectly homogeneous imponderable fluid in a manner as definite and orderly as that which *regulates the production* of a plant or an animal on the earth."—*Dr. Joel E. Hendricks.*

[44] Some writers seem to doubt the existence of nebulous matter, because the telescope continues to resolve the nebulæ. But comets are evidently in a nebulous state, and the existence of these shows us that matter may assume the nebulous condition. It is a fact that the evidence derived from observation, that nebulous matter, in large quantities, exists, is by no means conclusive. In fact, the truth of the nebular hypothesis cannot depend on our knowledge of the existence of nebulous matter in large quantities. It is sufficient to show that matter in such a state can and does exist in *any* quantity, and after that we must look to the general and particular phenomena of nature. It is scarcely probable that we shall ever learn, at least for ages to come, directly from observation, that large quantities of matter exist in a nebulous condition. But the general appearance of starry clusters, may indicate the origin whence they were derived. The appearance and arrangement of the planetary system are more conclusive in reference to the origin whence that system was derived.

[45] Mr. Nasmyth. See Annual of Scientific Discovery for 1857, p. 187.

52. All apparent motion in the region of the so-called fixed stars, is very slow; so slow, indeed, that in most instances it requires several years to elapse before *any* motion becomes apparent. Certainly, in most cases the nebulæ are situated far beyond the region of the visible stars. How much slower, then, than the stars, must the nebulæ appear to change their relative parts or positions in the heavens! We must, therefore, expect little from the motion of nebulæ or even clusters, in confirmation of the nebular hypothesis. We must look to the general conformation of nebulæ and of clusters. Lord Rosse has within a comparatively few years, by means of his great reflectors, shown that many of the nebulæ are of the *spiral* form. They appear as if they possess a rotatory motion on an axis, and this motion has so far increased as to project some parts of the nebulæ tangentially, and thus to break up the spheroidal form, causing parts of the nebulæ to fly off in one or more streams. The general principles which would lead to such a result, we have attempted to give from Arts. 3 to 12 inclusive. Mr. Nasmyth has also given[48] an exposition of the principles, though he has not based his explanations on mathematical formulæ and deductions as I have attempted to do. Many nebulæ present that appearance which we suppose our Solar System to have presented before the first planetary ring was abandoned, viz: a projected circular or elliptic appearance, not very bright in the outer portions, but increasing in luminosity with considerable rapidity near the center.

53. Some observations and deductions of a very interesting character, have recently been made. Many years ago, Sir William Herschel concluded, from a comparison of a figure of the nebula in Orion, made by Huygens many years before, with a figure of his own drawing, that some parts of the great nebula —(cluster, perhaps we should now say, since it has been par-

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[46] Vestiges of Creation. Harper's Ed., p. 17.

[47] I am unable to ascertain in what direction, if any definite one, double stars revolve around their common center of gravity. Who will give the required information? It is worthy of attention.

[48] Ann. of Sci. Disc. for 1852, pp. 187-8.

tially resolved since Herschel's day)—have changed. It is very probable that no such change has taken place; but that the supposed variation came from a defect in Huygen's representation, and from a difference in the telescopes employed. But it seems now quite certain that some nebulæ have changed. The nebula around *Eta Argus*, seems to be one of that character.[49] Also several others have been discovered in which some change seems to have taken place. Accurate micrometrical measurements and photographic representations only can here serve for the basis of positive knowledge in this interesting department of astronomy. It would seem that such patches of nebulæ, if, indeed, they *are* nebulæ, proper, cannot be very extensive, since they must be comparatively near our system. We must now leave this interesting part of our subject, since this, alone, could never, so far as we can see, furnish us the necessary data to enable us to say whether the God of Nature followed one or another method in the process of world-making. We must descend to the general and some of the particular phenomena of the Solar System, to enable us to draw a definite conclusion on this subject.

54. We know from observation that all the primary planets revolve around the sun in the same direction, and in nearly the same plane. The sun rotates on an axis in the same direction in which the planets revolve around him, from west to east. The period of the sun's rotation is about 25½ days, which is about $\frac{7}{24}$ths of the periodic time of Mercury. According to our theory, no planet will ever be discovered so near the sun that its period of revolution will be less than the period of the sun, viz: 25½ days, if this number be accurately determined. This is contrary to the supposed discovery of Mr. Lescarbault.

The inclination of the orbit of Neptune, as we have shown, corresponds very approximately with that of the invariable plane of the Solar System, as our theory requires. We also find that the distance between the planetary orbits increases with the distance from the sun. The following table will show

TABLE V.

Planet.	Mean dist.	Dist. to next Ex. Orbit.	Diam. of sphere of attraction.
Mercury,	0·3870981	0·336235	0·200979
Venus,	0·7233306	0·2766684	0·383390
Earth,	1·0000000	0·5236923	0·521348
Mars,	1·5236923	1·544983	0·779587
Ast. Planet,	3·068675	2·134101	0·968693
Jupiter,	5·202776	4·336010	4·876551
Saturn,	9·538786	9·643604	8·618608
Uranus,	19·18239	10·857110	7·487871
Neptune,	30·03950		

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[49] This Journal, [2], xxxvii, 294-5.

the difference between the mean distances of the planets, in terms of the mean distance between the earth and sun; and also the breadth of the primitive rings, according to Kirkwood's Analogy.[50] We have at present no method probably more accurate than Kirkwood's to determine the breadth of the primitive rings.

We thus see that, generally speaking, there is an increase of distance between the orbits of the planets, and also in the breadth of the rings abandoned by the sun; and yet it is not absolutely the case in every instance; nor is the increase according to any simple law, as we had occasion to point out from what was assumed to be the probable condition of the primitive solar spheroid.

55. If we compare the masses and the densities of the planets, we shall see here, also, a general increase in the masses, and a decrease in the densities, as we get farther from the sun, as can be seen in the following table:

TABLE VI.

Planet.	Magnitude.	Mass.	Density.
Mercury,............	0·0595	0·0729	1·225
Venus, ...	0·9960	0·9101	0·908
Earth,.............	1·0000	1·0000	1·000
Mars, ...	0·1364	0·1324	0·972
Ast. Planet, ...		0·2869 [51]	
Jupiter,............	1491·0000	338·7180	0·227
Saturn,............	772·0000	101·3640	0·181
Uranus,............	86·5000	14·2510	0·167
Neptune,...........	76·6000	18·9000	0·321

We see from this table what was concluded from theory to be very probable, viz: that in general the volumes and masses increase from the sun to a maximum—which is reached in Jupiter—and then decrease to the extremity of the system. The fact that there is no simple law perceptible which regulates the increase and decrease of the numbers in the two preceding tables, shows that the condition of the primitive solar spheroid was far from being symmetrical. The fact that the earth is more dense than Venus may be thus explained. The difference in the condition of the Earth-ring and the Venus-ring could not have been very considerable. But the former, after it united into a single spheroid, threw off a satellite-ring, taking off the rarer part and leaving the denser, so that the ultimate mean density of the earth became a little greater than that of Venus. There seems to be some peculiarity about Saturn in respect to density that cannot be easily accounted for.

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[50] This Journal, [2], xiv, 213.

[51] According to Kirkwood's Analogy, this Journal, [2], xiv, 213.

56. In examining the secondary planets, we see a general increase in number to each primary planet, as we go from the sun to the extremity of the system. The Earth has 1, Jupiter 4, Saturn 8, Uranus 6 and perhaps 8, Neptune 2 that have probably been seen. We cannot tell with certainty the number of satellites belonging to each of the last two planets, owing to the difficulty of observing such bodies at so remote distances. The satellites of Jupiter are distributed in the Jovian system very similarly to the primary planets in the Solar System. We find the masses to increase as the distance from the center of Jupiter increases, till we arrive at the third satellite, where we reach the maximum. The fourth satellite is the second in mass. The third satellite is also the largest, and the fourth the next in size, but the second, although it is of greater mass, yet is of smaller size than the first. The satellites of Saturn follow a similar general law. *Titan*, the 6th in the order of distance from the primary, is the largest, and *Japetus*, the most distant of them all, is next in size. Of the others we cannot so well judge, but it seems not improbable that they decrease in size as they are nearer the primary. We know still less of the satellites of Uranus, but the second and fourth, so-called, are very probably the largest. But within the second, (in the order of distance according to Sir William Herschel's discoveries), two, and possibly three, exist. We thus see it to be a general principle of the planetary system that the largest bodies are within some, and without some of the orbits of the others. We also observe this fact in reference to some of the secondary planets, viz: if we divide the distance of the remotest satellite of any primary, from the center of the planet, by the diameter of the sphere of attraction (which we shall consider to be the same as the breadth of the ring, or the diameter of the primitive planet,) we shall find that, in general, the quotient decreases from the earth to Uranus. We may therefore conclude that the outer planets had to condense much more than the inner ones before a satellite-ring was abandoned. It is a fact, also, that, so far as observation has determined, the satellites rotate on their axes in the same direction and in the same time in which they revolve around their respective primaries. All these motions are in the direction of the rotation of the primaries.

57. The rings of Saturn offer a living example of the primitive secondary rings. They open to us, in a measure, the nature and constitution of the primitive rings, both the primary and secondary. These rings have been retained as such, according to Prof. Peirce, by the attraction of the satellites. Should these rings entirely break up, they would probably form asteroid satellites. Since the rings of Saturn are very thin in comparison with their width, we conclude that the primitive planet was very

much flattened about the poles. If the rings of Saturn are fluid, they exist as such in consequence of the friction of their particles.

58. It is an interesting fact, and one confirmatory of our theory, that the rings of Saturn have a longer period of rotation than the planet itself, as required by the nebular hypothesis. According to the observations of Sir William Herschel, the rings of Saturn revolve around the planet in the space of $10^h\ 32^m\ 15^s$.[62] The period of rotation of Saturn is $10^h\ 29^m\ 17^s$.[63] We thus see that the rings require $2^m\ 58^s$ longer time to revolve about Saturn, than the latter does to turn on his axis. Here again we appear to have obtained conclusive evidence in favor of the nebular hypothesis. Had the period of Saturn's rotation been greater than that of the rings, it would have been very difficult to reconcile it with the nebular hypothesis.

59. The rings of Saturn are either fluid or composed of unconnected particles. According to Maxwell, even a fluid ring would be broken up into small satellites; but it does not follow that these satellites may not still be fluid and unite again into a ring—or perhaps several nearly concentric rings—after having been separate a sufficient length of time to restore the equilibrium by counteracting the disturbances to which the rings are subject. We are thus reduced to the very probable conclusion that the rings of Saturn are fluid (liquid). We are thus carried back one great step toward the gaseous condition from which we have supposed all the planets and their satellites to have been developed.

60. Observation shows that the satellites of Uranus revolve in a retrograde order. But we do not know in what direction, nor in what time, the planet rotates. If it should finally prove to be the case that the satellites of Uranus are in the plane of the planet's equator, and revolve in a direction opposite to that of the rotation of the planet, it will be very difficult, if not impossible, to reconcile the two motions with the nebular hypothesis. But should the axis of the planet be found to have an inclination to the plane of the orbits of the satellites differing considerably from a right-angle, the difficulty will not be insuperable.[64] But since the nebular hypothesis accounts so satisfactorily for so many of the phenomena of the solar system, we may venture to predict that the rotation of the planet will be found to be retrograde, or closely approaching that direction, if we may so express ourselves. But our theory, as we have

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[62] Grant's Hist. of Phys. Astr., p. 262.

[63] Herschel's Outlines, Art. 514.

[64] By giving an artificial globe a motion of rotation on its axis, and then reversing the poles, we can easily see how readily the direction of rotation is changed to the opposite direction. The same primitive impetus carries it in the opposite direction.

shown, requires that the outer planets rotate in a retrograde direction. Kirkwood's Analogy seems to require that Uranus should occupy about 37 hours in performing one rotation. Is it possible that a retrograde motion will influence the time of rotation of the planet? It would seem that some of the great telescopes, either in this country or in Europe, ought to be able to settle the question of the rotation of Uranus. I have not attempted to account for the great difference between the greatest and least diameters of Mars. A mere supposition could be of no advantage.

61. The following table gives the time of rotation of the several planets so far as known:

TABLE VII.

Planet.	Time of rotation.			Diam. of sphere of attraction.
	h.	m.	s.	
Mercury,	24	5	28	0·200979
Venus,	23	21	21	0·383390
Earth,	23	56	4	0·521348
Mars,	24	37	22	0·779587
Jupiter,	9	55	26	4·876551
Saturn,	10	29	17	8·618608

By the above table we see that, roughly speaking, the time of rotation diminishes as the sphere of attraction increases. The original impulse of rotation communicated to a planet, by the ring from which it was formed, at the time of its breaking-up, would be dependent on the difference between the velocity of those parts of the ring lying upon the opposite sides of the orbit of the future planet; or, in other words, upon the difference of the living force of each part. This difference would not depend on the absolute velocity of either part, but rather upon the relative distance of the two parts from the sun. We hence see that since the outer primitive rings were wider than the inner ones, we should expect a more rapid rotatory motion in the outer planets.[46] Since Saturn has the greatest sphere of attraction we should at first expect that planet to have the shortest period of rotation. But such is not necessarily the case. If this whole planetary ring had condensed into a single body, such might and probably would have been the case. But every time a planet abandoned a ring to form a satellite, it, in effect, lengthened the period of rotation of the future planetary spheroid. Jupiter abandoned only four rings of which we have any knowledge, and the united mass of them forms only the $\frac{1}{6000}$th of the mass of Jupiter; while Saturn abandoned no fewer than eleven rings, three of which still remain. According to Bessel, the mass of the remaining rings alone equals the $\frac{1}{118}$th of the mass of Saturn. Even now the period of Saturn's rotation exceeds that of Jupiter by only $3^m\ 51^s$.

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[46] Since the outer rings were less dense than the inner ones, the planets would have farther to contract than the inner ones, and this also would increase the rotatory velocity.

62. Humboldt has called attention[46] to the fact that the group of Asteroids divides the planets into two classes, those within and those without the zone of small planets, each class being marked by certain distinctive features. The outer group, "Neptune, Uranus, Saturn and Jupiter, are distinguished by considerable volume and slight density; the time of rotation about their axes is about ten hours, and from this there results in these bodies considerable flattening; moreover, of the 22 satellites of the solar system, 21 pertain to this group. The four *inferior* planets, Mars, the Earth, Venus and Mercury, have, on the contrary, much smaller volumes and much greater density; they revolve on themselves in nearly twenty-four hours, are but little flattened, and possess, among all the four, but one satellite, the Moon." Let us now see what account we can give of these peculiarities. We have already shown that according to Prof. Peirce's deductions a planetary-ring might exist for a great length of time just within the orbit of Jupiter; the same influence that would sustain a ring where the asteroids now exist, would tend to sustain one where Mars now revolves; and the same influence would extend to the orbits of the Earth and of Venus; and each of these latter bodies would lend their influence, and would thus influence the Mercurial-ring. These interior rings (as we will call them, being interior to the Asteroids,) being thus sustained for a great length of time, would loose much of their heat, and thus become condensed considerably perhaps, as have the rings of Saturn. When such rings broke up and assumed the spheroidal form, they would be much less likely to abandon satellite rings, than those planets whose rarity was greater. We have already shown that the mean density of those planets nearer the sun should be found to be greater than that of the outer planets; we now refer to the influence above mentioned to account for the sudden increase of mean density within the orbit of Jupiter; for evidently the mean density of a planet formed from a ring considerably condensed, would be greater than one formed from a ring less so; for a crust would form around one approximately as soon as around the other. The period of rotation of such planets would be greater than that of others, since they would not have so far to contract. The times of rotation would be approximately equal, since they would contract nearly equally.

63. The existence of 80 known Asteroids between the orbits of Mars and Jupiter, seems to confirm the view which we have given of their formation, in a preceding part of this paper. If we divide 360° by 80, we find an average of one Asteroid for

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[46] Cosmos, iv, 422, Bohn's ed.

every 4½°, making approximately a ring of these small bodies. But we cannot for a moment entertain the idea that nearly all these bodies have been discovered. They are very small and necessarily difficult to detect; and judging from what we already know of the size of these small bodies, we must conclude that many more exist, much the larger share of which are too small to be detected by our best telescopes. The most rational theory of meteoric stones, is that which regards them as minute asteroids. The whole group, many thousands very probably, perhaps millions—between the orbits of Mars and Jupiter, must have very eccentric orbits, and be subject to extraordinary perturbations in their motions, and it is highly probable that many of them become satellites of some of the planets, and finally come in contact with their surfaces as meteoric stones. It is certainly a little curious that the orbits of the known Asteroids intersect. The mean width of the whole zone so far as known, lies between the limits 2·145 (Feronia) and 3·452 (Maxamiliana) giving a breath of 1·307, which is rather greater than the diameter of the sphere of attraction of Kirkwood's Asteroid-planet.

At present we shall add nothing respecting comets, as Prof. Kirkwood has called attention, in several places, to the orbits of these bodies.

Hector, N. Y., Nov. 11, 1864.

ART. V.—*On Brushite, a new mineral occurring in Phosphatic Guano;* by GIDEON E. MOORE, Ph.B.

(Communicated to the California Academy of Sciences, Sept. 5th, 1864.)

IN the spring of the present year, I received, through the kindness of Wm. E. Brown, Esq., of Mare Island in this State, a specimen of a mineral discovered by him in a cargo of phosphatic guano at Camden, N. J. The locality from which it was derived is not known,[1] and, though letters of inquiry have been sent to the parties to whom the cargo was originally consigned, no reply has been received up to this date. The texture and appearance of the guano would, however, point to some one of the Carribean islands, and more particularly to the island of Sombrero as its probable source. It is very probable that the mineral may be recognized among the crystallized products occurring in other guano deposits.

In the specimen in my possession, the mineral occurs filling seams in the guano, varying from ⅛ to ½ of an inch in width. The matrix itself is of the variety known as rock guano. It possesses an oölitic structure and a brownish white color, interspersed with small spots of pure white.

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[1] **In a letter from Mr. Moore, dated San Francisco, Nov. 18th, 1864, he states that he has ascertained the locality of the new mineral to be Avis Island in the Carribean Sea.—EDS.**

The mineral is in the form of small but very perfect and brilliant crystals, with a cleavage in the direction of their greatest length nearly equal to that of selenite, the laminæ being also slightly flexible, as in the case of the latter species. Hardness, 2·25. Specific gravity, 2·208 (mean of two determinations). Color yellowish white. Transparent. Lustre vitreous, splendent, inclining to pearly on the cleavage faces.

When heated in a closed tube before the blowpipe, it whitens and gives off water at an incipient red heat. In the platinum forceps, it fuses with intumescence at about 2 on von Kobell's scale, tinging the flame with the peculiar green characteristic of phosphoric acid. The button formed by fusion crystallizes on cooling, showing numerous brilliant facets. It readily dissolves, even in coarse crystals, in dilute nitric and chlorhydric acids.

A qualitative analysis revealed the presence of lime, phosphoric acid and water, with barely discernible traces of magnesia and alumina.

The quantity of mineral at my disposal was very small, scarcely exceeding one gram in weight. In each of the two following analyses, the water was determined in 0·2 gram, the remaining 0·3 gram being employed in the determination of the lime and phosphoric acid. The result was as follows:

	1.	2.
Lime, - - - - -	32·65	32·73
Phosphoric acid, - -	41·50	41·32
Water, - - - - -	26·33	26·40
	100·48	100·45

These figures agree exactly with the composition of the neutral tri-basic phosphate of lime, 2CaO, HO, PO_5, with the addition of four equivalents of water of crystallization (2CaO, HO, PO_5 + 4 aq), viz:

2CaO, - - - - -	56·26	=	32·59
PO_5, - - - - -	71·36		41·34
HO - - - - - -	9·00		
4 aq, - - - - -	36·00		26·07
	172·62		100·00

In the polarizing microscope, the mineral shows a vivid succession of colors. A sample has been sent to Prof. J. D. Dana, who has kindly undertaken the study of its crystallographic characters, and I hope in a short time to be able to communicate the results of his investigations to the Academy.

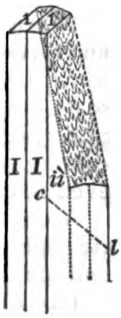
It is with great pleasure that I dedicate this species to Prof. G. J. Brush, of Yale College, to whose unwearied zeal and efficient labors American Mineralogy stands so deeply indebted.

San Francisco, Cal., Sept., 1864.

ART. VI.—*On the Crystallization of Brushite;* by JAMES D. DANA.

(Communicated to the California Academy of Sciences.)

THE crystals of the new mineral Brushite which I have had under examination were received from Mr. G. E. Moore, to whose chemical investigation science owes the first determination of the species.



The crystals are slender prisms, not over a third of an inch in length. A common form (containing all the occurring planes) is shown in the annexed figure. The prisms are monoclinic, and are often flattened parallel to the clinodiagonal, as here represented.

Cleavage is perfect parallel to the clinodiagonal section, or the plane *iì*; also distinct, parallel to the lines *cl*, as apparent often in the cross fractures of crystals, and by occasional striæ. This plane of cleavage may be called the basal, or *O*.

The planes *I* and 1 are brilliant, especially the former. The oblique plane, situated on the back side in the figure, which may be called *r*, is quite rough, owing to an oscillatory combination of two hemi-octahedral planes. In many of the crystals, only the right one of the two planes *I* is present, and also only the left one of the two planes 1. The prisms frequently terminate above in an irregular edge made by the meeting of the one or two planes *I* and the rough plane *r*, and this edge is sometimes cut off, more or less deeply, by a single oblique plane, which is one of the planes 1.

According to measurements with the reflective goniometer—

I : *I* = 142° 26′ 1 : *iì* = 101° 40′
I : *iì* = 108 47 1 : 1 = 156 20 (approximately.)

The inclination of 1 on 1 could not be accurately measured on account of the minuteness of the planes in the crystals in which both planes occur, and the want of perfection in the reflection. The angle obtained for 1 : *iì* would give for 1 : 1 156° 40′.

By measurement with a goniometer attached to a compound microscope, the plane angle between the lines of cross cleavage, or *cl*, and the edge *I* : *I* (which equals the inclination of *O* on the orthodiagonal section or the plane *ii*) was found to be 117°–117½°; and that between edge *I* : *I* and edge 1 : 1 (which equals *ii* on 1*i*, both unobserved planes), 95° to 95½°; whence, *O* : 1*i* would equal, approximately, 147° 30′. The inclination of the rough plane *r* on the edge 1 : 1 is about 110°, but varies much.

The results of calculation, taking as data the above-mentioned angles $I : I$ and $1 : i\grave{\imath}$, along with the inclination of O to ii = 117° 15′, and that of the edge $1 : 1$ (or $1i$) to ii = 95° 15′, are as follows:

$$C\ (= O : ii) = 117^\circ\ 15' \text{ and } 62^\circ\ 45'$$

$$a \text{ (vertical axis)} : b \text{ (clinodiagonal)} : c = 0{\cdot}5396 : 1 : 2{\cdot}614$$

$$1 : 1 = 156^\circ\ 46' \qquad -1 : -1 \text{ (unobserved planes)} = 164^\circ\ 22'$$

The species is related in form to Vivianite, in which

$$a : b : c = 1{\cdot}002 : 1 : 1{\cdot}3843;$$

for, if we double the a of Brushite, and halve the c, we have for the ratio of its axes—

$$2a : b : \tfrac{1}{2}c = 1{\cdot}0792 : 1 : 1{\cdot}307.$$

The two species are also alike in the perfect and pearly clinodiagonal cleavage.

ART. VII.—*Introduction to the Mathematical Principles of the Nebular Theory, or Planetology*; by GUSTAVUS HINRICHS, Professor of Physics and Chemistry, Iowa State University.

THE *nebular hypothesis*—the boldest thought that ever elevated the human mind, by bringing us, as it were, in sight of the mysterious fiat of the Almighty—was, in its great general features, unfolded almost at the same time by Germany's deepest thinker, the Königsberg philosopher, IMMANUEL KANT, and by PIERRE SIMON DE LAPLACE, the greatest mathematician of France. It is truly the closing stone in the philosophy of the celestial vault; for Copernicus and Kepler made us behold the foundation,—the first, by placing the sun as the lantern of the world in the center, and surrounding it with the planets—the second, by destroying the cycles and unravelling the harmony of the spheres in his immortal laws; and after the existing phenomena had thus been rightly viewed, Newton made us behold the invisible bond that connects the members of the system, while at length Kant and Laplace pointed out to us the hand that at "the beginning" projected these celestial balls into space and thereby insured the continued existence of the system.

But notwithstanding this noble parentage and its being the *logical sequence* of the discoveries in the theory of cosmos made by Copernicus, Kepler and Newton, the nebular theory enjoys as yet but slight consideration among astronomers. Arago[1] is the only one of these who has deigned to consider it earnestly, and he probably did so more in his capacity as a physicist than as an astronomer.

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[1] Arago, Astronomie Populaire, ii, 7. Paris and Leipsic, 1855.

The reason of this neglect seems to be the incomplete state in which even Laplace himself left the theory. Direct observation, moreover, seemed to contradict some laws given as necessary consequences of this hypothesis.

We have already, in a former article,[2] tried to vindicate the theory in this last respect by showing that the hypothesis is really confirmed even in these apparently contradicting observations. We will now endeavor to give a somewhat more complete development to the fundamental principles of Kant and Laplace, and to exhibit the exact position of the nebular theory itself, hoping thereby to show that this theory, if we only study it earnestly and patiently both by experiment and analysis, fully deserves our confidence.

As this subject is as vast as it is difficult, we beg the critic always to keep in mind that we do not pretend to give a treatise, but merely offer an *introduction* to this almost new field of analysis.

We commence with a short survey of the fundamental principles and the aim of the theory of the solar system, in order clearly to understand why the nebular theory is necessary, what it will have to accomplish, and how far it already has done its duty.

§ 1. *The fundamental constants of the Solar system.*

As the discovery of a law of nature is but the reduction of the infinitude of observed quantities to a few constants by means of a function, the algebraic expression of the law—we see that *the progress of astronomy to a great extent must be identical with the reduction of the number of such constants.* This is fully borne out by the history of the science. For, while the Ptolemaic theory[3] of the planetary motion required the radii and inclinations of seventeen different circles to express the observed motions of Saturn, Kepler reduced this number of constants to three, the semi-major axis, eccentricity, and inclination of the orbit. This very principle is also placed by Laplace[4] at the head of his *Mécanique Céléste.*

We may therefore trust in this principle, and with Laplace try to reduce the number of indispensable constants. We must first, however, ascertain which are those constants that are now *considered* fundamental or indispensable.

The *constants of the solar system* now exclusively deduced from observation are:

1. The *mass*, m, of the planet.

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[2] The density, rotation, and relative age of the planets: this Journal, Jan., 1864.

[3] Fracastor; see Bailly, Histoire de l'Astronomie moderne; vol. i. Paris, 1779. Eclaircissements, livre iv, § 28, and livre viii, § 27.

[4] Il importe extrêmement d'en bannir tout empirisme et de la réduire à n'emprunter de l'observation que les données indispensables.—*Méc. Cél.—Plan.*

2. The *figure.* On assuming a primitive fluidity and a small velocity of rotation, theory gives an ellipsoidal figure of the planets; hence not independent of observation.

3. The *ellipticity* of the planet. Theory may assign to it a higher and a lower limit by means of the mass (1) and the angular velocity (6)—but though the connection of this constant with others thereby is manifest, still its exact value can only be derived from observation.

4. The *volume,* or *diameter,* of a planet. Combining this constant with the first (mass) we obtain the *density.*

5. The *plane* (or *inclination* of the axis),

6. The *direction,* and

7. The *velocity of rotation.* Though this last element bears relation to others, still its exact value can only be obtained from observation. Even if Kirkwood's law of rotation[*] should prove to be perfectly exact, this element would continue to be a fundamental constant as long as that law remains an empirical one.

8. The *distance,* a, of a planet from the sun, or the semi-major axis of its orbit. By the theoretically proved third law of Kepler, we get from this constant the *periodic time,* T, requiring only the *constant,* μ, *of gravitation* to be known, and this latter is the same for all planets.

9. The *plane* or *inclination,* i, of the orbit.

10. The *direction* of the motion. The *velocity* is given by the distance.

11. The *eccentricity of the orbit.* This constant *is* fundamental, for the theory of gravitation only proves the orbit to be a *conic section* of some kind. The eccentricity can only be found from observation.

12. The *number of satellites* of a planet is also fundamental—and for each of them the same eleven constants have to be taken from observation; the first seven even are required by the sun.

From these twelve empirical data, theoretical astronomy can deduce the motion of the corresponding planet. The whole number of empirical constants is not at all inconsiderable; for

8 principal planets,	constants	1–12,	96
80 small planets,	"	8–11,	320
23 secondary planets,	"	1–11,	253
The sun,	"	1–7,	7
	Total number of constants,		676

to which the corresponding constants for the comets would have to be added.

What, in the face of this great number of constants that astronomy has to borrow from observation, shall we say about the boasted perfection of this science? Is it not in science, as in morals, that self-adoration hinders progress? Can any astronomer who has not merely studied the details of the celestial mechanics, but also kept in mind the great principle laid down by its author in his "Plan"—can he still pretend that Newton's theory of the solar system merely needs further development, seeing that the few bodies of this system require him to borrow about seven hundred constants from observation?

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[*] This Journal, ix, 395, May, 1850; also xiv, 210, Sept., 1852.

We shall honor the memory of Newton much more by trying to go beyond the results of his labors than by stupidly worshipping[6] the same, and thus arresting the progress of that science to promote which he spent his life.

§ 2. *These fundamental constants sustain remarkable relations to each other.*

The Newtonian theory of gravitation simply accepts these constants as observation gives them. For if our earth had Jupiter's mass, the rings of Saturn, the moons of Uranus and its axis in the ecliptic, the latter perpendicular to the orbit of Jupiter, a retrograde motion in a hyperbolic orbit—it still would as fully and as beautifully confirm the theory of universal gravitation as it does now; for, let us openly and frankly acknowledge it, these constants are independent of the theory of gravitation because the latter is independent of the former.

But, though this theory does not give any reasons for any kind of dependence between the often mentioned constants, observation shows that they sustain very remarkable relations to each other;[7] or in other words, *there are relations and laws in our solar*

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[6] The literature of astronomy teems with implicit instances hereof; but we find also direct expressions of this feeling, like the following:

Enfin, nous avons vu que ces resultats eux mêmes peuvent se composer en un seul et se representer par une loi unique, celle de la Pésanteur universelle; parvenus à principe nous nous voyons en quelque sorte élévé à *la source commune de tous les faits astronomiques; tous en dérivent* de la manière la plus simple et ils y sont en quelque sorte comme concentrés. Nous avons donc pour ainsi dire décomposé le systême du monde, *nous l'avons réduit à son élément unique*, et nous l'avons ensuite recomposé.—Biot, *Traité élém. d'astronomie physique.* Paris, 1805. Concluding remark of the work.

This "élément unique" is rather singularly unique, requiring no less than seven hundred elements to be borrowed from observation alone!

[7] Newton was aware of this—indeed, nobody can help seeing some of these relations. In tho scholium to the third book of Principia he says:

"Planetæ sex principalis revolvuntur circum solem in circulis soli concentricis, eâdem motus directione, in eodem plano quamproximé. Lunæ decem revolvuntur circum terram, jovem et saturnam in circulis concentricis, eâdem motus directione, in planis orbium planetarum quamproximæ. *Et hi omnes motus regulares originem non habent ex causis mechanicis.*" Edit. *le Seur* et *Jacquier* Geneva 1749–42.

It is customary to censure Kepler's fancy in contrast to the solidity of all Newton's words; still a sentence like the above is much more objectionable in science than the boldest fancy, for the latter is not accepted without severe scrutiny, while the former is repeated as a sacred truth. If Newton had written "mechanical causes known to me" instead of by "mechanical causes," simply to imply that he knew them all, he would have prevented many a drawback that has encumbered *world of a still higher order than those deduced from gravitation.* Thus, the inclinations of the orbits of the principal planets, instead of being uniformly distributed over the first quadrant, are all very small; and their direction, instead of being as often retrograde as direct, are for *all* planets and most satellites *direct.* Instead of having the eccentricities regularly varying from 0 to ∞, we find them for all planets nearly zero! The same may be said of any of the above fundamental constants, and not least of the distance, as it is found *approximating* to Titius–Bode's law, that is, to

$$a_n = 4 + 3 \cdot 2^{n-1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

But quantities that sustain mutual relations to each other are the particular values of a certain function for definite given values of the variable quantities; hence, if we intend to be true to the spirit expressed by the words of Laplace above quoted, it is a problem legitimately belonging to astronomy *to find these functions of which the fundamental constants are but particular values.*

Let us boldly face this great problem and not desist though astronomers tell us that it is not part of their science.

§ 3. *The fundamental constants satisfy the conditions of stability of the system.*

Since relations exist between the values of the fundamental constants, we may ask for the most general expression of these relations. The principal of these relations are, by Lagrange and Laplace, proved to be such as to fulfill the conditions of stability of the system. These conditions are—

1. *Incommensurability of the times of rotation,* ensured by the distances forming an exponential series (1).
2. The *central mass* vastly preponderating, and the greatest masses revolving where the mutual distances are the most considerable.
3. The *direction of all motions* is the same.
4. *The plane of all orbits is and remains nearly the same,* because

$$\Sigma m \sqrt{a} . tg^2 i = c_1, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

is and remains a small quantity (the letters having the same signification as in § 1).

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science. Less arrogant, because more true, he is when writing to Burnett (about 1680–81), "but yet I must confess I know no sufficient cause of y^e earth's diurnal motion."

Even Biot, notwithstanding his blind admiration, above cited, cannot help seeing something *more than* gravitation can account for, when noticing the harmony in the rotary and translatory motions. He says: "cet accord, *qui tient sans doute aux premières causes qui ont déterminé les mouvements planétaires, est un des phénomènes les plus remarquables du système du monde.*—Astron. physique, vol. iv, chap. v, p. 467.

5. *The eccentricity, e, of the orbit is and remains nearly the same,* because

$$\Sigma m \sqrt{a} . e^2 = c_2, \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

is and remains a small quantity.

6. The *density, d*, is such that the diameter of the bodies is small in comparison to their distances.

We may add—

7. The *form* of the planets is such that the influence of the deviation from a sphere is the smallest possible.

§ 4. *Gravitation is insufficient.*

The laws of Kepler are grand—as well as Newton's theory in accounting for them; but the above laws of Lagrange and Laplace are certainly of a superior order, and the theory of gravitation in failing to give even a shadow of a reason for these laws proves itself to be not the whole truth: we must go beyond this force!

Astronomers seem to forget the *history* of their own science; for how could they otherwise deny the legitimacy of accounting for the fact that the above laws express the stability of the world? Had not astronomers at the time of Kepler the same reason to be satisfied with his laws as astronomers have now in abiding by the laws of stability? And is it not as urgent to discover the causal connection between these laws ensuring great *duration* to our system, as it was to find in gravitation the mechanical cause of those laws ruling the spheres at the time being?

Now the hypothesis of Kant and Laplace will be found to account for the laws of stability as rigidly as the hypothesis of Newton accounts for the laws of Kepler; why, then, deride the former and adore the latter hypothesis? Or do we even forget that "the principle of gravitation" is but a "*hypothesis?*" Did not Newton himself consider it as such? Is not this force fully as mysterious and fully as much beyond the reach of direct observation as the chaos of Kant and Laplace? The former we *assume* as continually acting, because we find that *the motions* are such as this force would produce (*provided* a tangential force, of which gravitation knows nothing, also acts in a definite manner, etc., etc.). Why not also assume the latter as having been real, if we find by the same mechanical deductions that the existing harmony, as expressed in the stability of the system and ensured in the mutual relations of the fundamental constants follows directly from the above-named chaos? If in the one case we reason from fact or law to cause—why not also in the other, provided our conclusion is as legitimate?

Here astronomers will not fail to object that this last condition is *not* satisfied. We fully admit this; but beg them to remember that it has taken two centuries of labor to ensure this legitimacy to gravitation—that Newton did not leave gravitation as a mere suggestion (as such it had existence before him), but in his immortal *Principia* gave the necessary mechanical firmness to this hypothesis: how different has been the lot of Kant and Laplace's hypothesis! The first of these expounded it rather fancifully in connection with speculations on the inhabitants of distant globes;[8] the latter only gave a few bold and deep outlines of the nucleus of the theory! What would to-day be the estimation of gravitation if we, instead of Newton's *Principia*, only had a few of Hooke's sublime guesses, if these had only been considered by men like Fontenelle[9] instead of being investigated by Euler, the Bernoullis, Lagrange, Laplace, Gauss, Hansen, Plana, etc.?

Can anything be more unjust than exaltation of the hypothesis of Newton—this deservedly cherished subject of the master minds of two centuries—above the hypothesis of Kant–Laplace, which, being too early left even by its astronomical parent, has been ever since considered an outcast in the world, endangering the reputation of any one who would dare to touch it?

We will adopt this almost forlorn hypothesis as a *mere hypothesis*—we will patiently and carefully trace its bearings by means of as rigid an analysis as we can command in this most intricate field; we will minutely compare the results thus obtained with the actually observed state of things; and if we find *the correspondence between idea and phenomenon*, between analysis and observation, to be very close, we hope that those who have analysis more at their command than we, will pay as much attention to this high branch of astronomy as has been, and deservedly continues to be, bestowed on Newton's hypothesis of gravitation. If our feeble endeavors only succeed in making Kant–Laplace's *hypothesis* admitted as such among analysts, we shall have accomplished all we desire: for then this hypothesis will soon be considered as firmly established a principle as gravitation,[10] or as the fact of the rotation of the earth,[11] which latter—notwithstanding Foucault's pendulum and gyroscopes—still remains unproved by *ocular evidence*, all "demonstrations" being in fact inductive, reasonings with our senses. Thus, in the latter, we *see* the plane of the oscillations rotate, but *conclude* it to be the earth. It requires at least as much mental effort to apprehend its true bearing as the simple reference to the diurnal motion of sun, moon, and stars.

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[8] Allgemeine Naturgeschichte und Theorie des Himmels, 1755.

[9] Théorie des Tourbillons Cartésiens avec des réflections sur l'attraction (1752). Also his Entretiens sur la pluralité des mondes.

[10] Gravitation was always treated as a mere hypothesis by the Cartesians; the work of Fontenelle above cited offers the instance most generally known.

[11] The scientific prejudice existing against the nebular theory is perhaps as injurious as the religious prejudice once "*resisting*" the motion of the earth. See the amusing statement in the preface to the 'Dei massimi sistemi,' (ed. Padova, 1744,) that the "'Moto della Terra' non può né dee amettersi se non come pura ipotesi matematica, che serve a spiegare più agevolmente certi fenomeni." Also Galileo himself, in his Dialogue (*Opere compl.* Firenze. Vol. i, 1848, pp. 387 and 447).

§ 5. *How far the nebular theory accounts for the stability of the system.*

The theory of gravitation can never, therefore, account for the stability of the solar system. How far, then, does the nebular theory explain those great fundamental conditions of the system that ensure not only the harmony of the solar world but even make this harmony (almost) permanent?

In order to invite physicists and astronomers to the perusal of the following introduction, we will try to give a simple answer to this most important question.

I. *The plane of all planetary orbits must be nearly the same* (see § 1, 9, and § 3, 4, also § 10).

This theorem has been clearly seen by Kant and Laplace; it is the most immediate expression of the hypothesis. Mr. Trowbridge has recently pointed out[13] to us a very interesting consequence hereof, viz: the most distant planet *must move in the invariable plane;* and, indeed, the inclination of the orbit of Neptune is 1° 47′, that of the invariable plane 1° 41′.

II. *The direction of all planetary revolutions must be the same;* this obvious consequence of the hypothesis accounts for § 3, 8, and § 1, 10.

III. *The eccentricity of the planetary orbits must be very small*—accounting for observation, § 1, 11, and condition of stability, § 3, 5 and § 10.

This proposition has been deduced in general reasonings by Kant and Laplace; in the following we will try to give a demonstration of it.

IV. *The planetary distances are such that the successive planets were evolved at equal intervals of time;* or if $t=1, 2, 3, \ldots$ respectively for Mercury, Venus, Earth ...., then the distance is

$$a_t = \alpha + \beta . \gamma^t, \quad . \; . \; . \; . \; . \; . \quad (4)$$

where α, β, γ are constants, and t *the age of the planets* above that of Mercury. From this follows the condition of stability that *the periodic times are incommensurable* (§ 3, 1). Besides, it is seen that this law accounts for the empirical law of Bode (§ 1, 8).

The analytical demonstration of this law is one of the principal objects attempted in the present introduction. (See § 13.)

V. *The mass of the more distant planets is the greater*, on account of the greater space from which the material of the planet was condensed (space increasing according to IV). This is confirmed by the fact that the sum of the *four great exterior planets* is 480 times the mass of the earth, while the sum of the masses of the *four interior planets* is but twice the mass of the earth. Furthermore, *the mass must increase toward the sun*—as the density from which the rings were formed was greater nearer the center. This is confirmed, as the mass of Neptune is 25·6, the mass of Uranus but 14·5, so that the mean of the two most distant is 20; the mass of Saturn is *five* times as great (101·6), and the mass of Jupiter, again, three times greater (339·2). The minimum in the case of the mass of Uranus is evidently produced by the simultaneous influence of both the above principles.

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[13] On the Nebular Hypothesis, § 24; this Journal, November, 1864, page 355.

The mass of Jupiter is a maximum; it is so great that *the next following ring was broken up into fragments by the perturbing influence of so stupendous a mass*—thus originating the host of *asteroids*, and perhaps also the *meteorites*.

The very small mass of the interior planets as compared with the exterior ones is not astonishing, if we remember that the inter-planetary space between Jupiter and Saturn is to that between Venus and the Earth, as $10^3 - 5{\cdot}2^3$ to $1{\cdot}00^3 - {\cdot}72^3$, or as 860 to ·63, or nearly as 1300 to 1. The mass of Jupiter is to the mass of our earth as 340 : 1, thus giving us still some margin for an increase in density toward the center of the nebula.

Being as yet unable to give the precise *theoretical law of the masses*, we are obliged to make the above few suggestions, in order to show that the nebular hypothesis at least gives a general law of distribution of the planetary masses in conformity with § 1, 1, and § 3, 2. (Prof. Kirkwood takes a similar view of the asteroids; see this Journal, 1852, xiv, 214.)

VI. *The figure of the planets* (being a *condensed* vapor) *must be an oblate spheroid* of

VII. *Small ellipticity*, because

VIII. *The velocity of rotation is but small* (compare § 1, 2, 3, 7, and § 3, 7).

This last proposition is based upon the fact that the moment of rotation is but the *difference* between the moment of revolution of the exterior and interior part of the planetary ring.

Still, *the exact amount of this velocity*, as well as the period of rotation of the sun, has not yet been deduced from the nebular hypothesis; we have often attempted it, but as yet have not been able to solve this difficult problem.

Prof. Kirkwood has found[13] the empirical law of the velocity of rotation, *a law analogous to the third law of Kepler.* We have repeatedly arrived at expressions similar to (but not identical with) Kirkwood's law.

IX. *The Plane;* and

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[13] As above, this Journal, 1850, ix, 395.

X. *The direction of rotation* of the planets has been considered at variance with the nebular theory, ever since the discovery of the lunar system of Uranus. We believe that our analysis of this problem[14] shows that the rotation of Uranus and Neptune, both as to position of the axis and direction of motion affords a very interesting confirmation of the theory. See § 1, 5, 6.

XI. *The density* of the planets has also been considered as being adverse to the theory; but if, as necessary, the influence of the age is taken into account, it is found that the minimum density exhibited by Saturn is demanded by the theory.[15] Compare § 1, 4, and § 3, 6.

XII. *The number of satellites* was already shown, by Kant, to increase with the distance from the center of the nebula. Though not usually given as such, it nevertheless is a condition of stability of the system—at any rate it is conformable to observation (§ 1, 12).

The rings of Saturn are best considered as a host of satelloids, corresponding to the planetoids (and meteorites) of the solar world—thereby accounting for the excessive thinness and the subdivisions of the rings.[16]

In looking back upon the preceding account of the present aspect of the nebular theory, it will be seen

A. That the four great fundamental conditions of stability referring to *the system at large* are now satisfactorily deduced from the hypothesis of Kant–Laplace (I–IV above).

B. That the problem of the *mass* (V) and the *number of satellites* (XII), though not completely evolved, still is sufficiently comprehended to enable us to say that the analytical solution is *possible;* and

C. That the elements referring to the *single planets*, or rather their subordinate systems, are, with the exception of the *exact* law of rotation (VIII), fully deducible from the fundamental hypothesis of Kant and Laplace.

We see, then, that the *fundamental constants* of the solar system, which number about seven hundred (§ 1), exhibit very remarkable mutual dependencies (§ 2), which are such as ensure the permanence or stability of the system (§ 3), which Newton's law of gravitation cannot account for (§ 4). Though they offer a higher problem for theory than Kepler's laws, astronomers have hitherto been unwilling to recognize the analysis of the above conditions of stability as part of their science. Laplace, while instrumental in bringing to light the great laws of the stability of the system, independently reproduced the bold hypothesis of Kant, and though this has been most grievously neglected by analysts and astronomers, still it now affords us a full solution of the four great harmonies ensuring the permanency of the solar world, and also solves most, and at least indicates the solution of, all other problems relating to the harmony of the fundamental constants of the solar system.

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[14] On the density, rotation, and relative age of the planets; this Journal, 1864, xxxvii, 36, 48.

[15] As above, p. 49.

[16] On the density, etc.; this Journal, 1864, xxxvii, 54.

May we not hope that astronomers will begin to bestow on this theory some share of their labor?

§ 6. *The Hypothesis.*

We assume, with Kant and Laplace, as the primitive condition of the solar system, or as *nebula:*

The space of the solar system was filled with matter having a moment of rotation.

This *matter* is endowed with the same forces we know it to possess; a simple calculation will furthermore show that it was a highly rare vapor. Its chemical constitution we will leave out of consideration for the present; we therefore consider it as composed of the elements we know here on earth, many of which we *now* know to be found on the sun, and are probably also on the distant stars;[17] still there can be no doubt but that many more elements exist than we are acquainted with. Many of the spectral lines even of our own central star are irreducible to spectra of known elements. We therefore mean simply to say that at the above primitive period the *elements had been created.* I hope at some future time to publish an attempt at a *mechanical theory of the elementary bodies,* which has occupied my time for about ten years, and wherein I endeavor to show the physical properties of the known elements to be *definite functions* of their *atomic number and form.* Accordingly, there would yet remain a more primitive condition, the existence of the *one primitive matter* (Urstoff) which would be considered as the direct creation of the *one* God.

The *rotation* of the nebula is *not* to be thought regular, but simply amounting to a certain momentum. I have elsewhere[18] tried to show that such rotation may be considered as the *effect of a difference of any kind between the primitive forces of attraction and repulsion* wherewith we know matter to have been endowed.

If, therefore, these views should be well founded, we should have arrived at the grandest principle we can conceive of in the present state of our knowledge; we should be able to see how from *created matter alone the whole of the solar system has been developed;* we would be enabled to conceive the almighty *fiat* as

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[17] Rutherford, Astronomical Observations with the Spectroscope; this Journal, 1863, xxxv, 71. Above all, Bunsen's and Kirchhoff's memoirs on their great discovery.

[18] This Journal, 1864, xxxvii, 52.

one single act. How much such a theory would tend to elevate our conceptions of the great Author, we cannot here develop.

In the present paper I shall not go farther back in time than to the existence of the nebula of Kant and Laplace as above defined.[19]

§ 7. *Plateau's experiment.*

Before entering upon the analysis of the nebula, we must refer to the experimental evidence of the nebular theory afforded by the beautiful experiments of Plateau, detailed in his *Mémoire sur les phénomènes que présente une masse liquide libre et soustraite à l'action de la pésanteur*, Pt. I (Neuv. mém. de l'Acad. de Bruxelles, vol. xvi, 1843). His results are:

1. A liquid, subject only to the action of its molecular forces assumes the form of a perfect sphere (§ 2).

2. This globe is flattened at its poles, if subject to rotation (§ 10). Although, as he thinks, the molecular forces are not *identical* with those acting in the nebula, still the results ought to be analogous, if they are not identical.

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[19] It is perhaps not out of place here to give a synopsis of the different distinct *ages* that are characterized by a further individualization or a new direct creation according to the views above indicated.

Three (or four) direct acts of the Deity may be recognized, viz: the creation of *matter*, of *life*, of *mind* (and the *redemption*). The formation of the *elements* out of matter characterizes the *first age;* the formation of the *solar world*, with its planets, moons and central sun, the *second age;* while the *third age* beheld the development of our *earth* from a vaporous ball to its present shape; in the *fourth age, life* was created in the form of *plants* and *animals;* in the *fifth age*, mind, the investigating mind, was introduced by the creation of *man*, the *cephalized animal;* and in the *sixth*, redemption took place; while a *seventh age* will behold the destruction of the whole system, occasioned by the *extinction* of the solar body and the *resistance* of ether. (See this Journal, 1864, xxxvii, 56.)

To *every age* correspond two sciences: the first relates to the development—Whewell would call it the *Paletiology* of the age—while the second relates to the actually *existing product* of the development or creation of that age, i. e., *the* science. Thus we obtain the following general view of the natural sciences:

Age	1	2	3	4	5	(6)	(7)
	I. *Creation of* MATTER. Development of			I. Creation of *Life.*	III. Creation of *Mind.*	*Redemption.*	Destruction of the world.
	Elements.	*Solar system.*	*Earth.*				
Paletiology	Atomology	Planetology.	Geology	Paleontology.	Archeology.	Old Testament	
Science.	Physics, Chemistry.	Astronomy.	Geography.	Botany, Zoology	History.	New Testament	

These names have of course to be taken in their widest sense; thus geography stands for physical geography, meteorology, etc., and history comprehends not only political but also the intellectual history of the human race, thus including again *all* the sciences in their historical development. We see how "planetology" is allied to geology and astronomy.

3. If the rotation becomes sufficiently rapid, the *whole globe* is transformed into a *ring* in the equatorial plane (§§ 11–14) which, by continuing the rapid rotation of the large central disc, even loses its regularity, and separates into small masses which immediately assume globular forms. "But," continues he (§ 19), "this is not all; one or more of these spheres are always seen to take, at the instant of formation, a motion of rotation on their own axes, and this motion is almost always in the same direction as that of the ring."[20] He even found that still smaller globes were formed.

4. If a small disc is put into very rapid rotation, a ring is formed, while a part of the original globe remains on the axis (§ 21), so that Plateau rightly concludes with saying (§ 27) that most of the phenomena of the relative configuration of the heavenly bodies have been reproduced by him on a small scale.[21]

[*To be continued.*]

ART. VIII.—*Contributions to Chemistry from the Laboratory of the Lawrence Scientific School;* by WOLCOTT GIBBS, M.D., Rumford Professor in Harvard University.—No. 2.

§ 1.

On the separation of chromium from aluminum, iron, manganese, cobalt, nickel, zinc and magnesium.—Sesquioxyd of chromium in an alkaline solution is readily oxydized to chromic acid by means of chlorine, bromine or deutoxyd of lead. When chlorine or bromine are employed as oxydizing agents, the alkaline solution may be neutralized by acetic acid, after the oxydation is complete, and the chromic acid may then be precipitated by acetate of barium, when the solution is free from sulphuric acid, and directly weighed in the form of chromate of barium. In place of free alkali it will be found in practice very much more convenient to employ acetate of sodium or potassium. When a solution of sesquioxyd of chromium is rendered nearly neutral by a solution of carbonate of sodium, and acetate of sodium is added in excess, a current of chlorine gas, or a solution of chlorine water, readily converts the whole of the chromium present into chromic acid, especially when the solution is hot, and when it is kept nearly neutral by occasional addition of carbonate of sodium. The excess of chlorine is easily expelled by boiling, after which, in the presence of bases not precipitated by ammonia, the chromic acid may be precipitated by acetate of lead, or acetate of barium, and weighed in the form of chromate, provided, of course, that no sulphuric acid is present. When sulphate and chromate of barium are thrown down together, the chromic acid may be reduced to sesquioxyd by boiling with concentrated chlorhydric acid and alcohol, after which the barium may be precipitated by sulphuric acid, and the sesquioxyd of chromium thrown down in the filtrate by boiling with ammonia in the usual manner. As the reduction of chromate of barium by means of chlorhydric acid and alcohol does not take place very readily, it is better to boil the chromate with an excess of carbonate of potassium or sodium, to filter off the carbonate of barium, and determine the chromic acid by means of nitrate of suboxyd of mercury, or by reduction to oxyd of chromium and precipitation with ammonia in the usual manner.

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[20] Of course; for in this oil the density is the same. See our article, this Journal, 1864, xxxvii, 51, $\delta=0$.

[21] Malgré la difference des lois que suivent les forces attractives dans ce cas et celui des grandes masses planétaires, nous avons vu se produire, en petit, une représentation frappante de la pluspart des phénomènes de configuration relatifs aux corps célestes.

When aluminum and iron are to be separated from chromium by this process, the two oxyds may be precipitated together by simply boiling, the solution after the complete oxydation of the chromium to chromic acid, in the presence of excess of acetate of sodium. It is more convenient and equally accurate to neutralize the solution with ammonia, separate the alumina and sesquioxyd of iron by filtration, and determine the chromium in the filtrate by reduction and precipitation with ammonia.

When the oxyds of calcium, magnesium, zinc, nickel, cobalt and manganese are present in a solution containing sesquioxyd of chromium, it is best to oxydize the chromium to chromic acid as above, and then to precipitate with acetate of lead or barium.

I have stated in a former paper that chromic iron ore may be completely resolved by fusion with fluohydrate of fluorid of potassium. In this and in all similar applications of the fluohydrate, it is best to evaporate the finely pulverized mineral to dryness with a concentrated solution of the salt. On subsequently heating to low redness, the resolution of the mineral is effected with the utmost ease, a portion of the chromium being usually oxydized to chromic acid by the oxygen of the air. After expelling the fluorine by heating the fused mass with sulphuric acid, the remaining mass may be treated with acetate of sodium and chlorine in the manner already pointed out, the iron and aluminum separated by boiling, and the chromic and sulphuric acids precipitated by acetate of barium, after which the chromium may be determined as above.

In precipitating chromic acid by means of nitrate of suboxyd of mercury, hot solutions must not be employed, as a small portion of chromic acid is always reduced to sesquioxyd of chromium. The precipitated chromate should be allowed to stand some hours before filtering. In general, the precipitation by acetate of lead or acetate of barium is to be preferred even when the resulting chromate is to be weighed as such.

§ 2.

On the employment of acetate of sodium for the separation of iron and aluminum from other bases.—The facility with which iron and aluminum are precipitated from neutral solutions of the sesquioxyd, by boiling with acetate of potassium or sodium, has led to frequent analytical applications, though the method is not so generally employed as it deserves. Mr. C. F. Atkinson has devoted much time to a careful study of the subject, and has arrived at the following results, which appear to me worthy of attention. The sesquioxyds of iron and aluminum may be perfectly separated from the protoxyds of manganese, cobalt, nickel, zinc, magnesium and calcium, and from sesquioxyd of uranium, by boiling the neutral or nearly neutral solutions with acetate of sodium, provided that the following precautions are observed. The solutions from which the sesquioxyds are to be precipitated must be dilute: half a liter of the solution should not contain more than one gram of either sesquioxyd or of the two when both are present. The quantity of acetate of sodium should be sufficient to convert by double decomposition all the bases present into neutral acetates. The acetate should be added to the metallic solution when cold and the whole should then be heated together and boiled for a short time. It is not necessary to filter upon a water-bath funnel, but the beaker containing the solution should be kept nearly at the boiling point during filtration, and a ribbed filter should be employed. In all cases it is best to add a few drops of free acetic acid to the solution, to prevent the formation of basic acetates of the protoxyds. This is especially necessary in separating iron and aluminum from zinc and nickel.

Finally, it is best, whenever it is possible, to have all the bases present in the form of chlorids. The iron and alumina upon the filter in the form of basic acetates must, whenever an absolutely complete separation is necessary, be redissolved in chlorhydric acid and again precipitated by boiling with the acetate after rendering the solutions nearly neutral by means of carbonate of sodium. In this manner only is it possible to separate the last traces of the stronger bases. Finally, the basic salts of the iron and aluminum, after washing, must be redissolved in chlorhydric acid, and precipitated by boiling with ammonia in the usual manner, to free them completely from alkali. The precaution of a second treatment with acetate of sodium is more necessary with alumina than with sesquioxyd of iron alone. It is scarcely worth the trouble in the separation of iron from calcium and magnesium.

According to my own observations, the sesquioxyds of iron and aluminum cannot be separated from sesquioxyd of chromium by boiling with acetate of sodium, although the last mentioned oxyd is not precipitated when alone in solution. In this case it is necessary to oxydize the chromium to chromic acid by chlorine in the manner already pointed out.

§ 3.

On the separation of manganese from cobalt, nickel and zinc.—Schiel's method of separating manganese from the alkaline earths by adding acetate of sodium to the mixed solutions, heating the liquid gently and then passing chlorine through it so as to convert the manganese into a hydrate of the sesquioxyd, is better than that formerly given by myself in which peroxyd of lead is used as the oxydizing agent. With respect to Schiel's method, however, it must be remarked that it cannot be employed to separate manganese from nickel or cobalt, because both of these metals are converted into higher oxyds under the same circumstances. Nickel may, as Popp has recently shown, be completely precipitated as a deep blue hyperoxyd, while as I have myself observed, cobalt is also oxydized, though not precipitated, unless the solution is boiled with an alkaline carbonate. In separating manganese from zinc, calcium or magnesium, I have repeatedly found that a second treatment is necessary in order to obtain a perfect separation. This second treatment may be neglected in separating manganese from calcium and magnesium, but not in separating it from zinc, although the addition of a few drops of free acetic acid renders the process more exact.

Though the method of separating manganese from other bases by means of peroxyd of lead, which I formerly proposed, will hardly be used in future, now that we are in possession of more convenient processes, it will still be of some interest to chemists to know the precise nature of the insoluble black compound which is formed when peroxyd of lead, PbO_2, is digested or boiled with an excess of a solution of chlorid or nitrate of manganese and afterward thoroughly washed. An analysis of this body, made some years since in my laboratory by my lamented friend and former pupil, Mr. Theodore Parkman, gave the following results:

		Anhydrous.	Theory.
Manganese, - -	35·10	37·96	37·53
Lead, - - -	32·49	35·13	35·26
Oxygen, - - -	24·87	26·90	27·20
Water, - - -	7·52	....	....
	100·00	100·00	100·00

Neglecting the water, which may have been, in part at least, mechanically combined, and which amounts to between three and four equivalents, we have the formula, MnO_2+4PbO_2, as the simplest expression of the results of the analysis.

A simple and perfectly satisfactory process for separating manganese from cobalt, nickel, and zinc, is the following: To the neutral or nearly neutral solution of the chlorids, acetate of sodium is to be added in excess together with a few drops of free acetic acid. The solution is then to be boiled, and a rapid current of sulphydric acid gas passed through it while boiling and continued for half an hour. Every trace of cobalt, nickel and zinc, is precipitated in the form of sulphid, while the whole of the manganese remains in solution. The precipitate is to be thrown upon a ribbed filter and quickly washed with cold water saturated with sulphydric acid gas. It is easily washed, and though the sulphids of cobalt and nickel precipitated in this manner are far more easily oxydized than when precipitated by boiling sulphid of sodium from boiling solutions, they will be found to present no difficulty as regards oxydation upon the filter. Manganese may then be determined in the filtrate by boiling with chlorhydric acid and precipitating in the usual manner with carbonate of sodium. The mixed sulphids upon the filter, supposing for the sake of generality that all three are present, are to be dissolved in chlorhydric acid and the metals converted into double cyanids by means of an excess of cyanid of potassium, after which the zinc may be precipitated by means of sulphid of sodium, as recommended by Wöhler.

When perfectly pure cyanid of potassium is not at hand, the following process will be found particularly convenient. Acetate of sodium is to be added to the solution of the mixed chlorids, after which the vapor of cyanhydric acid generated in a flask from sulphuric acid and ferrocyanid of potassium is to be passed directly into the solution. Cyanid of zinc is immediately precipitated more or less completely as a perfectly white powder. A solution of sulphid of sodium is then to be added as long as a precipitate is formed, after which the sulphid of zinc is to be separated by filtration. Cobalt and nickel remain in solution as double cyanids. The same process may be used to separate manganese from cobalt and nickel, sulphid of sodium throwing down under these circumstances a pure flesh red precipitate. It is easy to see that zinc and manganese together may be separated from cobalt and nickel by the same process and at one operation. No cyanid of manganese appears to be formed when cyanhydric acid is passed into a solution containing a salt of manganese, acetic acid, and acetate of sodium.

I have stated in a former paper[1] that the sulphids of nickel and cobalt are thrown down from boiling solutions by a boiling solution of sulphid of sodium in an insoluble form, so that in fact even strong chlorhydric acid scarcely exerts upon them an appreciable action. This process has been applied to the separation of cobalt and nickel from zinc and manganese by my excellent assistant, Mr. Maurice Perkins, and gives results which are very satisfactory, especially for qualitative purposes, the sulphids of manganese and zinc precipitated under the same circumstances being readily soluble, even in dilute acid. The process is now substituted in this laboratory for that given in most of the recent works on qualitative analysis, and has been repeatedly tested with satisfactory results.

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[1] This Journal, March, 1864.

§ 4.

On the separation of cobalt from nickel.—A method of separating these metals, given some years since by Liebig,[4] consists in boiling the mixed double cyanids of nickel and potassium and cobalt and potassium with oxyd of mercury. Oxyd of nickel is precipitated, while an equivalent quantity of mercury is dissolved as cyanid. The method certainly gives good results but is not free from objection. Long boiling is necessary before the precipitation is complete, and it is difficult to prevent "bumping" during ebullition. The excess of oxyd of mercury must be separated from the oxyd of nickel by a special operation, and the nickel afterwards again precipitated by caustic alkali.

These inconveniences may be completely avoided by employing, instead of the oxyd alone, a solution of the oxyd in the cyanid of mercury. When this solution is added to a hot solution of the double cyanid of nickel and potassium, the whole of the nickel is immediately thrown down as a pale green hydrate of the protoxyd. Under the same circumstances cobalt is not precipitated from the double cyanid of cobalt and potassium. Mr. W. N. Hill, who has repeatedly employed this method and carefully tested it, has found that the separation effected is complete. No cobalt can be detected in the precipitated oxyd of nickel by the blowpipe, nor can nickel be detected in the cobalt (finally separated as oxyd) by Plattner's process with the gold bead. The solution of oxyd of mercury is easily obtained by boiling the oxyd with a strong solution of the cyanid, and filtering. According to Kühn[5] the cyanid formed in this manner has the formula $HgCy+3HgO$. The hydrated oxyd of nickel precipitated may be filtered off, washed, dried, ignited and weighed. The cobalt is more readily and conveniently determined by difference, when, as is always possible, the two metals have been weighed together as sulphates. I am not prepared to say that this modification of Liebig's method of separating nickel and cobalt gives better results than Stromeyer's process by means of nitrite of potassium, but it is at least very much more convenient and requires much less time. The complete precipitation of cobalt in the form of $Co_2O_3,2NO_3+3KONO_3$ usually requires at least forty-eight hours, and rarely succeeds perfectly except in experienced hands.

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[4] Ann. der Chemie und Pharmacie, lxv, 244.

[5] Berzelius, Lehrbuch der Chemie, iii, 872.

§ 5.

On the separation of uranium from zinc, cobalt and nickel.—The method which I have given above for the separation of manganese from cobalt, zinc, and nickel, by precipitating the sulphids of the three last named metals, by means of sulphydric acid gas, from a boiling solution of the acetates, may be also used, according to the carefully conducted experiments of Mr. Perkins, for the separation of uranium from the same metals. The process is in all respects the same, and requires, therefore, no further description. It will be found much simpler and more convenient than that described by Rose, by means of carbonate of barium.

§ 6.

On the electrolytic precipitation of copper and nickel as a method of analysis.—The precipitation of copper by zinc, in a platinum vessel, with the precautions recommended by Fresenius, leaves nothing to be desired, so far as accuracy, ease and rapidity of execution are concerned. The method labors, however, under a single disadvantage: the introduction of zinc renders it difficult, or at least inconvenient, to determine with accuracy other elements which may be present with the copper. It has occurred to me that this difficulty might be overcome, the principle of the method being still retained, by precipitating the copper by electrolysis with a separate rheomotor. The following numerical results, which are due to Mr. E. V. M'Candless, will satisfactorily show the advantages of the method for the particular cases in which it is desirable to employ it. The copper was, in each case, in the form of sulphate; the deposition took place in a small platinum capsule, which was made to form the negative electrode of a Bunsen's battery of one or two cells, in rather feeble action. The positive electrode consisted of a stout platinum wire, plunged into the surface of the solution of copper at its center. The following table gives the results obtained in the analysis of pure sulphate of copper:

Number.	Salt taken.	Copper found.	Percentage.
I.	1·2375	0·3145	25·41
II.	0·4235	0·1075	25·38
III.	1·0640	0·2705	25·42
IV.	1·3580	0·3440	25·33
V.	0·5665	0·1450	25·59
VI.	0·4735	0·1205	25·48

In seven determinations of copper in the alloy of copper and nickel employed by the government for small coins, the following results were obtained:

Number.	Weight of alloy.	Copper.	Percentage.
I.	0·4160	0·3640	87·50
II.	0·6180	0·5410	87·54
III.	0·4600	0·4090	88·91
IV.	0·5120	0·4481	87·51
V.	0·4220	0·3693	87·51
VI.	0·2525	0·2225	88·11
VII.	0·3705	0·3255	87·85

The percentage of copper required by the formula Cu O, SO_3+5HO is 25·42, while the government standard alloy of nickel and copper contains 87·50 per cent of copper. The time required for precipitation varied from one to three hours, the separation of the last traces of copper being in each case determined by testing a drop of the liquid upon a porcelain plate with sulphuretted hydrogen water. The copper, after precipitation, was washed with distilled water, dried in vacuo over sulphuric acid, and weighed with the platinum vessel. The only precaution necessary is to regulate the strength of the current so that the copper may be precipitated as a compact and bright metallic coating, and to dry as quickly as possible. When the copper is thrown down in a spongy condition, it not only oxydizes rapidly, but it is impossible to wash out the last traces of foreign matter contained in the solution. This is well shown by number III and number VI of the second series, in both of which cases the copper was precipitated too rapidly. The solution from which the copper has been deposited contains the other elements present in the original substance. It may be easily poured off without loss, and the washings added.

It appears at least probable that nickel may be determined by electrolysis in the same manner as copper, the solution employed being the ammoniacal sulphate with excess of free ammonia. Mr. M'Candless obtained in two determinations in a commercial sample 91·36 and 91·60 per cent nickel. In both cases the nickel was thrown down completely as a bright coherent metallic coating upon the platinum.

Cambridge, Oct. 1st, 1864.

ART. IX.—*Note on the Planetary Distances;* by Prof. DANIEL KIRKWOOD, Indiana State University.

To obviate any misapprehension that may have existed in regard to the order of planetary distances proposed in this Journal for July, 1864, p. 12, it may be proper to state that by the term "radii of gyration of the primitive rings," was meant the radii of gyration of homogeneous circular rings, or, in other words, the radii of the circumferences which bisect the circular intervals between the orbits of Neptune and Uranus, Saturn and Jupiter, &c. The formula was given as empirical, in the hope that it might *suggest* a true law of nature, if not found itself in strict harmony with observation.

In the article referred to, the writer suggested the hypothesis of the contemporaneous formation of the different members of our planetary system. Some additional considerations bearing on this subject may not be destitute of interest.

The radius of gyration of the sun, regarded as a homogeneous sphere, is 0·002936; the unit being the mean distance of the sun from the earth. If we designate by K the principle radius of gyration when the rotation period was the same as Neptune's orbital revolution, we shall have

$$25^{d}\cdot325 : 60126^{d}\cdot7 :: (0\cdot002936)^2 : K^2 \text{; whence } K = 0\cdot143.$$

But the radius of gyration of a *homogeneous* sphere filling Neptune's orbit is 18·974. Hence we infer that before the formation of the planets the condensation of the solar spheroid had advanced much more rapidly about the center than toward the surface.[1] The consequent tendency to unequal velocities of rotation would produce a divellent force, thus separating the nucleus from the outer parts, and also breaking up these exterior portions into distinct zones or annuli. As subsequent condensation would, to some extent, increase the rotary velocity of the nucleus, the primitive circle of division between the latter and the surrounding nebulous mass must have been somewhat exterior to the present circle of equilibrium between the centripetal and centrifugal forces. The points in the revolving nebulous matter at which the planetary formations would originate, were determined by certain conditions which in the present state of our knowledge we are unable to define. It is not, however, an improbable assumption that the intervals between the orbits of such nuclei would be arranged in regular order. This, in fact, is found to be the case, at least approximately, in regard to Jupiter's satellites. The differences between the radii of the orbits, commencing with the greatest, are,

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[1] **This fact, together with its cause, as the writer has recently learned, was stated by Prof. Stephen Alexander, at the Montreal Meeting of the Am. Assoc. for the Adv. of Sci.**

11·64811, 5·72677, and 3·57494.

But while the first is almost exactly double the second, the second is less than double the third. In other words, as in the case of Mercury, the theoretical distance of the innermost satellite is somewhat greater than the actual distance. Now, if the *original* intervals constituted an exact geometrical progression, can a probable reason be given for the *present* deviation? We reply (1) that in the case of the primary system considerable masses of meteoric matter are believed to be still revolving within the earth's orbit; portions of which are exterior to Mercury, and some even beyond the orbit of Venus; (2) that if the Nebular Hypothesis be true, the primitive constitution of the secondary systems was probably similar; (3) that in any system an increase of mass in the central body would diminish the orbits of the satellites; and (4) that the contraction of meteoric rings until portions originally *exterior* to a given orbit had become *interior*, would obviously have the same effect. In the manner thus indicated, the distance of the innermost satellite may have been diminished, and thus the original interval between the first and second orbits increased. The same explanation applies to Mercury.[2]

With an interval = 11·64811 between the third and fourth satellites, and a ratio = $\frac{1}{2}$, we find 3·70213 as the limit at which the central body separated from the exterior mass. This is a little beyond the present circle of equilibrium between the centrifugal and the centripetal forces, the radius of this circle being 2·299.

THE SATURNIAN SYSTEM.

In the primary system[3] and also in that of Jupiter, we have found that the original circle of separation between the nucleus and the surrounding mass was a little exterior to the present circle of equilibrium between the two central forces. This is what ought to be expected, as the contraction of the nucleus would accelerate the rotation. For Saturn, however, we found the primitive circle of equilibrium *less* than the present;[4] but in this calculation no allowance was made for the contraction of the orbits of the innermost satellites. If the radius of gyration for Mimas and Enceladus was diminished in a ratio equal to the mean of those obtained for Mercury and the innermost satellite of Jupiter, it was originally 3·8720. Adopting this value and interpolating the two missing terms, we have the following elements conforming to our hypothesis:

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[2] This explanation, it must be confessed, is apparently inconsistent with the conclusions of Leverrier in regard to Mercury's motion. The distinguished director of the Paris Observatory "has found that Mercury's mean motion has gone on diminishing; as if the planet were, in the progress of his revolutions, receding further from the sun. This is explained, if we suppose that there is, in the region of Mercury, a resisting medium which moves round the Sun in the same direction as the Planets move."—*Whewell's Hist. of the Ind. Sci.*, 3rd ed., vol. i, p. 560.—Perhaps no satisfactory determination of the facts can be reached without long continued observations.

[3] This Journal for July 1864, p. 13.

[4] Ib., p. 15.

SATELLITES OF SATURN.

Names.		Distances.	Rad. of Gyr. of Intervals.	Intervals.
I.	Japetus,	64·359	54·9764	
	———	43·6203		32·0070
II.	Hyperion,	25·029	22·9694	
	Titan,	20·706		12·4154
III.	———	11·958	10·5540	
	Rhea,	8·932		4·8150
IV.	Dione,	6·399	5·7390	
	Tethys,	4·9926		1·8670
V.	Enceladus,	4·0819	3·8720	&c.
	Mimas,	3·1408	&c.	
		&c.		0·0000
			Limit=2·6862	

The ratio of the ascending series of intervals is 2·578+. These ratios in the systems of Jupiter and Saturn are to each other inversely as the orbital velocities of the two planets. The distance from the center of Saturn at which a satellite would complete its orbital revolution in the present period of the planet's rotation is 2·0075. The distance at which the nucleus originally separated from the surrounding mass has been found (theoretically) somewhat greater, as it ought to be, viz., 2·6862.

If, then, the arrangements of the Saturnian System should not be admitted as confirmatory of the empirical order of distances, we may at least conclude that it is not incompatible with it. The *exact* coincidences are of course produced by the interpolation of the two terms. With these, our formula gives three equations with three unknown quantities, and, consequently, whatever the distances of the known satellites, the roots of these equations, whether real or imaginary, must, in an algebraic sense, satisfy the conditions. At the same time it is easy to perceive that these algebraic results *might be* decidedly unfavorable to the proposed hypothesis.

The tendency in a rotating nebula to unequal angular velocities, resulting from the increased rapidity of condensation from the equator toward the center, may, perhaps, also account for the phenomena of spiral nebulæ. If, in a contracting mass of vapor, a free motion of the particles among themselves be established before the centrifugal force becomes equal to the centripetal, a spiral convergence, like that of No. 51 in Messier's catalogue, would naturally ensue.

Finally, if the original constitution of the Solar and Saturnian Systems was such as we have supposed, can any probable reason be given why the satellites of Jupiter should be found an exception?[*] It may be worthy of remark that if these bodies, or the rings from which they were derived, were originally double, the proximity of the members was such that they might be brought into collision by perturbation, while in the gaseous state. The ratio, moreover, of the ascending series of intervals is considerably less than in the case of Saturn, and much less than in the primary system.

ART. X.—*Caricography;* by Prof. C. DEWEY.

(Continued from vol. xxxv, p. 60, 1863.)

No. 281. *Carex conjuncta*, Boott, Illust. No. 282.
—— *vulpina*, Sullivant, Carey, Dewey.

Spica composita oblonga; spiculis ovatis sessilibus superné staminiferis et aggregatis, inferné laxis sæpe sub-ramosis et sub-remotis, bracteatis; fructibus *distigmaticis* ovatis acutis brevirostratis bidentatis stipitatis et interdum sub-cordatis margine subscabris divergentibus, squamam ovatam acutam paulo superantibus; spica plantaque pallido-viridi; culmo 1–3 pedali late-triquetri, cum foliis sub-radicalibus et longis.

This form was discovered some years since in Ohio, and named by Mr. Sullivant, after the European plant, C. vulpina of Linnæus. So it was held to be by Mr. Carey and others, and under that name described in this Journal, vol. viii, p. 348, 1849. Dr. Boott, however, thought it differed from C. vulpina, and gave it the name above. The figures in the Illustrations, No. 282, were taken from plants collected near Columbus, Ohio, and fully resemble those from Menard Co., Ill., now before me.

In the Illustrations, Dr. Boott gave the following particulars in which this plant differs from the European *C. vulpina*, viz: a very "acute-angled and flaccid culm," sheaths of the leaves transversely corrugated, spike more lax and pale, inflorescence of a pallid color, and its general appearance and habit. These or similar characters show the great difference between C. cephalophora and C. Leavenworthii, and doubtless should be considered important and distinguishing, even when, as in this case, the characters taken from the inflorescence are so nearly the same.

This species resembles C. sparganioides, but its *nerved* fruit separates it; as does the form of the fruit from C. stipata. The specimens of C. vulpina, *L.*, from England, Germany, and Sweden, have an inflorescence more *compact* and *reddish brown;* and the plant generally has a *deeper green*, and is also *stouter.*

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[*] For some suggestions in regard to Mercury, see this Jour. for July, 1864. p. 18.

282. *C. glabra*, Boott, Illust. No. 229.

Spicis distinctis cylindraceis pedunculatis; terminali staminifera, interdum ad apicen vel basin pistillifera, vel in medio; spicis pistilliferis, 3–4, sub-laxifloris, bracteatis, infernè longo-pedunculatis et nutantibus; fructibus *tristigmaticis* ovalibus sub-inflatis et infernè teretibus, supernè conicis brevi-rostratis, bidentatis glabris nervosis, squamam oblongam sub-acutam vel lanceolatam multo superantibus; planta pallida et glabra.

Culm 1½ foot high, slender, erect, leafy toward the root; staminate spike slender, sometimes with a few fruit at the apex, in the middle or at the base; pistillate spikes 3–4, cylindric, slender, rather loose flowered, on slender peduncles with sheathing bracts which equal the culm nearly; stigmas 3; fruit oval, tapering below and conic above, smooth and glabrous, nearly twice longer than the oblong acutish scale which is white on the margin and green on the back: whole plant light green.

This plant is the well-known *glabrous* form of C. flexuosa, *Schk.*, the C. debilis, *Mx.*, and blended with it till Dr. Boott separated them in 1860. More than 40 years ago it was in our country commonly labelled from fig. 124, Schk.

283. *C. Magellanica*, Lam. 1789, and Schk., 1802. Boott, No. 199.
C. limosa, var. irrigua, Wahl. 1803; Dewey, 1826.
C. paupercula, Mx. 1803, and Torrey, 1843, and Boott.
C. irrigua, Smith, English Botany, 1845.

Spicis pedunculatis cylindraceis vel brevi-oblongis ferrugineis; terminali vulgo staminifera brevi, raro apicen pistillifera; spicis pistilliferis 1–3, subremotis recurvo-pedunculatis, bracteatis, laxifloris, interdum basin stameniferis; fructibus, *tristigmaticis*, longo-rotundis obovatis sub-triquetris vix rostratis ore integris stipitatis, squama ovato-oblonga acuta vel lanceolata brevioribus; culmis, foliis, bracteisque glaucis.

The stamens at the base of the fertile spikes have not been noticed by Torrey, Carey, and others generally who have collected the plant. They seem to me to be less common than they appeared to Dr. Boott, though I find them, and they were found by Schk. The American plant seems to be a larger and stronger plant than the European, even to the European botanists.

Though the resemblance of the figure of C. Magellanica in Schk. to C. limosa may have been noticed, it was reserved for Dr. Boott to prove, by examination of the specimens of Lamarck, their identity with common C. irrigua of Europe and North America. Lamarck's name, being the oldest, must be the accepted name of this species. It was briefly described in this

Journal, volume x, page 42, 1826, under the above name of Wahlenberg.

In cold marshes in the Northern and Western States, and Canada.

284. *C. rariflora*, Smith, Eng. Bot., 1845.
C. limosa, var. rariflora, Wahl., 1803 and Schk., 1812.

Spica terminali staminifera brevi; spicis pistilliferis 1–3, sæpe 2, linearibus brevibus laxifloris pedunculatis et nutantibus, bracteatis vix remotis; fructibus, *tristigmaticis*, ovatis oblongis vel ellipticis triquetris subcompressis obtusis apiculatis ore integris, squamam ovatam obtusam vel acutam sub-fuscam subæquantibus; planta glauca brevi et gracili.

White Mountains, N. H., on borders of Blue Pond—Dr. Barratt: northern parts of Europe and America. As a variety of C. limosa, this was very briefly described in this Journal with the preceding species.

In the *Summa* (p. 233, 1846) of the acute Fries, he has spoken of this C. rariflora, "quasi forma reducta," *as if a reduced form*, "scarcely a span high," of his preceding species, C. Stygia; but C. Stygia seems more nearly related to C. limosa, *L.*, than is C. rariflora. Hence, we find that Lang, in his *Scandinavian Carices*, admits both as proper species; and he will doubtless be approved by others.

Note. The following rare variety of C. limosa *L.*, was discovered the last summer by Rev. John A. Paine, of Utica, who has been actively and successfully engaged in looking up the *rare* as well as the common plants.

C. limosa, L., var. *Painei*, Dew.

Culm with the terminal spike staminate, and one pistillate spike of the common size and form on a slender or hair-like peduncle, from five to nine inches long, rising from near the root. In one instance there was a single pistillate spike on the culm and near the staminate spike, and the long radical pedunculate spike as before. The common form and the variety here described were found growing on different culms from the same root.

This is a very curious variety, and seems not to have been discovered before. Dr. Boott says of the lowest pistillate spike of C. Magellanica, "rarius subradicalis;" but this is on C. limosa and is truly radical, while he has figured that as subradical and short pedunculate instead of very long.

285. *C. mirata*, Dew.

Spicis 3–5, longo-cylindraceis inclusè pedunculatis longe-folioso-bracteatis; spicis staminiferis 1–3, sæpe 2, approximatis, interdum ad basin vel erga apicen pauco fructiferis, cum glumis longis arctis attenuatis scabro-subulatis; spicis pistilliferis 1–2, laxifloris suberectis sub-remotis ad apicen vulgo staminiferis; fructibus parvi-ovatis longo-conicis vel lanceolatis vix inflatis nervosis vel striatis longo-stipitatis divergentibus rostratis, rostro profundi-fisso bicuspidato interdum bifurcato vel bidentato; glumis fructiferis lineari-lanceolatis scabro-subulatis, fructu superno spicæ brevioribus, fructum inferiorum æquantibus, atque fructus infimos plus duplo superantibus: culmo superne scabro, inferne obtusi triquetri et lævi; foliis bracteisque nodosis et margine scabris.

Culm 15–20 inches high, stout and stiff, triquetrous, obtuse-angled and smooth below the lowest bract, but rough above it, with long and broad leaves from the base and long leaf-like bracts, longer than the culm, and the lower the longer, both net-veined and very scabrous on the edges; staminate spikes 1–3, commonly 2, long-cylindric, near but the lower more remote, covered with long narrow bristle-form glumes attenuate and rough-subulate, with a few fruit at their base or toward the summit; pistillate spikes 1–2, often 1, long cylindric, suberect, exsert-pedunculate, at their summit staminiferous: stigmas three; fruit round and small ovate, long-conic, diverging, scarcely inflated, long-stipitate, with a beak deeply bifid, sometimes bicuspidate or bifurcate or bidentate with scales ovate lanceolate, rough-subulate, shorter than the fruit at the upper part of the spikes, equalling the fruit along the lower, and sometimes several, four to ten, of the lowest scales even twice as long as the fruit. Plant pale green.

In Greece, eleven miles west of Rochester and six south of Lake Ontario, in 1829, by Dr. S. B. Bradley. It was not named for a long time, as its anomalous characters made more specimens desirable. But no others have been discovered in the vicinity of this locality, which the clearing of the forest destroyed. In Belleville, Canada West, in 1863–4 by John Macoun, Esq. One form, sent by him, accords with the above description, especially in the varieties of the fruit, of the rostrum, and the very long scale of the fruit at the base of the lower pistillate spike.

Dr. Boott in Illust. No. 58, refers this species to C. aristata, *R. Brown*, by a mistaken[1] reference to my description of C. aris-

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[1] His reference, "C. mirata, *Dewey*, Sill. Jour., xxvii, 240, v, 49, 48; Wood's Bot.," should have been *C. aristata*, Dewey, *Sill. J.*, xxviii, 240, vol. xlix, 48 p.; *C. mirata*, Dewey, *Wood's Bot.* Not only was the volume wrong, but *mirata* was unintentionally substituted for *aristata* by Dr. Boott, which changed the whole subject. Indeed the full description of C. aristata, Sill. J., vol. xxviii, 1835, referred to by Dr. Boott, was printed years before I had heard of C. mirata, proving that the description was of C. aristata evidently, and of no other, it may properly be added, as Dr. Boott implies in 1858.

If the "Note," Sill. Jour., xlix, 48, 1845, is noticed with care, it is obvious a *mistake* is made, because the last sentence *denies* what the preceding *asserts*. It will tata, which has no connection with any other specimens, and by that self-destructive "Note," vol. xlviii, p. 49.

When No. 58 was published, Dr. Boott had not seen any specimen of C. mirata; but since that time he has seen two specimens of it, which I forwarded to him, and one of which was a present to him from Dr. Bradley. Circumstances led me to suppose he had changed his opinion in consequence; but his death, so unfortunate for this science, has prevented the publication of the 4th Part, which was to contain it and was known to be nearly ready for publication. What measures can be taken to secure its being printed and circulated?

Another form, judged by Mr. Macoun to belong to this species, has been provisionally named

C. mirata, var. minor, *Dew.*

Staminate spikes 2–3, often 3, long and short, and scales slightly larger; pistillate spikes 1–2, some shorter with stamens at the apex, or below it, or both, as in C. mirata, with fruit slightly more inflated, and its scales generally shorter than the fruit; plant older, with brown spikes and scales.

In Belleville, C. W.; Mr. Macoun.

Note. C. rostrata, *Mx.*, commonly obtained from ponds near the base of the White Mts. N. H., was found most abundant by Rev. Mr. Paine of Utica, in August last, at a pond or small lake near the well known Bald Rock or Mt. in the N. E. part of Herkimer Co., at the west foot of the Adirondack Mountains. Muhlenberg had not seen it, as he referred this name, *rostrata*, to his C. tentaculata. It was not recognized from the time of Michaux to 1840, when it was described in this Journal, vol. xxxix, p. 52.

C. lenticularis, *Mx.*, was found also by Mr. Paine along the chain of the "Eight Lakes," in Herkimer Co.

C. Houghtonii, *Torr.*, has been found, the last season, 60 miles north of Belleville, by John Macoun, Esq., an active and discriminating botanist. The same species has been found, the last two seasons, by Rev. Mr. Blake, of Gilmanton, N. H., and in Milford, north of Bangor, Me. With great pertinence was it named by Dr. Torrey.

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be rectified by the insertion of C. aristata, so as to read, "For description and figure of C. aristata, *R. Br.*, refer to vol. xxviii, p. 240, of this Journal; tab. v, fig. 67. This species the fig. 67 finely represents, while it is very unlike C. mirata, as is that description in vol. xxviii. So certain is it, that the "Note" assumes the great difference between C. aristata and C. mirata, instead of uniting them.

ART. XI.—*On the Action of Ozone upon Insensitive Iodid and Bromid of Silver;* by M. CAREY LEA, Philadelphia.

A STATEMENT has recently appeared in the French scientific Journal *Les Mondes*, from which it has been copied,[1] to the effect that ozone is capable of giving sensitiveness to insensitive iodid of silver. Mr. J. P. Kaiser states that he has found that certain vapors, such as the vapor of benzine, exercise a powerful effect of this kind, converting one insensitive variety into one that was highly sensitive: that he attributed this effect to the ozone produced by the contact of this vapor with atmospheric air:—that he therefore experimented with air ozonized by a galvanic induction apparatus, and found the same results produced, but in a much more marked degree.

This statement was not altogether the first suggestion of the matter which I had seen. A French Photographic Journal narrated, a few months back, a case in which a photographer, whose plates had refused to afford any image at all, restored their sensitiveness and got them into satisfactory working order by simply generating ozone in the room. To this latter statement, I was disposed to attach little importance. But the explicit and circumstantial one just above referred to, was a different matter.

Those who put forth these statements do not seem to have been aware of their true importance. Not only would such a fact be of great theoretical interest, but its practical applications would be immense, especially in reference to out-door photography, the easy execution of which on wet collodion would at once have become practical. For plates could be coated with insensitive iodid of silver in ordinary light, (say, for example, after Mr. Sayce's formula). This plate could be placed in an ordinary dark slide; and a vessel containing the means of generating ozone could be placed in the camera. On withdrawing the dark shutter from the slide, the plate would be uncovered, and exposed to the ozonized air of the camera. In due time the lens would be uncovered, and the image allowed to fall on the now sensitive surface. The development would then be effected in one of Weiske's developing cases, and thus the whole process would be reduced to the utmost simplicity.

In view, therefore, of the interest which attaches to this question, I was led to make the following trials. In place of using ozone generated by electricity, as done by M. Kaiser, I preferred to generate it by chemical means, viz: by phosphorus, and also by the action of sulphuric acid on chameleon mineral. As the source of the ozone was not the same as that used by M. Kaiser, my results cannot be considered as strictly controlling his, but the action of ozone from the two sources is so similar that we should naturally expect similar results in the two cases.

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[1] See British Journal of Photography, 1864, p. 392. Also Phot. Mittheilungen, Nov. 1864, in which it is suggested that ozone acts by destroying a trace of alkaline iodid present.

A. OZONE BY PHOSPHORUS.

The ozone was generated in a large bell glass, and the experiments were not commenced till paper impregnated with starch and alkaline iodid exhibited an immediate and strong ozone reaction.

I. *Action on Iodid of Silver.*

Paper was plunged into an ordinary negative bath, and dried. Strips were immersed in solution of iodid of potassium, and without leaving them in too long, were next thrown into clean water and washed.

(1.) A piece of this paper, thus imbued with washed insensitive iodid of silver, was placed in the ozone apparatus for two minutes. It was then exposed to diffuse daylight for six seconds. The application of an iron developer produced no darkening whatever. A longer exposure to light was also without effect. The paper was just as insensitive as before being exposed to the ozone.

(2.) The effects of a longer exposure to the ozone were next tried. The paper, prepared as before, was exposed for half an hour to the action of the ozone, and exposed to a moderate diffuse daylight for twenty seconds. The iron developer produced no effect whatever.

(3.) Same as (2), but exposed to light for thirty seconds. Result as before.

(4.) The paper was prepared as before, but, after immersion in the solution of iodid of potassium, the washing in water was omitted, and the strip was placed in the ozone apparatus just as it left the solution of iodid. The paper immediately changed to a deep chocolate brown, while still in the ozone apparatus. This effect was at once attributed to the action of the ozone on the free alkaline iodid, but to place the matter beyond doubt, the paper was thrown into a solution of hyposulphite of soda, which instantly bleached it.

It seems hardly likely that this reaction, so well known, could have been mistaken for an indication of sensitiveness to light. Perhaps, if the exposure to ozone had been conducted in diffuse daylight instead of in a dark room, a careless experimenter might have been misled, by the similarity of the chocolate-brown color produced to that so often occurring in photography, into a misapprehension of the agency at work, and might have supposed that the ozone had rendered the insensitive iodid of silver sensitive to the diffuse light of the room. Such an error seems, however, unlikely.

(5.) Paper was prepared as before, was exposed fifteen minutes to the action of ozone, then one minute to the light of a strong argand gas burner, one half being protected from the light. An iron developer was then applied. No effect was produced, nor could the side exposed to the light be distinguished in any way, after the application of the developer, from that which had not been exposed.

[It was previously ascertained that a sensitized collodion plate placed in the same position as respects the same light, was powerfully impressed in 20 seconds, or one-third the time just mentioned.]

(6.) Paper was prepared in the same manner, but after placing in the solution of iodid, and washing, it was dried, and in this condition exposed to the ozone for fifteen minutes. Then to the same gas light for one minute, partly covered. Result same as number 5.

(7.) After treatment with silver and iodid, and washing, the paper was exposed to ozone for fifteen minutes, then to the same gas light as in (5) and (6) for one minute, partly covered. It was then developed with pyrogallic acid to which nitrate of silver and citric acid had been added, and this was kept on till the paper was blackened by the deposition of silver. Not the slightest distinction could be traced between the parts that had been exposed to light, and those that had been produced by thick yellow paper.

II.—*Action on Bromid of Silver.*

(8.) In order to give a greater variety to the experiments, the bromid of silver submitted to trial was not disseminated through paper as was done with the iodid in the foregoing trials, but was used in a collodion film. For this purpose a collodion was made somewhat similar in character to that proposed by Mr. Sayce, in which the bromid of silver is suspended in the collodion with its sensibility destroyed by the presence of a small excess of alkaline bromid. The proportions used were as follows:

Ether, - - - - - -	½ ounce.
Alcohol, - - - - - -	1 "
Pyroxyline, - - - - -	10 grs.
Bromid of ammonium, - - -	9 "
Nitrate of silver, - - -	13½ "

These proportions leave a slight excess of bromid of ammonium present, which ensures that the bromid of silver is in the insensitive form.

(9.) A portion of this collodion was poured on glass and exposed to a highly ozonized atmosphere for three minutes. It was then exposed to a large gas light for fifty seconds. On the application of an iron developer no darkening was observable.

(10.) In order to afford a term of comparison, a film of ordinary collodion was sensitized in the negative bath in the usual manner, and was exposed to the same light for twenty seconds. On the application of the same developer it was strongly darkened.

(11.) The same collodion as in (9) was exposed to the same ozonized atmosphere for 45 minutes. It was next exposed to the same gas light as (8) and (9) for thirty seconds. The application of the developer produced no darkening.

(12.) Same as (10), but exposed to light for one and a half minutes. Result as before.

(13.) Same, but exposed to light for three minutes. Result as before. In no case was the slightest reducing effect produced.

B. OZONE BY CHAMELEON MINERAL.

Action on Bromid of Silver.

The bromid of silver was used in the same form as before, viz: in collodion containing a small excess of bromid of ammonium. I should have mentioned before that the insensibility to light of this argento-ozonized collodion was first carefully tested and proved. Ozone was generated in a closed box by the action of undiluted sulphuric acid on chameleon mineral; the vessel containing which was set aside for a short time to let the purple vapors pass off. It was then set in the box, and the condition of the atmosphere was examined from time to time by appropriate test paper.

(14.) Before commencing with the ozone trials, a piece of glass was collodionized with ordinary collodion and sensitized in the negative bath. It was then exposed to a weak diffuse daylight for thirty seconds, after which it immediately darkened under the developer. This was to determine the action and power of the light in question.

(15.) The collodion containing insensitive bromid was exposed to a weakly ozonized atmosphere for ten minutes. It was then exposed to the same daylight as the ordinary sensitive plate mentioned in (14), and for the same time. An iron developer was then applied, but not the slightest indication of sensitiveness was observable.

(16.) The last experiment was repeated under the same conditions, but with a treble exposure to the action of the same ozonized atmosphere, viz: thirty minutes instead of ten. The exposure to light was the same, and the insensitiveness the same.

(17.) Instead of a weakly ozonized atmosphere, as in the last two cases, a strong one was prepared, and a plate similarly prepared with insensitive argento-bromid collodion was placed in it ten minutes. It was then exposed to the same light and treated in the same manner as in the experiment immediately preceding, and with the same results:—not the slightest indication of sensibility.

In this last experiment, the atmosphere was ozonized sufficiently to render iodid of potassium and starch paper instantly blue. In the two previous, this reaction showed itself more gradually.

In remarking upon the experiments just detailed, I may observe in the first place that the ozone was unquestionably always present in such strength as to bring out its marked chemical effects. This was demonstrated not only by its action on the characteristic test paper, but also by its chemical action exhibited in experiment (4).

Again, the experiments were tried in the most varied ways. The ozone was produced in two different manners, and of varied strength. The surfaces to be tested were also exposed for widely varied times, from two minutes to forty-five.

The nature of the light was also varied, experiment having been made both with daylight and artificial light. The time of exposure to light was very various, and in several cases, after the first results had been noted, the paper or film still wet with developer, was carried into the light and exposed for some time, to see if any faint sensibility existed and would manifest itself by the prolonged action of light. No such result appeared. The experiments included both iodid and bromid of silver, and both paper (of which two different sorts were intentionally used) and collodion were used as the vehicles for the silver compound.

The result appears to show pretty clearly that ozone has no power of giving sensibility to insensitive iodid or bromid of silver formed in the presence of excess of alkaline iodid, whether the excess be left present, as in the bromid experiments, or be removed, as in those made with iodid. Or at least that this is true in respect to ozone produced in the two manners which I have described. If Mr. Kaiser has really succeeded in sensitizing plates in the manner described by him, I think that result might possibly be ascribable to some direct effect of the induced electricity which he employs, and not to the ozone produced. But the description of his experiments which has reached me in the *British Journal* is so brief (no details whatever are given) that the above can only be considered as a mere suggestion of an explanation of the opposite results obtained by him. The whole subject is an interesting one, and the effect of all the forms of physical force, especially electricity and heat, on the silver haloids might no doubt be advantageously studied.

ART. XII.—*Prize for applications of the Electric Pile.—Heterogeny.—Influence of Light on Proto-organisms.—Association for the Advancement of Meteorology.—On the intensity of action of the Solar disk.—Acclimation of Salmon in Australia.—Production of the Sexes.—Cutting of the Isthmus of Suez.—First idea of Electric Telegraphy.* Correspondence of Prof. J. NICKLÈS, dated Nancy, France, Oct. 20, 1864.

Prize for applications of the electric pile.—The prize of 50,000 francs, which was founded in 1852 by the Emperor Napoleon, has just been decreed to Ruhmkorff, for the induction apparatus known as "Ruhmkorff's coil," the mechanism of which we were among the first to make known.[1] The committee, consisting of Messrs. Pelouze, Rayer, Serres, Becquerel, H. St. Claire Deville, and others, was presided over by Dumas. We make the following extracts from his report:

"Mr. Ruhmkorff was at first a workman for some of our best constructors of physical apparatus, afterward had his own workshop, and finally became head of a house of constantly increasing celebrity.

"His education was gained, little by little, through reflection, study, and the lectures of certain professors heard as it were by stealth in his occasional hours of leisure. Modest, of unyielding perseverance, and of a self-devotion which has earned for him the highest encomiums, Mr. Ruhmkorff will ever remain the type of his class—a model for the numerous intelligent workmen who fill the higher order of workshops (ateliers de precision) of Paris. To those, who, like him, know how to control their desires, who faithfully strive for perfection in work and clearness in conceptions, who bend their attention to one object, and labor untiringly until a high superiority is gained and also for themselves the satisfactions of a ripe age, the compensation for the sacrifices and privations of youth will not be lacking in a country where, more than ever, merit finds recompense.

"Since 1851, Mr. Ruhmkorff has devoted himself to the construction and perfection of his apparatus, and he has ended by securing for it his own name, by giving it a scientific value which no one contests, and by rendering it of so great power as to become a means of numerous practical applications.

"The Ruhmkorff apparatus unites the two forms of electricity, which had been separated as by an abyss—the old machine electricity, characterized by a capability of producing sparks and by strong tension, and the electricity of the pile, characterized by very feeble tension and by its inability to produce true sparks.

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[1] This Journal, 1853, xv, 114; 1862, vol. xxxiii.

"If Franklin's discoveries placed beyond doubt the identity of mechanical electricity and lightning, there remained, nevertheless, among the phenomena which accompany storms, many circumstances the explanation of which was yet inaccessible to science. We must therefore regard as a valuble acquisition to meteorology the observation that the spark of the Ruhmkorff apparatus consists of two parts—an instantaneous line of light and an areola of measurable duration. The magnet divides the latter; a breath, or any body in motion, draws it out, and the electric spark thus divided continues its route in these two directions at once, as long as the passage of the current continues uninterrupted.

"In a vacuum, the electric spark develops light, and that developed by the Ruhmkorff spark takes different colors in different gases; it brilliantly illuminates fluorescent bodies, and divides itself into parallel bands separated by dark spaces perpendicular to the axis of the recipients. These colored luminous bands obey the action of the magnet which attracts or repels them, and which impresses upon them, at the pleasure of the operator, those movements of translation or of rotation by means of which De la Rive[1] has reproduced the appearances observed in the Aurora Borealis, justifying, thus, the analogy recognized between the electric light produced in a vacuum and that of the polar auroras.

"Glass tubes illuminated by the same means give out a light sufficiently bright to be used in mines that are liable to explosion: they also serve under water as a lamp for divers—and in surgery, to carry into the back part of the mouth, and other cavities of the body, a light which gives no sensation of heat.

"The spark of the Ruhmkorff apparatus inflames combustibles, and explodes gaseous mixtures; and it has thus supplied to Lenoir's gas-engine the means necessary for producing the successive inflammations to which it owes its mechanical power.

"The working of quarries, the boring of tunnels, the explosion of heavy charges in mines, give to-day regular employment to the Ruhmkorff apparatus.[2] Mines had been exploded previously by the aid of the pile; but the Ruhmkorff apparatus has left all other modifications far behind, by reason of the small number of elements which it requires—three instead of one hundred—the power of its spark, and finally the possibility of exploding eight or ten mines at once. In the expedition to China, in 1860, the Ruhmkorff apparatus was used to blow up, by means of eight mines simultaneously exploded, the principal fort of the Peiho, as well as the iron stockade at the bottom of the river."

Dumas then reviews the principal applications of electricity to Mechanics, such as the *automatic break* of Achard, the weavinglooms, the pentagraph of Caselli, the writing telegraph of Prof. Hughes, etc. He then spoke of illumination by electricity, and mentioned the electric regulators of Staite, Serrin, and Dubosc, the illumination of light-houses by induction currents, observing that successful experiments have been made with it by the Light-house Board: at Havre, upon Cape de la Hève it erected, several years ago, an electric lighthouse equivalent to 3000 Carcel burners, by the side of one lighted with oil; the electric light was readily distinguished, by its brilliancy and intensity, from its neighbor which appeared red.

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[1] **This Journal, 1858, vol. xvi.**

[2] **This Journal, 1854, vol. xviii.**

Under *Medical electricity* there was a reference to the observations of Dr. Duchenne of Boulogne and those of Middledorf, of which we have given an account when treating of Galvanocaustic, and according to which, by means of platinum wires brought to incandescence, the tissues may be divided, and the removal effected of polypi and tumors from organs that are deeply seated or otherwise difficultly accessible.

A new competition will take place, five years hence, to which all applications of electricity will be admitted, whether to medicine, the mechanic arts, or industry, without distinction of origin or of nationality.

Heterogeny.—The discussion which has existed for several years between the partisans of spontaneous generation and their opponents, is entering upon a new phase. The two parties desire to unite and experiment together, for it is in that direction that a beginning must be made. Thus it has happened, as in politics, at first fighting, then, conferences or conventions.

Whatever be the opinion on this great subject of generation, it is impossible that new and instructive facts should not be brought out by this convention, in which chemists of known skill are assisted by such physiologists as Pouchet and Joley. There is every reason to believe that each party holds some truth, and that their differences have arisen from their different methods of experimenting. At any rate, there is no opportunity for temper in the debate.[4]

Influence of light upon the production of proto-organisms.—One element of the solution of this question of heterogeny resides in the sun's light, which produces some effects that are not known to result from artificial light or heat, as has been already made known by Messrs. Pouchet and Ch. Morren. Some new facts in their support have been observed by Professor Montegazza at the University of Pavia.

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[4] Since the above was written, several meetings have been held at the Garden of Plants in Paris. Nothing has resulted from this convention except the very sad fact that theological considerations seem to have got the better of the attentive observation of facts, just as in the time ef Galileo. There is, then, nothing to do but to observe that the savants have all agreed that reason, which doubts, observes and studies, has nothing in common with that kind of faith which believes without examination and without proofs.

Two female frogs were quickly killed, by the destruction of the spinal marrow, and placed in two glass vases each of which contained 115 cubic centigrams of well-water perfectly transparent and free from all foreign bodies. One of these vases was left in diffused light, the other was placed in a box which did not permit a single ray of light to reach it.

The experiment was continued seventeen months. It is necessary, before giving the results of this long trial, to state that every time that the two vases were compared, they remained exposed to the air for an hour or more, and that the air was renewed at each observation. Germs could, then, easily fall in; and yet the results were very different in the two cases.

The following are the observed results:—

1. Two identical bodies exposed to free air may present very different phenomena of putrefaction, according as they are exposed to the influence of the light, or shut off from it.

2. The chemical and biological phenomena of the two forms of putrefaction are very different, that is, we have in each case some special chemical products, and some peculiar animal and vegetable productions.

3. In darkness, there was a marked tendency to the production of vegetable organisms and very simple infusoria; the frog that underwent putrefaction while shut off from the light produced only some Mucedines, Monads and Vibrios; while the other afforded a very complicated fauna—Bacteriums, Vibrios, Spirellas, Monads of different species, Amœbas, Kerones, Alysum, Enchelides, Trachelius,—and finally Infusorians still undescribed, much resembling the Zoosperms of the Tritons.

4. The abundance and superior organization of the Infusoria depend much more upon the progress of the putrefaction than upon the amount of putrescible matter. The more simple species always appear first.

5. The production of species of Bacterium takes place many times during the course of a long putrefaction.

6. When the liquid presents a new fauna, the new species are from the outset represented by a number of individuals at once; from one day to the next, they are simultaneously produced.

7. In the course of a long putrefaction, there are some generations which endure for some days; others exist for a much longer time.

8. Rapid changes in the chemical composition of a putrescible liquid are always, or nearly always, followed by new sets of animal and vegetable microscopic life.

9. When circumstances are little favorable to heterogeny in a very long putrefaction, there may be intervals of time of greater or less length, in which the liquid presents no organism. Who-

ever should content himself with observing at such a moment, might say that there had been no generation; while some days before, or some days after, there had been, or there would be, a very abundant production of vegetables, or of animals, or of both at once.

From all the comparative tables, I select an observation made on November 20th, after more than seven months of putrefaction. Temperature at the time 10° C.

In the light.—The liquid had a slight odor of boiled meat.—On the surface many very lively Kolpodes, a great quantity of Vibrios, some Alysca.—Some Infusoria which strikingly resemble the Zoösperms of Tritons.—At the bottom, some dead Kolpodes, and some in the act of multiplying by division into two or four individuals. Many Vibrios; some Monads.

In the dark.—The liquid had a very strong odor of mushrooms.—No organisms; the whole mass liquid.

Association for the advancement of Meteorology.—This association, founded at the commencement of the year 1864, under the presidency of Le Verrier, is a great success. It meets periodically at the Observatory of Paris. It has just founded prizes for the encouragement of meteorological studies, and especially the study of the general movements of the atmosphere. All meteorologists are admitted to competition, without distinction of nationality.

The following are some of the details of the programme:

"According to the generally received opinion, the storms of the coasts of France come, already formed, from the Atlantic. An extended series of meteorological observations made over this vast sea, is, then, the necessary basis of the work proposed for the principal prize. This prize will be of 4000 francs. The memoirs must be delivered to the Secretary of the Society before December 31st, 1865.

"A sum of 3000 francs will be divided between the authors of the best observations made at sea, or in places little known in a meteorological point of view.

"Finally, two prizes, of 500 francs each, are offered for the best memoirs upon the application of Meteorology to agricultural questions. The prizes of 300 and 500 francs may consist of instruments for observation."

Upon the intensity of action of different parts of the solar disk.—With regard to the late researches by Secchi—according to which the calorific radiation of the center of the solar disk is greater than that of the borders, nearly in the ratio of 2 : 1, Mr. Volpicelli writes that the fact was very exactly observed in 1614 by Luc Valerio, a mathematician of Naples, author of a work, *De centro gravitatis solidorum*, and of another *De quadratura parabolæ per simplex falsum.* He was a professor in the Roman University, and has been called the Archimedes of his age.

In one of his letters to Galileo, Luc Valerio considers the rays proceding from the central part of the solar disk as the more active.

Analogous facts have been observed by Mr. Roscoe, according to whom the center of the disk exerts a more intense chemical action than the borders. He has also observed that the south polar zone is more active than the north.

Acclimation of Salmon in Australia.—Recent experiments carried forward by the Acclimation Society have shown that it is possible to transport to distant countries the eggs of fertile fishes. One of its members, Mr. Millet, having observed that melting ice diminished the pulsations of the young fish of the Salmonidæ and delayed the hatching of the eggs, took the idea that this method would serve for the transportation of eggs of the Salmonidæ to Australia and Tasmania. The plan has succeeded, in spite of damaged eggs; a very large number have arrived there in a healthy state, and have been deposited in the rivers. Starting from London on January 15th, 1864, the eggs arrived at Melbourne on April 5th, and at Hobart Town, (Tasmania,) on the 23d. Everything indicates success.

Similar attempts have been made in the French possessions in Algeria, the rivers of which are very barren of fish. Eggs of salmon and trout have been carried there from Huningue (Haut-Rhin), where, as we have previously seen, are found the principal basins for pisciculture. In spite of the differences of climate, these eggs have arrived safely, and have hatched in the basins prepared for them; they already begin to people the rivers. It is a result of no moderate interest to see the salmon of the Rhine and the trout of the Vosges transported to Africa, Australia, and Tasmania, and living and becoming acclimated there.

Production of the sexes.—What are the causes of the production of the sexes? This question, which has occupied the earnest attention of physiologists, has been thoroughly studied by Mr. Thury, according to whom the product is always of the male sex when the fertilization of the ova occurs at complete maturity, and is always female when it takes place at a less advanced period.

There is a very simple way of solving this problem. It is to select for experiment species that come to maturity in succession, and, that during a single impregnation, fertilize the whole series of ova which detach themselves from the ovary during a period of eight, ten, twelve, fifteen, or even eighteen days. We know, indeed, that, in the case of the hen, a single coupling suffices for the fertilization of five, six, or seven eggs which she is about to lay and which are arranged in her ovary in the order of their maturation. Now, in such a case, if the theory is exact, the first egg laid ought always to produce males and the others females without any possibility of the inversion of this order.

This is very near what has been observed by Messrs. Coste and Gerbe:

A hen, separated from the cock at the time of her first laying this year, gave five fertile eggs in the space of eight days.

The egg laid on March 15th produced a male; that on March 17th a male; that on the 18th a female; that on the 20th a male; that on the 22d a female.

A characteristic fact in this experiment is the production of a male after a female, which ought not to have taken place according to the theory. But is it only a simple exception? Or is it necessary to consider the fact a radical objection? We may learn by and by on this point, from the researches in which Mr. Gerbe is now engaged.

On the occasion of the preceding note, Flourens recalled an experiment which he made, thirty years ago.

"Aristotle had observed that the pigeon ordinarily lays two eggs, and that of these two eggs one commonly produces a male and the other a female. He wished to know which was the egg that gave the male, and which the one that produced the female. He found that the first egg always gave the male, and the second the female. I have repeated this experiment as many as eleven times in succession, and eleven times in succession the first egg gave the male and the second egg the female. I have seen again that which Aristotle saw."

Cutting of the Isthmus of Suez.—The almost certain success of the canal across the Isthmus of Suez fixes attention, more than ever, upon other projects of the kind. The cutting of the isthmus of Malacca and of that of Darien await only the completion of the Suez ship canal. In France, they are talking of uniting the Atlantic Ocean with the Mediterranean by a ship canal which would borrow part of its route from the old Southern Canal. In Holland, a society is incorporated, under the title of a "Company for the cutting of the isthmus of Holland," to establish a canal joining Amsterdam and the North Sea by a direct route, while, at the same time, the Dutch government has undertaken to establish another navigable route from Rotterdam to the sea.

A new project is presented, which, if realized, will complete the series of maritime communications in the north of Europe. It is proposed to establish a canal, navigable for ships of war and commerce, between the North Sea and the Baltic, to avoid making the passage of the Danish islands. This project, often thought of, is now very seriously considered.

Finally, they are speaking of cutting one other isthmus, and this time it is Spain which has the honor. It is proposed to pierce the Spanish isthmus in such a way that Gibraltar will be an island. The canal is to start from Trafalgar and end at Andalusia. This canal, which would cost hardly a hundred millions of francs, has for its object to prevent more than 4000 vessels every year from lying to before the strait of Gibraltar without power to get out.

This project is just now submitted to the examination of the Spanish government, as well as to some others, such as the English and French governments.

First idea of an electric telegraph.—This first idea was brought forward in the 16th century in an anonymous work published at Rouen at that time. The question has already been discussed here. A discovery has just been made of a letter of a clerk, the abbè of Barthélemy, well known as a literary man. He writes to Madame du Dessaud, April 8th, 1772:—

"I think often of an experiment which would give us happiness. They say that with two clocks whose hands are magnetized it is sufficient to move the hands of one for those of the other to take the same direction, so that causing one to strike twelve the other will strike the same hour. Suppose that we could perfect artificial magnets so that their power would communicate itself from here to Paris: you would have one of these clocks, we would have another; upon the dial we should have the letters of the alphabet. . . . You perceive that we could thus facilitate the operation; the first movement of the hand might sound a bell which would announce that the oracle was about to speak."

Bibliography.—Among recent publications by Hachette, at Paris, are the following:

Les Assolements et les Systèmes de Culture, par M. Heuzey; 1 vol., 8vo, 540 pp.—This work contains the lectures delivered by Mr. Heuzey at the Imperial School of Agriculture at Grignon, of which he is one of the most distinguished professors. The principles which are here set forth are applicable to all countries which are to be put under cultivation.

Le Médecin des Salles d'Asile, par Dr. Cerise; 2d ed., 8vo, 200 pp.—The "Salles d'asile" of France are charitable institutions for the education of poor children. After a short historical account of the institution itself, the author proceeds to set forth the principles which should govern physicians in their supervision of these infant schools and all which relates to the care to be taken of a miscellaneous collection of children.

Abrégé de Géographie commerciale et industrielle, avec un tableau des monnaies, poids et mesures de tous les pays, par M. Gardon; 4th ed., 8vo, 400 pp.—While referring to all countries, the author deals especially with France, each department of which he describes fully as regards statistics, agricultural, mineralogical, metallurgical, manufacturing and commercial industry, importation, exportation, navigation, etc.

La terre avant le Déluge, par Louis Figuier; 4th ed., 8vo.—This is a popular work designed to initiate the people of the world in the grand phenomena of geology. It is illustrated with many colored geological maps, and more than 300 figures and ideal views of landscapes of the ancient world.

De l'Electricité considérée comme câuse principale d'l action des eaux minérales sur l'organisme, par Dr. Scoutettin; 8vo, 420 pp. The material which mineral waters hold in solution being often in insufficient proportions to account for their health-giving effects, the author has recourse to another explanation, viz: the electrical action which these waters may exert upon the human organization. We find in his work not merely a simple ideal explanation, but, on the contrary, the recital of numerous experiments made by the author upon the principal mineral waters of Europe.

L'Astronomie au XIXe siècle, par A. Boillot, 12mo, 340 pp. This work, written for universal use, gives a very faithful picture of the progress which astronomy has made from ancient times up to our day. Mr. Boillot is a very distinguished mathematician and makes astronomy his favorite study. His work has been received in France with much favor.*

Nancy, Oct. 20th, 1864.

ART. XIII.—*Discovery of Emery in Chester, Massachusetts*; by CHARLES T. JACKSON, M.D., Geologist and State Assayer.

IT has been said, in England, that "a good mine of emery is worth more to a manufacturing people than many mines of gold." Such being the case, it affords me great pleasure to be able to announce the discovery of an inexhaustible bed of the best emery in the world in the middle of the State of Massachusetts, in Chester, Hampden county, quite near to the Western Railroad, which with its ramifications leads to the largest armories and manufactories of metallic articles in this and the adjacent States.

For more than two years, the existence of important beds of magnetic iron ore, originally discovered by Dr. H. S. Lucas, has been known, and endeavors were made by that gentleman to organize a company for the purpose of smelting these ores. In consequence of this agitation, I was employed by John B. Taft, Esq., on the 19th of August, 1863, to examine the locality and to make report of my results to him.

On examination of my specimens of minerals, after returning to Boston, and my notes for sectional profiles of the rocky strata containing the iron ore, I found that the minerals, margarite and chloritoid, in talcose, hornblende, and mica slate rocks, indicated the occurrence of emery, the association of the rocks and minerals being identical with conditions known to exist in the localities of emery in Asia Minor.

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* The following titles of literary works are also contained in the letter of Mr. Nicklès:

Histoire de la Littérature française, par M. Demogeot; 5th ed., 12mo, 680 pp.

Eléments de Rhetorique, par M. Filon; 12mo, 300 pp.

Les grandes scènes de la nature, par M. de Lanoye; 12mo.

Les Champs d'or de Bendigo (Australie), par M. Perron d'Arc; 12mo, 200 pp. A description of the Australian placers.

Souvenirs d'un Sibérien, par Rufin Piotrowsky; 12mo, 200 pp. This is a very touching recital of the misfortunes which afflict the unhappy Poles, for whom Europe has only barren sympathy.

I therefore called the attention of the owners of the property to these facts, and directed search to be made for emery, and that every mineral resembling it should be sent to me for examination. Little attention was paid to this prediction at the time, and not until I had invited Dr. Lucas, who resides in Chester, by personal representations and solicitations, to make the required search, the characters of emery being fully described to him.

On his return to Chester he soon learned that the miners were complaining of the great hardness of the supposed iron ore, and that no less than forty drills were dulled in boring a single hole for blasting. He then sent me pieces of this hard rock, in the belief that it was the emery I had predicted. On examination it was found to scratch quartz and topaz readily, and to have all the properties of emery; a chemical analysis proved it to be identical with the emery of Naxos.

The owners, resident in Boston, being notified of this discovery, went with me to the locality on the 11th of October last, when a full exploration of the premises was made. There are several large beds of rich magnetic iron ore at this locality, and the emery being magnetic (as it always is) had caused it to be mistaken for magnetic iron ore, and many tons of it had been smelted with the carbonate of iron and hematite in the Berkshire county iron furnaces, without a suspicion, notwithstanding its refractory nature, that the ore was emery, with only a small admixture of iron ore.

The principal bed of emery is seen at the immediate base of the South Mountain, where it is four feet wide, and cuts through the mountain near its summit, at an angle of 70° inclination or dip to the eastward. Its course is N. 20° E., S. 20° W., and its known extent four miles. Near the summit of the mountain the bed expands to more than 10 feet in width, and in some places is even 17 feet wide.

The alternations of rock in two sections are as follows, beginning to the eastward:

1. *a*, Mica slate; *b*, 15 ft. soapstone or talcose rock; *c*, 2 feet crystallized talc; *d*, talcose slate; *e*, 1 ft. granular quartz; *f*, chlorite slate; *g*, 4 ft. *Emery*; *h*, chloritoid and margarite; *i*, magnetic iron ore; *j*, hornblende rock highly crystalline.

2. *a*, Mica slate; *b*, 6 ft. magnetic iron ore; *c*, talcose slate; *d*, 6½ ft. magnetic iron ore; *e*, chlorite slate; *f*, hornblende rock, crystallized; *g*, 7 ft. *Emery*, chloritoid and margarite; *h*, magnetic iron ore; *i*, hornblende rock.

The elevation of the upper outcrop of this bed above the immediate base of the mountain is 750 feet. There are curious rounded masses of remarkably pure emery three feet in diameter in this bed entirely invested with a coat of delicate rose-colored margarite, and a thick layer of bright green chloritoid, the investing coat being from half an inch to two inches in thickness. It is found extremely difficult to break up these masses of solid emery, drilling holes in them for blasting being very slow and laborious, and no grip can be had on their rounded sides by the sledge. A heavy drop hammer will be required to break them to pieces—or they may be cracked by fire if heat does not injure the emery.

A branch of Westfield river separates the South from the North mountain, a hill nearly 750 feet high, through the summit of which the great emery bed also cuts. On this hill the emery is more largely crystalline and less mixed with magnetic iron ore. It is more like corundum, but still contains the combined protoxyd of iron, characteristic of true emery. Its specific gravity is from 3·75 to 3·80, while that from the South mountain is from 4·02 to 4·37; Naxos emery being from 3·71 to 3·72, according to my trials of it, in comparison.

On the North mountain, three large beds of rich magnetic iron ore, distinct from the ore accompanying the emery, occur, the ore yielding 54½ per cent of metallic iron. This ore is mined, and is smelted into bar iron by forge fires, and is also sold to mix with the hematites and carbonates of iron worked at the Lenox and Stockbridge furnaces.

Digesting the South mountain emery in fine powder with nitro-muriatic acid and sulphuric acid for a long time, it was found that 73 per cent of it was wholly insoluble in acids; and on microscopic examination the grains were seen to be translucent, and exactly like the Naxos emery prepared in the same way; but these translucent grains are readily taken up by the magnet. I therefore infer that protoxyd of iron is a chemical constituent of true emery.

Chemical analysis of the coarsely crystalline emery of the North Mountain, Chester. Sp. gr. 3·75. H.=9.

Alumina,	46·50
Protoxyd of iron,	44·00
Titanic acid,	5·00
Silica & loss,	4·50
	100·00

Emery of the South hill.* Sp. gr. 4·02. H.=9.

Alumina,	45·50
Protoxyd of iron,	43·00
Silica & titanic acid,	11·50
	100·00

Regarding the oxyd of iron which can be dissolved out from emery by acids as accidental, and that which cannot be so removed as an essential constituent, we shall have for the composition of three samples of emery analysed, after digestion in acids,

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* The highest specific gravity of any sample from the South mountain was 4·3784.

	Chester, 1.	Chester, 2.	Naxos best selected emery.
Alumina,	60·4	59·05	62·3
Protoxyd iron,	39·6	40·95	37·7
	100·0	100·00	100·0

If this view is adopted, emery must be ranked as a distinct species, and not as a mere granular form of corundum or sapphire.

In conclusion, I would state that practical trials of the Chester emery, in several of the large armories and machine shops of this and the adjoining States, have proved it to be fully equal in value to the well known emery of Naxos, which I have no doubt it will wholly supplant in this country, and that it will ere long become an article of export to Europe, either in its native form, or in a manufactured state.

It may be proper to add, that John B. Taft, Esq., of Boston, in behalf of his associates, owners of the emery mine, has the sole management of the business connected with the mine.

I would express my obligations to Mr. J. L. Smith for the valuable information contained in his articles on the emery of Asia Minor and on the associated minerals of the emery localities, published in vols. x. and xi. of this Journal. Also to Dr. H. S. Lucas, of Chester, for kind assistance in the field.[7]

32 Somerset St., Boston, Dec. 12th, 1864.

ART. XIV.—*On the Solubility of the Sulphate of Baryta in Sulphuric Acid;* by Prof. J. NICKLÈS.

THE sulphates of baryta, strontia, and lime, are slightly soluble, as is known, in boiling sulphuric acid. I have found that they are soluble in *cold* acid when in a nascent state.

To obtain this result, it is only necessary to throw a little chlorid of barium, or of strontium, into a sufficient quantity of monohydrated acid: the chlorid is by degrees decomposed, chlorhydric acid escapes, and the sulphate produced dissolves in the surrounding acid.

This is especially true for the chlorid of barium; but to succeed, the acid should be concentrated, and it is best to have the chlorid well dried and in powder. On adding water to the solution thus obtained, the sulphate of baryta falls as a white precipitate.

The chlorid of strontium gives the same results, and the acid solution affords a similar precipitate with water, though less in amount, since less is dissolved.

[7] On page 87, the date 19th of August should be 19th of October.

The sulphate of lime is still less soluble in sulphuric acid, and the solution takes several days to become limpid. Moreover, water does not then becloud it; a light precipitate is obtained with alcohol.

The solubility of these sulphates in sulphuric acid, is then the reverse of their solubility in water, except for the sulphate of strontium, which, in both cases, is a mean between those of the other two.

Nancy, Oct. 20, 1864.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On a new saccharimeter.*—Wild has described a new saccharimeter which appears to possess great advantages over the well-known instrument of Soleil. In Wild's apparatus the incident light is polarized by means of a Foucault's calc-spar prism, which is capable of being turned round its axis, the amount of rotation being measured by means of a vernier applied to a small circle graduated to single degrees. The analyzer consists of a modification of Savart's polariscope, that is to say, of two crossed plates of quartz, each 20mm. in thickness, cut at an angle of 45° with the optical axis. The fringes produced are viewed with a small astronomical telescope of low power (objective 33mm. focal length, ocular 24mm. focal length). The telescope is adjusted for parallel rays, has cross wires, and a Nichol's prism in front of the eye piece. The tube containing the solution of sugar is of course placed between the polarizer and analyzer. The author instituted comparative experiments to test the degree of accuracy attainable with Soleil's instrument and with his own, employing for the purpose a solution containing 298 grams of cane sugar to the litre. The mean error of a single compensation in Soleil's instrument amounted to 1°, when the eye was properly protected, while it reached 1½° by direct observation in an ordinary room without exclusion of side light. From this it results that in the most favorable case there is with Soleil's instrument an uncertainty of ±13·9 gr. in a litre of the solution, or rather more than one per cent. With Wild's instrument the error of observation with white light amounted to 12′, which corresponds to an error of ±2·78 gr. of sugar in a litre of solution, and consequently to a degree of accuracy five times greater than with Soleil's apparatus. When this instrument was directed toward an Argand gas burner in a dark chamber, and a dark red glass was placed between the eye and the ocular, the error of observation amounted to only 3′. Finally, when a homogeneous soda flame was employed, the error amounted to only 1′. In the first case, the error in the quantity of sugar would amount to ±0·7 grs.; in the second, to only ±0·2 gr. in a litre.[1]—*Pogg. Ann.*, cxxii, 626.

W. G.

[*Note.*—Wild's Saccharimeter, independently of its purely technical uses, will form a most acceptable addition to physical cabinets. To the chemist it offers the most precise method of determining the rotatory polarizing powers of liquids at present known, provided that homogeneous light be employed, since the characteristic lithium, sodium and thallium lines afford perfectly definite degrees of refrangibility. In Broch's measurements of the rotatory polarization in quartz by means of the fixed lines in the spectrum, the mean error of a single measurement varied from about 6′ to about 30′, according to the refrangibility of the light. In a series of measurements undertaken to determine the degree of accuracy with which the change of tint could be determined in plates of quartz placed between two Nichols' prisms and examined with white light, I found, as might be expected, that the error of observation increased as the intensity of the incident light diminished, the minimum error of estimation being about 15′. Hence it appears probable that the degree of accuracy attainable in Wild's instrument might be still farther increased by using a condenser so constructed as to give an intensely brilliant beam of parallel and homogeneous rays.—W. G.]

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[1] Wild's Saccharimeter can be procured of Hoffman, optician, 3 Rue de Buci, Paris.

2. *On the mechanical energy of chemical action.*—Schröder van der Kolk has communicated an interesting and suggestive memoir upon the application of the theory of mechanical energy to chemical processes, especially in connection with the relations of chemical affinity to heat. We shall endeavor to give some idea of the train of reasoning in this paper, referring however to the original for more ample illustration. The author in the first place cites the opinion expressed by H. Sainte-Clare Deville, in his paper on dissociation, that all chemical compounds may be resolved into their elements by a sufficiently high temperature. In some cases the separated atoms reunite at a lower temperature; in other cases they remain separate. This gives us from this point of view two classes of bodies, and the author connects the phenomenon with the facts observed by Favre and Silbermann that certain combinations are effected with evolution, others with absorption of heat. The relation between the two classes of phenomena appears from the following considerations. If we conceive of a body at the temperature of 0°, and then apply heat, the body at a definite temperature will have absorbed a definite quantity of heat. This heat is applied partly in raising the temperature, partly in molecular changes, and partly in external work. The entire quantity of heat continually increases with the temperature, and heat is always taken up both in raising the temperature and in changing the state of aggregation from the solid to the liquid, and from this to the gaseous condition. To find the quantity of heat in the body after the process, we must subtract the quantity which is consumed in external work. The quantity remaining is termed by Thomson the mechanical energy of the body. Starting as we have done from 0°, we obtain in this manner only the accumulation of energy in the body from 0° to the particular temperature under consideration, and not its absolute quantity. Every body has therefore in any definite condition a definite quantity of energy. If now we consider two substances, as, for example, oxygen and hydrogen, which combine with each other under the influence of the electric spark, we have in each, before combination, a definite quantity of energy. During the combination, heat is evolved, and if we now cool the vapor of water

which is formed to the temperature of the components before combination, the steam will contain a quantity of energy less than that of its components by the quantity which is given out in the combination. We here suppose the combination of H and O to take place without external work. When the vapor of water is decomposed at the same temperature, just as much energy must be added as became free during the combination. Two cases may then occur: either the body has more energy than its components, or else the contrary is true. In the first case heat is given out in the decomposition, in the second, it is absorbed. It follows that heat must become free when we heat a body of the first class to the point of decomposition, and the components will not unite on cooling, since the quantity of energy in them is not sufficient for the formation of the compound at the same temperature. This could only take place in two cases: either the body must take up heat from the surrounding medium, which, however, has at most an equal temperature, or else the compound must become suddenly colder. So far as known, no such cooling occurs. From the above we obtain the following law: Bodies which evolve heat in being decomposed by heat are not again formed in the subsequent cooling. This law may be tested by the results of the experiments of Favre and Silbermann. These physicists found that protoxyd of nitrogen, NO, peroxyd of hydrogen, HO_2, chlorous and chloric acids, $Cl\,O_3$ and $Cl\,O_5$, evolve heat during decomposition, while the elements separated do not recombine on cooling. The same is true for the chlorid, iodid and sulphid of nitrogen, which are suddenly decomposed on heating, with evolution of heat, and the components of which do not recombine. The above law is also true when dimorphous or polymorphous bodies change their physical condition. Thus, aragonite on heating passes into calc-spar with evolution of heat; the converse change does not take place on cooling. Fused sulphur crystals (System V) on cooling become crystallized in System IV with evolution of heat; the converse change does not take place. Various other illustrations are given, and the author generalizes the law already stated as follows: When bodies on heating pass into a different physical state, with evolution of heat, the previous state is not resumed on cooling. It is of course possible that in certain cases the purely chemical affinity between the molecules may be so powerful that combination will ensue, heat being abstracted from the surrounding medium. The converse proposition is as follows:

If heat be absorbed in decomposing a body by means of heat, the contrary action will take place on subsequent cooling.

This proposition cannot be demonstrated theoretically, and is therefore not to be regarded as proved. It is frequently found by experiment to be true: thus, carbonate of lime, according to Favre and Silbermann, absorbs heat when decomposed by heating, and has therefore less energy than its components together. CO_2 and $Ca\,O$ recombine on cooling. On the other hand, much heat is evolved in slaking lime, and CaO, HO has therefore much less energy than its components. On heating, the hydrate is decomposed, and is again formed on cooling. In the formation of CO_2 and HO heat is evolved, and the compounds have therefore less energy than the components. The elements only unite at a high temperature, and the decomposition takes place at a still higher temperature.

On cooling, the components ought to recombine, but in point of fact this does not happen, though the components have much more energy than the products. The explanation of this depends upon the action of the purely chemical affinities, which at lower temperatures are not sufficient for the formation of the compound. Two conditions are in general necessary for the formation of a compound. First, a degree of chemical force or affinity sufficient for the combination, and secondly, the necessary energy. A single cause is insufficient. The author here distinguishes between reversible and unreversible processes. When a body changes under the action of heat, and with evolution of heat, it passes into a new condition of less energy, and therefore cannot in itself pass back into its original state. This is the unreversible process, and occurs most frequently. When heat, on the contrary, is absorbed, the new body has more energy, and may therefore on cooling pass of itself back into its previous condition. This is illustrated by the latent heat of fusion and evaporation. Other consequences of this theory are these; the heat of combination remains the same whether the compound be formed suddenly or by degrees; the heat of combination of a compound body is in general less than that of its components taken singly. The following examples serve to illustrate still further the application of mechanical energy to the explanation of chemical changes. An electric spark causes combination in two ways. Certain bodies, H and O, H and Cl, CO and O, &c., combine suddenly in unlimited quantities by means of a single spark, and with evolution of heat. Others, like N and O, combine only gradually; no heat is evolved, and the combination ceases with the cessation of the spark. The formation of ozone is also a case in point. In the first case, the energy of the components is greater than that of the compound. Sufficient energy is present, but not sufficient affinity. The electric spark here increases the affinity; some atoms of H and O combine, and the heat produced causes a further combination, since heat within certain limits increases affinity. In the case of the combination of N and O to form nitric acid, the components have less energy than the compound. Then the spark furnishes the requisite energy, and each spark yields only a definite amount of work. The case of NH_3 forms an exception which for the present must remain unexplained. The author also applies the theory to the catalytic action—so called—exercised by platinum upon H and O, but not upon N and O. In conclusion, the author takes ground against the assumption often made that the heat of combination is to be taken as the measure of chemical affinity. He considers that this heat, regarded as difference in energy, is the measure of the stability of the compound. Thus, water with the heat of combination expressed by 29413 has more stability than chlorhydric acid with a heat of combination of 23783 units. In each case, besides the requisite energy, the requisite affinity must be present. Thus, in a simple mixture of O and H no combination takes place until the affinities are sufficiently increased, although the necessary energy is always present. A further argument against considering the heat of combination as the measure of affinity is to be found in the fact that in certain combinations heat is absorbed, so that in these the chemical affinity would be negative.—*Pogg. Ann.*, cxxii, 439. W. G.

3. *On the replacement of hydrogen in ether by chlorine, ethyl and oxethyl.*—Lieben and Bauer have studied the action of monochlorinated and bichlorinated ether upon zinc-ethyl and ethylate of sodium, and have obtained results of much theoretical interest. By the action of chlorine upon ether, at ordinary temperatures, the author obtained a mono-chlorinated ether with the formula, $\left.\begin{matrix}C_4H_4Cl\\C_4H_4Cl\end{matrix}\right\}O_2$. The action of zinc-ethyl upon this compound gives rise to two products which have respectively the formulas, $\left.\begin{matrix}C_4H_4.C_4H_5\\C_4H_4.Cl\end{matrix}\right\}O_2$, and $\left.\begin{matrix}C_4H_4.C_4H_5\\C_4H_4.C_4H_5\end{matrix}\right\}O_2$, and in which, as will be seen, one or two atoms of hydrogen in the radical ethyl are replaced by one or two atoms of ethyl itself. When mono-chlorinated ether is heated with an alcoholic solution of potash, or with ethylate of sodium, an oily liquid is formed, which boils at 157° C., is heavier than water, and has a most refreshing and agreeable odor. The formula of this body is, $\left.\begin{matrix}C_4H_4Cl\\C_4H_4.C_4H_5O_2\end{matrix}\right\}O_2$, so that it must be regarded as ether in which one atom of hydrogen in the ethyl is replaced by chlorine and one atom by the radical, $C_4H_5O_2$, which the authors term oxethyl. By the continued action of ethylate of sodium upon the last mentioned substituted ether, the authors obtained an oil lighter than water, boiling at 168° C., and having the formula, $\left.\begin{matrix}C_4H_4.C_4H_5O_2\\C_4H_4.C_4H_5O_2\end{matrix}\right\}O_2$. In this, two atoms of hydrogen are replaced by two of oxethyl. Finally, the action of ethylate of sodium upon this body, $\left.\begin{matrix}C_4H_4Cl\\C_4H_4.C_4H_5\end{matrix}\right\}O_2$, gives rise to a new liquid, boiling at 148° C. and presenting the formula, $\left.\begin{matrix}C_4H_4.C_4H_5O_2\\C_4H_4.C_4H_5\end{matrix}\right\}O_2$. The new substances described are evidently the types of a large class of similar substituted bodies, and deserve in themselves an attentive study, especially with reference to physical properties and to products of decomposition.—*Comptes Rendus*, lix, 445. W. G.

4. *On a new mode of preparing oxygen.*—Robbins has given a method of preparing oxygen which is particularly interesting from a theoretical point of view, and which may hereafter be of much practical value. The process is based upon an experiment due to Schönbein, and consists in pouring dilute sulphuric acid upon a dry and powdered mixture of three equivalents of peroxyd of barium and one equivalent of bichromate of potash. Ozone and antozone are given off simultaneously, and unite to form common oxygen, which escapes with effervescence.—*Cosmos*, xxvi, 386. W. G.

II. GEOLOGY.

1. *On buried stems and branches in Illinois;* by J. S. Bliss. (From a communication to the Editors, dated Columbus, Wisconsin, Nov. 7.)—While passing through the county of Adams, in the State of Illinois, four years since, I learned of the existence of a well, which had been dug the year before, from which small branches of trees and twigs had been obtained. I repaired to the spot, and found the well to be twenty-five feet in depth; and was informed that, at a depth of twenty-three feet, the owner

came upon a layer of *fine black soil,* full two feet in thickness, in which were found branches an inch in diameter; I procured a shovel, and obtained several specimens three-eights of an inch through, in a tolerable state of preservation. Just above this bed of *soil* the material was clay.

In another well, three miles from this, the same kind of soil was met with, after passing through clay for twenty-seven feet from the surface. In the soil twigs were found; but on being brought to the air they crumbled to pieces, though the *bark* and *heart* were plainly to be seen.

Four miles from the first mentioned well, there was another, still more remarkable, which was 30 feet deep. At a depth of 27 feet, the diggers came square on a log, twelve inches in diameter; they cut it off on both sides of the well, and on taking it out pronounced it to be *Black walnut;* but I think that it was probably some other kind of timber, that had been dyed in consequence of having been submerged, and that this caused the mistake. In the same soil was also found a *Buckthorn* in a tolerable state of preservation. The top of the tree was lying southeast.

Another fact, I have noticed in Wisconsin, is that stones protruding through the surface, such as those known as *hard-heads,* present their perpendicular sides to the north, northeast, and northwest. This is not confined to any small portion of the State, but to quite an extended region.

2. *On the Geology of Eastern New York;* by Professor JAMES HALL and Sir WILLIAM E. LOGAN. (Read before the Natural History Society of Montreal, Oct. 24, 1864, by T. STERRY HUNT.)—Professor James Hall and Sir William Logan spent a few days together last summer in examining some points of the geology of Eastern New York, and propose to continue their examinations next season, when we may expect from them a detailed account of their results. Their principal object was to compare the rocks of that region with some of those of Eastern Canada; and I have now permission, in the absence of these gentlemen, to lay before this Society some of the results of this exploration.

The shales of the Hudson River group, which are seen for a considerable distance north and south of Albany, disappear a few miles east of the Hudson, and are succeeded by harder and coarser shales, sometimes red or green in color, and passing into green argillaceous sandstones. These various strata, which are associated with concretionary and shaly limestones, are now recognized as belonging to the Quebec group. The line of contact between this and the much more recent Hudson River group has nowhere been clearly seen in this region, but the two series are readily distinguished by their differences in color, texture, and hardness,—differences which were formerly supposed to depend upon the partial metamorphism of the eastern portion, when this was looked upon as a part of the Hudson River group. The green sandstones and conglomerates of Grafton Mountain, formerly looked upon as a portion of the Shawangunk conglomerate, are recognized as belonging to an outlying portion of the Sillery formation. This mountain Professor Hall had found in a previous exploration (1844–45) to have, at a point farther south, a synclinal structure, and it probably lies in three low synclinal axes. The Sillery formation scarcely extends south of Rensselaer County.

Canaan Mountain is also apparently synclinal, and, while limestones appear in the valleys on each side of it, consists chiefly of slates, the highest beds being a hard green sandstone, sometimes shaly, without any of the conglomerates of the Sillery, although boulders and angular fragments of these are found in the adjacent valleys. To the east of this, Richmond Mountain, in Massachusetts, presents in its upper portion a compact green slate, passing upward into a harder rock similar to that of the summit of Canaan Mountain. To the southward, as far as Hillsdale, the sparry limestones of the Quebec group appear in the valleys, while the hills are of slate. Proceeding thence westward toward the river, only the lower portions of the Quebec group are met with, until we come upon the rocks of the Hudson River group.

Washington Mountain is also of slate, flanked by limestone, all of the Quebec group, and is probably synclinal in structure. The valley to the south of the mountain exhibits limestones, apparently alternating with slates. Columbia and Duchess counties appear to be mainly occupied by the shales of the Quebec group, with broad exposures of its limestones, until we approach the river to the westward, when the shales of the Hudson River group are met with, extending a considerable distance below the city of Hudson. The gneiss of the Highlands occupies the southeast part of Duchess County.

From Fishkill the explorers proceeded to Coldspring, crossing what Mather called the Mattewan granite, but which they found to be an altered sandstone. Soon after this they came upon the great gneiss formation of the Highlands of the Hudson, which continues beyond Peekskill. They failed to find the sandstone described by Mather as coming out at this place; nor was anything representing the Potsdam sandstone detected in approaching the Highlands from Fishkill, nor elsewhere along their northern limits. Near to Peekskill, in the valley of the creek, was found a low ridge of black slate, supposed to belong to the Quebec group, and a similar slate was observed along the north side of the Highland range, not far from the gneiss. The gneiss of the Highlands presents all the aspects and characteristics of that of the Laurentian system, as seen in northern New York and in Canada.

Further examinations are necessary to determine the extension to the northeast of the Laurentian rocks of the Highlands, and also the succession of strata to the southeast of them. The recognition of the Sillery and of the Quebec group in this region are great and important facts of its geology, and not less so the identification with the Laurentian system of the gneissic district of the Highlands, to which the interesting mineral region of Orange county and the adjacent parts of New Jersey doubtless belongs. This conclusion, although opposed to the views of Mather and Rogers, who looked upon the crystalline rocks of the latter region as altered Lower Silurian strata, is in accordance with the older observations of Vanuxem and Keating, and with the more recent ones of Professor Cook, according to all of whom the gneiss and crystalline limestones of Orange County and of New Jersey underlie uncomformably the Lower Silurian strata.[1]—*Can. Nat. and Geol.*

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[1] See Am. Jour. Sci., [2], xxxii, 208, and also Kitchell's 2nd Annual report on the Geology of New Jersey, (1855,) page 134 and onward.

3. *Action of Glaciers;* by J. Ruskin.—What a river carries fast at the bottom of it, a glacier carries slowly at the top of it. This is the main distinction between their agencies. A piece of rock which, falling into a strong torrent, would be perhaps swept down half a mile in twenty minutes, delivering blows on the rocks at the bottom audible like distant heavy cannon,[1] and at last dashed into fragments, which in a little while will be rounded pebbles (having done rough damage to everything it has touched in its course)—this same rock, I say, falling on a glacier, lies on the top of it, and is thereon carried down, if at fullest speed, at the rate of three yards in a week, doing, usually, damage to nothing at all. That is the primal difference between the work of water and ice; these further differences, however, follow from this first one.

Though a glacier never rolls its moraine into pebbles, as a torrent does its shingle, it torments and teases the said moraine very sufficiently, and without intermission. It is always moving it on, and melting from under it, and one stone is always toppling, or tilting, or sliding over another, and one company of stones crashing over another, with staggering shift of heap behind. Now, leaving out of all account the pulverulent effect of original precipitation to glacier level from two or three thousand feet above, let the reader imagine a mass of sharp granite road-metal and paving-stones, mixed up with boulders of any size he can think of, and with wreck of softer rocks, (micaceous schists in quantities, usually,) the whole—say half a quarter of a mile wide, and of variable thickness, from mere skin-deep mock-moraine on mounds of unsuspected ice—treacherous, shadow-begotten—to a railroad-embankment, *passenger*-embankment—one eternal collapse of unconditional ruin, rotten to its heart with frost and thaw, (in regions on the edge of each,) and withering sun, and waste of oozing ice; fancy all this heaved and shovelled, slowly, by a gang of a thousand Irish laborers, twenty miles down hill. You will conjecture there may be some dust developed on the way?—some at the hill-bottom? Yet thus you will have but a dim idea of the daily and final results of the movement of glacier moraines;—beautiful result in granite and slate dust, delivered by the torrent at last in banks of black and white slime, recovering itself, far away, into fruitful fields, and level floor for human life.

Now all this is utterly independent of any action whatsoever by the ice on its sustaining rocks. It *has* an action on these, indeed; but of this limited nature, as compared with that of water. A stone at the bottom of a stream, or deep sea-current, necessarily and always presses on the bottom with the weight of the column of water above it—plus the excess of its own weight above that of a bulk of water equal to its own; but a stone under a glacier may be hitched or suspended in the ice itself for long spaces, not touching bottom at all. When dropped at last, the weight of the ice may not come upon it for years, for that weight is only carried on certain spaces of the rock bed; and in those very spaces the utmost a stone can do is to press on the bottom with a force necessary to drive the given stone into ice of a given density (usually porous); and, with this maximum pressure, to move at the maximum rate of about a third of an inch in a quarter of a hour! Try to saw a piece of marble through (with edge of iron, not of soppy ice, for saw, and with sharp flint sand for feldspar slime,) and move your saw at the rate of an inch in three-quarters of an hour, and see what lively and progressive work you will make of it!

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[1] Even in lower Apennine. "Dat sonitum saxis, et torto vertice torrens."

I say "piece of marble;" but your permanent glacier-bottom is rarely so soft—for a glacier, though it acts slowly by friction, can act vigorously by dead weight on a soft rock, and (with fall previously provided for it) can clear masses of that out of its way, to some purpose. There is a notable instance of this in the rock of which your correspondent speaks, under the Glacier des Bois. * * * *

[Mr. Ruskin continues with further explanation of his views, in the course of which he expresses the opinion that "the Glacier des Bois has not done more against some of the granite surfaces beneath it, for these four thousand years, than the drifts of desert sand have done on Mt. Sinai;" and "it never digs a hole" like the bed of a lake. He closes with a paragraph containing the following statement as to his early tastes and studies.]

I find it difficult to stop, for your correspondent, little as he thinks it, has put me on my own ground. I was *forced* to write upon Art by an accident (the public abuse of Turner) when I was two-and-twenty; but I had written a "Mineralogical Dictionary" as far as C, and invented a shorthand symbolism for crystalline forms, before I was fourteen: and have been at stony work ever since, as I could find time, silently, not caring to speak much till the chemists had given me more help.—*Reader, Nov.* 26, 1864, *the communication dated Denmark Hill, Nov.* 21.

4. *Geological Survey of California;* J. D. Whitney, State Geologist. —Paleontology, Volume I: *Carboniferous and Jurassic Fossils*, by F. B. Meek; *Triassic and Cretaceous Fossils*, by W. M. Gabb. 244 pp. Small 4to, 1864. Published by order of the Legislature of California.—The principal results of the geological survey of California, up to the present time, have been briefly stated in our last volume at p. 256. Their number and importance are sufficient to show that the survey could not have been in better hands. Through the labors of Professor Whitney and his assistants, we are beginning to have some definite knowledge of the geology of the western border of the continent,—of the distribution of its rock-formations, of the age and structure of its mountains, of the origin of its metallic veins, of the action of rivers and glaciers in educing the features of the surface, besides many other points of geological and economical interest. The Report on the general Geology, as far as the subject is completed in the yet unfinished survey, will, we understand, be soon published. The volume just issued is the first of the two on the Paleontology of California. It treats of the fossils of the Carboniferous formation, the oldest fossiliferous beds yet observed, and of the Triassic, Jurassic and Cretaceous formations, leaving the Tertiary for the second volume.

The Carboniferous fossils make but a meagre list, the rocks of the age having small extent. Twelve or fourteen species are all that are described. Three of them are referred by Mr. Meek, though with some hesitation, to the widely spread *Lithostrotion mammillare, Productus semireticulatus* and *Spirifer lineatus.*

The Triassic species, described by Mr. Gabb, are 28 in number; the Jurassic, described by Mr. Meek, 12; the Cretaceous, described by Mr. Gabb, nearly 300, the larger part being new, and all pertaining, according to Mr. Gabb, to the Upper or White Chalk. Some of the general facts respecting the extent and character of the formations have already been published by us in the article in our last volume above referred to.

A volume of plates will soon be ready for delivery. It will contain figures of all the new species. The plates are lithographic, and thirty-two in number; and, as we know from an examination of many of them, are excellent in drawing and engraving.

The work is brought out in the first style of the art, both as to paper and printing. We know nothing superior, in these respects, from any American press.

5. *Oil region of Pennsylvania.*—[Extracts from a communication by Ira Sayles, dated Meadville, Crawford Co., Pa., Nov. 13, 1864.]—As shown in the Geological Map of Pennsylvania by Professor Rogers, the surface-rocks of the higher country in the oil region of western Pennsylvania are mostly Carboniferous, while those of the valleys appear to be Devonian of the Chemung and Portage groups. In the borings, a whitish sandstone, called the *first sand-rock* is met with at a depth of 70 to 200 feet; and wells penetrating only through this bed generally yield a thick oil. 100 to 200 feet below this, lies the *second sand-rock*, of similar aspect to the first; and from beneath this there is usually another "show of oil," a heavy oil, though less so than the former. From this rock proceed nearly all the wells of the Alleghany river, while the wells of French Creek, with few exceptions, descend only to the *first* sand-rock. Below the *second*, 100 to 200 feet, there is a *third* sandrock, and from beneath this comes the pure, limpid, light oil. To this depth descend most of the "flowing" or fountain wells, four of which, when first opened, (the Empire well, one of them,) deluged the region around with 3000 barrels of oil per day. The wells of Oil Creek are mostly in this rock.

The valleys have been selected in nearly all cases for the borings under the impression that breaks in the hills indicate chasms in the rocks beneath. But there appears to be no reason why oil should not be reached also by borings through the higher land. There are evidently two kinds of sources; open cavities affording the free flowing wells, sometimes intermittent; and the oil-bearing shales giving a slower yield, as if by a gradual percolation from rocks that partially retain it; and the latter at least should be encountered as well through the hills as the valleys, if the boring is carried down to the same level. Moreover, there is no certainty that fissures or chasms in the rocks may not underlie the hills.

It is a little remarkable that the oil from beneath the upper sandstones should be the heavier, while that from beneath the lowest is the lightest. Although there is no evidence of any connection between the oil repositories at these three different levels, the sources of the same level often communicate. This has been abundantly proved, by a sudden diminution in the supply of an older well in consequence of sinking a new one near by.

The quantity of oil produced now does not materially differ from that of two years ago—viz: nearly 6000 barrels daily. The number of wells has been increased in that time; but about as many old ones have been abandoned as new ones opened. Fewer of the new wells are great *spouting* wells than formerly. Pumping wells are very constant in their yield.

6. *Petroleum in California.*—A petroleum region on the coast of Santa Barbara, near Buenaventura, in Southern California, has recently been visited by Prof. Silliman, Jr., from whose Report we extract a few facts. Ten miles North of Buenaventura there is a mountain ridge, 2000 feet in height, extending for 13 miles from east to west, which consists in part of bituminous shales supposed to be of either the Cretaceous or Tertiary period. The dip of the layer is 70° to 80° to the north. From these shales, mineral oil comes out in many places, and at some points very abundantly. One of the wells is 30 feet in diameter, and is full of a tar-like oil, boiling with the escape of marsh-gas. There is also an area of asphaltum three-fourths of a mile long by half a mile wide, exuding tar and rock-oil at numerous points, besides several other oil springs, the places of discharge in all exceeding twenty. The range of bituminous shales extends to the south, occurring at intervals for a hundred and fifty miles, and also to the north as far as Glenroy, in Santa Clara County, or about 80 miles from San Francisco.—*Extract from Prof. Silliman's Report.*

III. BOTANY AND ZOOLOGY.

1. *Dioico-dimorphism in the Primrose Family.*—Mr. John Scott, late of the Edinburgh Botanic Garden, has communicated to the Linnæan Society an elaborate paper, entitled, *Observations on the Functions and Structure of the Reproductive Organs in the Primulaceæ*, which has recently been published in the 8th volume of that Society's Journal of Proceedings. Mr. Scott has followed up Mr. Darwin's well-known researches by many additional experiments, has confirmed his results, and brought out others of no small interest. The paper is long, and, on the whole, clear and satisfactory, but with occasional vagueness or obscurity of language, from the attempt of an acute, but perhaps rather untrained mind to indicate the bearings of the subject further than they have been clearly made out. A good summary appended to the paper sets forth the principal points.

Of 54 species or forms of Primula which he has examined (many of them only in the herbarium),—

"36 are truly dimorphic, presenting both long- and short-styled forms; 13 in which the long- or short-styled forms, respectively, have alone been observed by me; and 5 species and one variety with non-dimorphic characteristics, i. e., presenting stamens and pistils of an equal length.

"The allied genera *Hottonia* and *Aretia* have also truly dimorphic species; whereas other allied genera, *Dodecatheon*, *Soldanella*, and *Cortusa*, are very generally characterized by species presenting the structural characteristics of the long-styled form only, without, however, any decreased fertility arising from their hermaphrodite conjunctions.

"The general differences of the two sexual forms may be thus briefly summed up:—First, the long-styled forms have pistils equalling in length the tube of the corolla; stigmas usually larger and rougher; stamens attached to, or frequently below, the middle of the corolla-tube, whose diameter is thus expanded upward; pollen-grains generally smaller and more transparent. Secondly, in the short-styled form the pistil is short, not rising above half way up the corolla-tube; stigma generally smoother and depressed on the summit; stamens attached to the mouth of the corolla-tube, causing an abrupt expansion; pollen-grains generally larger and more opaque. According to all the trials, these structural differences are accompanied by equally remarkable functional differences,—the pollen of the long stamens being alone adapted to fertilize the long pistils, and the pollen of the short stamens to fertilize the short pistils. By applying, on the other hand, either form pollen to own form stigma, i. e. effecting a homomorphic union, the degree of fertility relatively to the above, or heteromorphic union, is greatly decreased. Analogous, though less striking, functional differences, however, occur without any appreciable change of structure, as shown by the *P. verticillata*, e. g., yielding a much higher grade of fertility by its diœcious than its hermaphrodite conjunctions. Such an instance from a genus whose members are so generally characterized by a sexual dimorphism, naturally leads me to regard it as indicative of the acquirement of similar characteristics. An objection to this view may be urged from the occurrence of species which, having no immediate affinity with any structurally dimorphic species, nevertheless present individuals incapable of fertilization by own pollen, though perfectly susceptible to reciprocal fertilization, either with another individual of the same species, or one of a distinct species. To this category, at least, those who disbelieve in the genetic affinities of organic beings will no doubt refer the case of *P. verticillata*, and simply regard it as further illustrative of our ignorance of the conditions upon which sterility, in its varied grades, depends. Those, on the other hand, who believe in the existence of these genetic relations will look with an intelligent interest upon these functional peculiarities of the *P. verticillata*, and regard them, mayhap, as the primary indications of a tendency to assume those remarkable sexual characteristics of the correlated species, and thus presenting an illustration of incipient dimorphism.

"The usual differences in the fertility of the heteromorphic and homomorphic unions will be best appreciated by giving the mean results from the unions of several species. Thus, taking the five heteromorphic and homomorphic unions given above, namely, *P. Auricula*, *Sikkimensis*, *cortusoides*, *involucrata*, and *farinosa*, we see, from the mean results of their combined products, that for every 100 seeds yielded by the heteromorphic unions, only twenty-four are yielded by the homomorphic unions —the heteromorphic thus exceeding the homomorphic unions in about the proportion of five to three! I have also shown the remarkable fact that the pollen of a distinct species will produce a much higher grade of fertility than an ordinary homomorphic union, i. e., a flower's own pollen!

"It is well known that A will fertilize B, and B will not fertilize A. I have given instances of this law with Primulas. I have also shown the new and remarkable fact, that of the two forms of the same species the pollen of the one, but not of the other, will fertilize a distinct species!

For example, the long-styled *P. Palinurii* can be fertilized readily by pollen of the long-styled *P. Auricula;* yet, after numerous trials, I have failed to effect a single union between the long-styled form of the *P. Palinurii* and the short-styled *P. Auricula.* How utterly inconsistent, then, are such facts with the teachings of those who would have us believe that an absolute causal relation exists between the sterility from hybridism and systematic affinity! On the other hand, how unequivocally do these cases show us that the greater or less facility of one species to unite with another is, as Mr. Darwin has sagaciously argued, 'incidental on inappreciable differences in their reproductive systems. And that there is no more reason to think that species have been specially endowed with various degrees of sterility to prevent them crossing and blending in nature, than to think that trees have been specially endowed with various and somewhat analogous degrees of difficulty in being grafted together in order to prevent them becoming inarched in our forests.'[1]

"Probably the most remarkable result from my observations is that when the dimorphic species cease to be dimorphic, their reproductive functions are greatly modified. Thus, in the case of the Cowslip, for example, we have seen that an ordinary homomorphic union yields about fourteen seeds per capsule, the heteromorphic about twenty-four seeds per capsule, whereas the form with stamens and pistils of an equal length yields, when fertilized with its own pollen, thirty-four seeds per capsule! Thus the non-dimorphic form by own pollen exceeds, first, the homomorphic unions in the proportion of 5 to 2, and secondly, the heteromorphic in the proportion of 3 to 2! Again, from the four different unions of the long- and short-styled forms with the non-dimorphic form, the seed-results in each case fall considerably below an ordinary homomorphic union: thus the mean results of the unions of the non-dimorphic with long- and short-styled are six seeds per capsule, whereas the pure homomorphic unions of the latter give an average of thirteen seeds per capsule—that is, as two to one!

"Connected with these are the remarkable changes in the fertility of the [differently] colored varieties of the Primrose, the red variety yielding no seed when fertilized by pollen of either yellow or white varieties: the reciprocal crosses of these, i. e. the pollen of the red variety applied to the stigmas of the yellow and white, are also absolutely sterile! On the other hand, fertile unions may be effected by the reciprocal crossing of the yellow and white varieties, though in every case we have found that the average seed-result of such unions is considerably under that of the pure unions of these forms.

"Whether or not the ultimate tendency of dimorphism is a complete separation of the sexes, I think we have the clearest testimony that dimorphism has not always been a genealogical characteristic; and furthermore, that the two forms did not *per saltum* assume these structural and physiological characteristics. I here allude to the evidence afforded by the non-dimorphic Cowslip—namely, the resumption of perfect hermaphrodism, and the occasional production of intermediate stages between this and the normally dimorphic. These, taking us back in the genealogical line, show us an original non-dimorphic progenitor, and the graduated plan by which it gave rise to a dimorphically characterized race."

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[1] Origin of Species, 3d ed., p 299.

The most noteworthy points are:—1. That, in a genus which has many species with dimorphous flowers, there are some which present, so far as known, only one of the two forms (say the long-styled, which is perfectly fertile *per se*), as well as others with stamens and pistil of equal length. 2. That the Red Cowslip, a clear variety of the common one, has become non-dimorphic, and, with this change of structure, has become much more fertile of seed than the heteromorphic unions of the Common Cowslip. And the Auricula offers a similar case. But, on the other hand, a non-dimorphic variety of *Primula farinosa* is less fertile. These changes in the genetic system of a species seem very remarkable. 3. Still more remarkable is the result of fertilizing the Red Cowslip with its ancestor the common yellow form; this union, and also that of the Common Cowslip fertilized by the Red, being very sterile;—thus supplying that *desideratum* which has been called for as a test, "a physiological species produced through variation." 4. That the two forms of a dimorphous species hybridize with very different degrees of facility with distinct species. And that such a cross sometimes yields more seed than either homomorphic unions with own pollen. 5. Mr. Scott confirms (though it is not stated in his summary) Mr. Darwin's statement, that the long-styled forms artificially fertilized with their own pollen are about twice as fertile as the short-styled similarly fertilized with its own pollen; while, as is well known, when left untouched, the short-styled is most fertile, owing to the relative position of the anthers and stigmas. So that the advantage of position in the latter is counterbalanced by an increased differentiation of pollen and stigma with respect to their mutual action, Nature thus rendering the short-styled equally dependent with the long-styled on external agencies,—thus keeping it in existence, and more completely effecting the great object of dimorphism,—the intercrossing of individuals.

We may here append the remark that the Thymelæaceous genus *Leucosmia* is dimorphous, and some species of *Drymispermum* exhibit one if not both of the two forms. A. G.

2. *Observations upon Dimorphous Flowers;* by H. VON MOHL; in *Bot. Zeitung*, Oct. 1863, translated in *Ann. Sci. Nat.*, *Bot.*, April, 1864.—These observations are principally upon that case of dimorphism in which, besides the ordinary hermaphrodite (but often infertile) flowers, there are other and surely fertile ones, of simplified structure, apetalous, or rather cryptopetalous, and which, "their development being as it were arrested in the bud," and fertilized without opening, we had long ago termed "precociously fertilized." Some remarks were made upon them in this Journal, in November, 1862, (p. 419), stating that Nature, in this case, takes as much pains to secure self-fertilization as she does in the other case of dimorphism (represented by *Primula*, *Houstonia*, &c.,) to secure cross-fertilization. This is the conclusion which Mohl also reaches after a careful investigation of the common instances, viz: in *Specularia*, *Impatiens*, *Viola*, *Oxalis*, &c., clearly showing that such flowers, with a possible exception in violets, must needs be self-fertilized. The record of his

own observations is preceded by a good history of what was known upon the subject, from the beginning made by Dillenius down to the communication of Michalet in the *Bull. Soc. Bot. de France*, 1860. That the little tympanum which covers the minute anthers and stigmas of *Specularia perfoliata* until after fertilization, is a rudimentary corolla, however, was shown by Dr. Torrey, in a paper read before the Lyceum of Natural History, New York, in 1830, but not published.

One of the most interesting things to notice, and a curious confirmation of the intention to self-fertilize in the cases here considered, is the unusual paucity and the greater activity of the pollen in the anthers of the small and closed flowers. In those of *Oxalis Acetosella*, as Mohl informs us, the larger anthers contain only about two dozen, the smaller scarcely a dozen pollen-grains,—only a few times more numerous than the ovules they are to fertilize, while in the normal blossoms they are in countless excess,—the economy in number being in proportion to the sureness that they will do their work. Moreover, these pollen-grains never leave the anther, but, (as was observed by D. Müller and by Michalet, but misunderstood, and neatly shown by Mohl,) send out their tubes to seek the stigmas, adjacent, indeed, but often at a considerable relative distance, these tubes directing their course with the same precision, and in the same mysterious manner, apparently, that they ordinarily do when having reached the ovarian cavity they seek the orifice of the ovules. This is well seen in *Viola*, where, though it is barely possible that extraneous pollen might sometimes be introduced, yet, in view of the immediate contiguity of the stigma to the line of dehiscence of the two more fertile anthers, through which the pollen-tubes are emitted, close-fertilization here cannot be doubted. So Mohl concludes that, "considering that these little flowers are constantly fertile; that in some plants they are the only ones that are fertile, while in others they are far more plentiful than the large flowers, and that upon these the propagation of the plant mainly depends, it results that it is not a general law in hermaphrodite flowers that Nature should favor fecundation by the pollen of another flower in preference to its own pollen." Mohl proceeds to state that self-fertilization is equally secured in some non-dimorphic flowers, and instances *Fumariaceæ* as a case where this is "absolutely necessary, the transport of pollen from one flower to the stigma of another appearing to be absolutely impossible, on account of the special structure of the corolla, and of the intimate connection of the exterior [interior?] petals which enclose the anthers and stigma." We had thought the same, although the nectariferous sacs should have made us pause; but Mr. Darwin called our attention to the great facility with which the cap or united tips of the inner petals would be pushed off to either side by a bee visiting the blossom and seeking honey from its nectary. To this it sufficed, as we thought, to answer, that pollen was actually falling from the anthers upon the stigma they surrounded a full day before the tips of the outer petals opened, i. e., before there could be any access whatever from without. The reply to this seeming demonstration duly came in Mr. Darwin's capital discovery that, in *Linum* and other cases, the pollen may be less potent or even impotent upon its own stigma, while that from another flower is prepotent! Still these little precociously-fertilized flowers, of various and diverse families, stand as veritable exceptions to the universality of the law which Darwin has ably deduced.

What causes the pollen-grains to emit their tubes without contact with the stigma, and indeed at a considerable distance from it, is an utter mystery. Mohl remarks here an analogy with the case of *Asclepias*, and that in *Viola*, *Specularia*, &c., as Brown has stated for *Asclepias*, there is no liquid poured out by the stigma which might somehow place the pollen in relation with the stigma.

Finally, Mohl calls attention to an early remark of Linnæus, that, in the year 1753, a considerable number of plants raised in the Upsal garden, for which the Sweedish climate was too cool, flowered clandestinely, yet produced fruit. We may note that in the Cambridge Botanic Garden, *Oxybaphus nyctagineus* copiously fruits from the bud in cool and cloudy weather: so do one or two Pavonias for a considerable part of the season. And, several years ago, *Nyctaginia capitata* of Texas seeded abundantly all one summer without expanding a single flower, or even producing a full-grown flower bud. The next year it blossomed freely.

Mohl justly notes that these diminutive bud-fruiting flowers are often produced earlier in the season, as in *Specularia*, *Impatiens*, &c., but sometimes later than the normal blossoms, as in *Oxalis* and *Viola*. And he shows that the former conform (he looking upon such flowers as preponderantly female) to Knight's hypothesis, (drawn from cucumbers and melons,) that a high temperature favors the development of male flowers, while a lower temperature is more propitious to female blossoms, —a view which Thury took as the starting point of his interesting investigation upon the production of sexes in the animal kingdom. But Naudin has appended to the French translation of Mohl's paper, an important note, in which he declares that his sedulous observations upon *Cucurbitaceæ* for the past ten years are far from confirming Knight's theory; that in this family male and female blossoms appear independent of conditions of temperature, but intimately connected with peculiarities of temperament, which vary from species to species, from race to race, or even among individuals of the same race. And he notes especially (what we suppose applies generally) that the races of melons, squashes, and gourds which have long been cultivated in northern Europe are comparatively more precocious, and need much less heat for maturing their fruit, than those of the same species which have been recently brought from tropical regions. Also that the same *Cucurbitaceæ* arrive at adult age, i. e., to the age for flowering and fruiting, far sooner under the hot and cloudless sun of the south of France than under the cooler and obscurer skies of Paris, where they vegetate richly, but hardly blossom. Naudin remarks, also, that many monoicous *Cucurbitaceæ* tend to become dioicous, without assignable cause; and, as especially fatal to Knight's view, that *Coccinia diversifolia*, cultivated simultaneously at Paris and at Hyères, in a great number of individuals, has produced only male blossoms at Paris, but an abundance of both sexes (monoicous) at Hyères. A. G.

3. *Najas major*, *Ruppia maritima*, &c., *discovered at Salina*, *N. Y.* —The September number of Dr. Seemann's *Journal of Botany* contains a *Revision of the Genus Najas*, by Professor Braun of Berlin. To the section *Eunajas* three species are assigned, viz., *N. major*, All., *N. muri-*

cata, Del., of Egypt, and *N. latifolia*, Braun, from the lake of Valencia, Venezuela. The section *Caulinia* has *N. flexilis*, which is found here and there in the North of Europe, and several varieties in the West Indies, Venezuela, &c., *N. arguta*, H.B.K., in New Granada, *N. minor*, All., in Europe, Egypt, and India, *N. fasciculata*, Braun, in India, and *N. graminea*, Del., in India, Egypt, &c. *N. major*, to which our attention is at present directed, under several varieties, is widely distributed throughout the Old World, in slightly brackish or fresh water, but it was also detected by Chamisso in the Sandwich Islands. What is equally remarkable, Prof. Braun has seen specimens collected in Florida by Cabanis.

Shortly before Prof. Braun's article came to hand, *Najas major* was discovered in Onondaga Lake, first by Judge G. W. Clinton, on the northern border of the lake (between Salina and Liverpool), and soon after by Mr. John A. Paine, Jr., on its western side, growing luxuriantly and abundantly, and quite fruitful. The marshy borders of the lake, as is well known, are brackish, at least in the vicinity of the salt-works, and abounding in maritime and submaritime plants. To the species formerly known there, the above gentlemen have added *Chenopodium glaucum*, *Leptochloa fascicularis*, and we believe some others. But a more interesting discovery, of a water-plant not before met with away from the sea coast, was recently made by Mr. Paine, he having detected *Ruppia maritima* in Onondaga Lake, along with *Najas major*, equally in a fruiting state.

A. G.

4. *Spontaneous return of hybrid plants to their parental forms.*—We have been prevented from preparing, as we wished to do, abstracts of the two rival essays upon Hybridity in Plants, viz., that of Mr. Naudin, of the Jardin des Plantes, and of Dr. Godron, of Nancy. The latter was published in full in the *Annales des Sciences Naturelles*, 4th series, volume xix, and also a part of the former, the whole memoir of Naudin, with details of the experiments and figures, having been accepted for publication in the *Mémoires* of the French Academy. The prize for which the essays competed were awarded to Naudin. A brief account of the two essays appeared a year ago in the Natural History Review. As respects one, the principal question, to use the summary of the Review, "the general result is, that Dr. Godron says: simple hybrids are absolutely sterile. Mr. Naudin says: hybrids are fertile whenever their anthers contain well organized pollen. Of 30 to 40 hybrids experimented upon, three-fourths were found to produce seeds capable of germination."

Upon another point of equal interest, Naudin's testimony is explicit, he maintaining that hybrid plants, however constant at first, tend in subsequent generations to a separation of the two specific elements, which are, as he expresses it, rather intermixed than truly combined, so that they would at length resolve themselves into the two parental types, or, by failure on one side, return to the one on the other. Naudin returns to this subject in a note in the *Flore des Serres* for July, 1864, where he gives the result of his experiments upon our common sorts of thorn-apple, *Datura Stramonium* and *D. Tatula*. These have more commonly been taken for varieties of one species; but their specific distinction has been maintained, especially of late, by various arguments. According to Naudin they are truly distinct species, which do not sensibly vary.' One always exhibits green stems and pure white flowers; the other, dark-purple stems and violet-tinged flowers. These two thorn-apples Mr. Naudin crossed, in 1855, and obtained a hundred or more hybrids, both *Tatula-Stramonium* and *Stramonio-Tatula*, both just alike, and exactly intermediate between the two species in the coloration of the stems and flowers. They had, however, the peculiarity of a gigantic size, attaining at least twice the size of their parents, and a tendency to sterility, which was manifested in the failure of all the flower-buds which were produced at the first forkings of the stems. The later flower-buds opened, however, and were, perfectly fertile, the pods being as large and as full of good seeds as those of either parent. In 1861, these seeds (of *Stramonio-Tatula*) were sown, and produced a second generation like the first. Seeds of this crop were sown in 1862, and twenty-two seedlings were preserved for experiment. Five of these developed as *D. Stramonium* in all its purity, i. e., had green stems, white flowers, only the ordinary height, and all the flowers fertile, ripening their pods in the first forks, three months earlier than did their immediate parents. Nine individuals returned as completely to *D. Tatula* as the five did to *D. Stramonium*. Two others seemed to be *D. Tatula*, and were equally reduced in size and fertile from the first forks, but they still showed in their paler coloring a trace of the other ancestor. The remaining six of the twenty-two showed somewhat more of it, both in color and in the tallness and lateness of fructification; expressed numerically, Naudin estimates that they were, say, nine-tenths *D. Tatula* and one-tenth part *D. Stramonium*. "Here, then," says Naudin, "is a hybrid completely intermediate between the two parent species, which, left to itself, fecundated by its own proper pollen, is spontaneously dissevered at the second generation, dividing its offspring between the two species. And it is to be remarked that the division is very unequal, *D. Tatula* taking the lion's share, totally or nearly reclaiming seventeen individuals out of the twenty-two. This unequal division is common in hybrids, and sometimes goes to the extreme, one of the parents wholly disappearing from the hybrid progeny, which thus passes over entirely to the other species."

As to crossing species and their hybrids, in view of obtaining diverse and particular varieties, Naudin shows, accordingly, that, to obtain the desired results, the hybridizer should take pains to cross those mongrels which tend toward one parent with those which tend to the other, thus opposing the tendency to return to the specific types, which would otherwise surely take place. By doing this for a few generations, the two hereditary tendencies become, as it were, thoroughly confused, and a multitude of new variations break out in all directions, from which the amateur may select what he likes for preservation and further experiment. This is according to a principle first expounded by that excellent investigator, the late Louis Vilmorin. A. G.

5. *Flora of the British West Indian Islands*; by A. H. R. Grisebach, M.D.; Professor of Botany in the University of Göttingen. London, Lovell Reeve & Co. 8vo, parts VI and VII, 1864.—These parts, extending from p. 507 to p. 789, complete this most valuable work, which, as was intended, closes with the Vascular Cryptogamia. A full index of the

species is appended, and a list of colonial names. The preface gives an account of the circumstances under which the work was undertaken, and of the materials which the author has so sedulously and promptly elaborated. West Indian botany was very difficult and confused: "Almost all the principal authors who have written on West Indian plants belong to the last century, and consequently to the Linnæan school, and a general synopsis of West Indian plants has never before been attempted, not even by Swartz, whose Flora contains descriptions of his new species only, with a few remarks on allied forms." Moreover, the British West Indies offer only the separate fragments of a larger flora; and Trinidad, as its geographical situation indicates, naturally belongs to the flora of Venezuela and Guiana. The northern Bahamas might be supposed to have a vegetation very like that of East Florida, from which they are separated by the Gulf Stream; but this seems not to be the case. "Jamaica, again, from its mountainous character and more distant position, most of the leeward islands, from being wooded volcanos, and the majority of the windward ones, with a dry climate and a low calcareous soil, form three divisions of this tropical archipelago, which show as many peculiarities. Thus the whole of the British West Indies, as comprised in this flora, may be divided into five natural sections, each with a distinct botanical character." Altogether, they amount to about 15,000 English square miles, or nearly twice the area of Wales. But yet Hayti alone is nearly twice, and Cuba nearly thrice, as large as all the British Islands together, and not only far richer in vegetation, but far less explored, the publications of Jacquin, Swartz, &c., having been almost confined to the British possessions; so that it was with old species mainly, that Dr. Grisebach had to deal, those which were "the foundation, indeed, of our scientific knowledge of the flora of tropical America." And these "have so often been misunderstood that their synonyms are far more numerous than their numbers." A general West Indian Flora being out of the present question, we learn with interest that Dr. Grisebach is preparing a special paper on the geographical range of the West Indian plants, including the capital island of Cuba, which Mr. Charles Wright has so industriously and successfully explored through its length and breadth, and is expecting still further to explore.

We may here add the remark that Mr. Wright's third distribution of phænogamous plants and ferns—the fruits of his labors for the last three years—has just been made, consisting, in the fullest sets, of about 1800 species or numbers, (including some which have been redistributed under old numbers,), and that the greater part of them have already been determined by Dr. Grisebach, the now recognized authority in West Indian botany. An equally rich field probably remains in Hayti, to tempt an adventurous explorer. The Bahamas, also, bare as they seem to be, "have proved very interesting," and are thought to promise still many novelties.

The series of Natural Orders adopted in this work being different from the ordinary ones, and especially from that of the other colonial floras, mainly owing to the intercalation by Dr. Griesbach, of the apetalous or reduced forms among the polypetalous orders—where many of them clearly belong,—the author has added a tabular arrangement more in accordance with the Candollean series, and with that employed in the new *Genera Plantarum* of Bentham and Hooker, which will be the natural companion of these colonial floras. These volumes are all written in English; and it is interesting to see how nearly at home the Göttingen Professor makes himself in a language foreign to him. A. G.

6. *Florula Australiensis: a Description of the Plants of the Australian Territory*, by GEORGE BENTHAM, F.R.S., P.L.S., assisted by FERDINAND MULLER, M.D., F.R.S. & L.S., etc., vol. II, *Leguminosæ* to *Combretaceæ*. 1864. pp. 521, 8vo.—That the second volume of this important work has been brought out so soon after the first, may be attributed, not merely to the indomitable industry and force of the author, but also to the fact that four-fifths of its pages are devoted to the great order *Leguminosæ*, to which Mr. Bentham has previously devoted much attention. In fact, to him the reformation of the order from the state in which DeCandolle left it, is almost wholly due. He first indicated, we believe, the floral characters which distinguished the Cæsalpineous from the Papilionaceous suborder, settled the tribes of the latter upon their present basis, and, besides various special memoirs, revised the *Mimoseæ* several years ago in a complete monograph. The huge genus *Acacia* is most largely Australian, where, indeed, of all Phænogamous genera it is most numerous in species; and the Phyllodineous Acacias are almost exclusively Australian. The present flora describes 293 species of *Acacia*, of which 271 are of the Phyllodineous section. These are so neatly analyzed by a tabular key, of the sort which Mr. Bentham makes so much of,—that the crowd of specimens in the herbarium may be most readily determined and arranged. *Chrysolalanceæ* are here recombined with *Rosaceæ*. *Saxifrageæ* (we know not why the form *Saxifragaceæ* is refused, and *Saxifrageæ* therefore used as the name both of an order and of a tribe) are well made to follow immediately after *Rosaceæ*; then come *Crassulaceæ*, of course; and *Droseraceæ*, retained as a good order. The volume is smaller than its predecessor, closing with *Combretaceæ*, not to sever the *Myrtaceæ*, so largely represented in Australia. A. G.

7. *Upon a form of budding in some Insect Larves.*—In the last number of the *Zeitschrift für wissenschaftliche Zoologie* are two articles of interest,[1] upon a form of multiplication by budding occurring in some larves of dipterous insects, and also containing a description of a new genus and species of Cecidomyians founded on one of the insects in which it was observed. It seems that Nicolaus Wagner published, in the memoirs of the Kazan Institute for 1862, an account[2] of a larval multiplication by a sort of internal budding, and that C. E. v. Baer confirmed the discovery last year in the 6th vol. of the St. Petersburg Bulletin, supplementing it with the full history of the larve, of its development into a perfect insect, and of the deposit of a few large eggs thus fructified. In July of 1864, Dr. Meinert published in the Danish *Naturhistorisk Tidsskrift* an account

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[1] Weitere Erläuterungen über die von Prof. Nic. Wagner beschriebene Insectenlarve, welche sich durch Sprossenbildung vermehrt, von Fr. Meinert. Aus dem Danischen mit Bemerkungen übersetzt von C. Th. v. Siebold.

Die ungeschlechtliche Vermehrung der Fliegenlarven von Prof. H. Alex. Pagenstecher. Zeits. f. Wiss. Zool. xiv: 4 Oct., 1864.

[2] This was noticed in the Zeitschrift f. Wiss. Zool., xiii, 513, but the writer of this has been so situated that it wholly escaped him, and he has not yet seen that notice.

of the same facts as observed by him, and also a description of the perfect insect as a new genus and species, with the name *Miastor metraloas.* The first of the two articles cited above, is a translation of a part of Dr. Meinert's paper, with some remarks by Dr. Siebold, the editor, which, he says, was just completed when he received the second article. This latter is a well illustrated and detailed account by Dr. Pagenstecher of a similar larval multiplication occurring in another species of dipterous insect.

It will be sufficient here, after calling attention to these articles, to give the principle fact confirmed in them. This is, that an insect larve has been seen to have egg-like bodies within the abdominal cavity; that these have been seen to develop into larves, which finally bursting out became free and resembled in every way the stock larve; that then, or perhaps after a second similar larve-brooding, the larves developed into a perfect dipterous insect which laid eggs and started anew the circle of species-life; and lastly, that this is seen in more than one species.

There are some points not altogether agreed upon by the observers above referred to. One is as to the place and the material at and from which these larves are formed in the larve-stock. Dr. Meinert describes them as formed from the granular fatty mass of the abdominal cavity, which he regards as a sort of remnant of the embryonic yolk division. Dr. Pagenstecher, on the contrary, says he saw none of these fat cellules in the way of developing into eggs, and thought these latter wholly different from any cells approaching their size, and sums up as follows: "In some unknown part of the body, cells are set free, which, without being specially fructified, assume the character of eggs. These develop into embryos that exist in a sort of pupa state within the mother-body, and develop at the expense of the parent structures. Finally they break forth, the larve-stock dying, and become independent themselves."—pp. 414–5.

W. C. M.

IV. ASTRONOMY.

1. *Discovery of a new Planet, Terpsichore,* (81).—Mr. Tempel, at Marseilles, discovered on the 30th of September a planet equal in brightness to a star of the 10th magnitude. The following elements are given by Mr. Tietjen:

Elements of Terpsichore (81).

T = 1864, Oct. 23·0, B. m. t. i = 8° 45′ 43″·0.
M = 351° 31′ 52″·0, φ = 7 32 35 ·4.
π = 23 40 12 ·1, μ = 765″·494.
☊ = 2 48 29 ·3,   log. a = 0·444043.

2. *New Comet.*—Another Comet was discovered on the 9th of September by Mr. Donati, at Florence. It was very faint, and seen with difficulty. Mr. Celoria, of Milan, gives the following elements, computed from observations of Sept. 9th, 11th, and 13th:

T = 1864, July, 27·8829 m. t. of Milan.
☊ = 175° 11′ 57″·4,
π = 190 10 34 ·7,
i = 44 56 53 ·6,
log q = 9·787184, motion retrograde.

The comet, according to these elements, passed its perihelion before those heretofore known as Comet I, 1864, and Comet II, 1864.

3. *Numbers of stars in the northern hemisphere.*—In the last edition of his *Wunder des Himmels*, Prof. Littrow gives a summary of the numbers of stars that are in Argelander's charts of the northern hemisphere.

From N. declination, 0° to 20°, 110,987 stars.
" " 20° to 40°, 105,082 "
" " 40° to 90°, 108,131 "

Classified according to magnitude there are,

Mag.	No. of Stars.	Mag.	No. of Stars.
1–1·9	10	6–6·9	4,328
2–2·9	37	7–7·9	13,593
3–3·9	128	8–8·9	57,960
4–4·9	310	9–9·	237,544
5–5·9	1,016		

There are, besides these, 60 nebulæ and 64 variable stars.—*Heis' Wochenschrift, Sept. 28th.*

4. *On the age of the moon's surface*; by J. NASMYTH.—The views I entertain on the subject in question are these, namely, that, as a direct consequence of the small mass of the moon, and its comparatively large surface, it must have parted with its original cosmical heat with much greater rapidity than in the case of the earth, and consequently the moon must have assumed a final condition of surface-structure ages before the earth had ceased from its original molten condition. And as the moon in all reasonable probability never possessed an atmosphere or water envelope (it certainly has none such now), while the earth has both, the action of the earth's atmosphere, and especially that of its ocean when it existed in the first instance as a vast vapor envelope, ere the earth had cooled down so as to permit the ocean taking up its final position *as an ocean*, this mighty vapor envelope must have retarded the escape into space of the cosmical heat of the earth millions of ages after the moon had assumed its final condition as to temperature.

Therefore it is from such considerations I am led to the conclusion that the surface features and details of the moon present to us a sight of objects the antiquity of which is so vast as to be utterly beyond the power of language to express, and scarcely less so for the mind to conceive.

Such considerations appear to me to enhance so vastly the deep interest which ever attends the examination and contemplation of the moon's wonderful surface, that I would earnestly urge those who agree with the soundness of these views to bear them in mind the next time they have an opportunity to behold the marvellous details of the lunar surface, as I am fain to think that in doing so the interest of what is there revealed to them will be rendered vastly more impressive.—*Proceedings Manchester Lit. and Phil. Soc.*, Nov. 15, 1864.

5. *On the probable error of a Meridional Transit-observation, by the eye-and-ear, and Chronographic method;* by EDWIN DUNKIN, Esq.—At the close of an elaborate discussion of the observations made at the Royal Observatory at Greenwich, in 1853 and 1857, presented to the Royal Astronomical Society in May, 1864, Mr. Dunkin says:

In laying before the Society the preceding abstracts, which are the results of a great mass of computations, I consider that I can with some confidence offer the following conclusions, which are derived from this discussion:—

(1.) In "*eye-and-ear*" observations, the probable error of a Greenwich transit observed in 1853 over *one wire* is $\pm 0^{s}{\cdot}078$, while that of a *complete* transit over the seven wires is $\pm 0^{s}{\cdot}029$. In *chronographic* observations, the probable error of a Greenwich transit observed in 1857 over *one wire* is $\pm 0^{s}{\cdot}051$, and that of a *complete* transit over the nine wires is $\pm 0^{s}{\cdot}017$.

(2.) There does not appear to be any certain difference in the probable error of transits of stars between the first and sixth magnitudes.

(3.) In "*eye-and-ear*" transits, for stars whose N.P.D. is greater than 60°, it would seem that the probable error of a transit increases slightly as the N.P.D. decreases; while in the *chronographic* transits the corresponding changes are insignificant.

(4.) In "*eye-and-ear*" transits, the personal discordances are liable to a considerable variation between the different observers; in *chronographic* transits, the differences between the observers are comparatively small. The general steadiness of observing by the latter method is very remarkable, and shows the great advantage obtained by its adoption.

(5.) The probable error of a Greenwich result for Right Ascension in the year 1853, as determined from "*eye-and-ear*" transits, is $\pm 0^{s}{\cdot}048$; while the corresponding probable error resulting from *chronographic* transits, in the year 1857, is $\pm 0{\cdot}034$. By arranging the separate results into groups, according to the stars' magnitude, it is found that no certain difference can be distinguished in the accuracy of the observed transits, excepting only that there is a tendency in both methods towards an increase in the probable error when transits of stars of the first magnitude are observed. The amount of increase in this instance, however, is in reality small, and is probably to some extent accidental. H. A. N.

5. *Shooting Stars in November*, 1864.—Arrangements were made at various places in the United States to watch for the shooting stars on the mornings of Nov. 13th and Nov. 14th. The sky was entirely overcast in most places, and nearly so in all. Through openings in the clouds, which would last a few minutes, occasional stars were seen, but whether there were more than usual it is impossible to determine. There was certainly nothing like the great display of 1833.

In San Francisco the sky was overcast. It is possible that some parts of California were favored with clear skies, and that we may yet obtain observations from them. Mr. Quetelet writes that a similar clouded sky prevented observation in Brussels. H. A. N.

6. *Supplementary note to the article on the Nebular Hypothesis.*—The form of Mars, as determined formerly, did not seem to agree with what we might expect from the primitive fluidity of the planet. The attention of astronomers having been recently called to the ellipticity of Mars, it seems now to be improbable that it has so large an ellipticity as has been heretofore received. (*This Journal*, [2], xxxviii, 436.) We thus see that this unconformable case is likely to yield to the requirements of the nebular hypothesis. We may venture to add that it is extremely improbable that Mars has an ellipticity differing much from that of the earth. D. T.

VI. MISCELLANEOUS INTELLIGENCE AND BIBLIOGRAPHY.

1. *National Academy of Sciences.*—The Report of the National Academy of Sciences, made by the Academy to the General Government, for the year 1863, has been published. It forms an octavo volume of 118 pages, with several maps. It contains only such Reports as had been made by committees of the Academy on the call of the General Government, with a brief statement of the operations of the Academy during the year 1863. The more important contents are a Report on the protection of bottoms of iron vessels from corrosion, etc.;—Report of the chairman of the Compass committee appointed to make experiments for the correction of local attraction in vessels built partly or wholly of iron;—Report of the committee appointed to examine the "Wind and Current Charts" and "Sailing Directions" issued from the Naval Observatory.

At the last meeting of the National Academy, held in August at New Haven, Ct., three vacancies in its list of members were filled by the election of Dr. J. C. Dalton, of New York, Leo Lesquereux, of Columbus, Ohio, and S. F. Baird, Assistant Secretary of the Smithsonian Institution.

2. *American Philosophical Society.*—The *Magellanic premium* of a gold medal has been awarded by the Philosophical Society to Mr. Pliny Earle Chase, for the discovery of numerical relations between gravity and magnetism.

3. *Medals of the Royal Society.*—At the Anniversary meeting of the Royal Society, on Wednesday, the last day of November, the Copley medal was awarded to Charles Darwin, Esq., F.R.S., "for his important researches in Geology, Zoology, and Botanical Physiology;" and the Rumford medal to J. Tyndall, F.R.S., for his researches on the Absorption and Radiation of Heat by Gases and Vapors.

4. H. R. SCHOOLCRAFT.—This distinguished traveller, and investigator of the manners, history, and language of the American Indian tribes, died at Washington on the 19th of December, in the seventy-second year of his age.

5. F. W. STRUVE, the celebrated astronomer of Pulkowa, Russia, died on the 23d of November last. His son, Otto Struve, was appointed his successor last year.

6. *Thoughts on the Influence of Ether in the Solar System, its relation to the Zodiacal Light, Comets, the Seasons, and periodical Shooting stars.* By ALEXANDER WILCOCKS, M.D. Extracted from the Transactions of the American Philosophical Society.—Dr. Wilcocks advances the hypothesis that there is a constant circulation of ether induced by the heat of the sun; that the cold ether descends near the poles of the sun, and that heated ether ascends from the sun's equator, or rather from a parallel of solar latitude just north of the equator; that this heated current as it rises is compressed by the cold ether into a thin conical sheet; that the ether bears with it matter capable of reflecting light, and thus causes the zodiacal light; that the tails of the comets are lighter than the ether, and arise for that reason from the sun, except when near the poles of the sun, when they are made to descend in the vortices of descending ether, so as to present double and multiple tails; that twice in the year, in August and November, the earth plunges through the sheet of warm

ether, causing a warm period in each month, the dog days and the Indian summer; and that in the middle of these two periods it causes the return of the August and November meteors.

Dr. Wilcocks has brought together and framed into a plausible hypothesis many facts, some of them hitherto not connected with any theory. But we do not believe that astronomers and physicists will be likely to accept his explanation. What he says relative to the seasons, and to the shooting stars seems peculiarly open to criticism. Why, for instance, the two periods of heat (supposing them to have an existence,) should not, according to his hypothesis, be equi-distant from the 5th of June and 5th of December, the two days when the earth is in the sun's equator, he does not explain. For the middle day of one period he names the 11th of August, which is 67 days from the 5th of June, while that of the other, which he calls the 12th of November, is only 24 days from the 5th of December. In what way the periodical star-showers could possibly be produced by such an ether we find it difficult to imagine. H. A. N.

7. *Report of the Superintendent of the Coast Survey, showing the progress of the Survey during the year* 1862. xx and 434 pp., 4to, with 49 diagrams and charts. Washington, 1864.—Besides the General Report on the Survey and its maps, there is the usual Appendix of Special Reports relating to various collateral physical observations. The subjects include—Tide tables for mariners; Cotidal lines for the Gulf of Mexico; Longitude of America from Europe, by Prof. Peirce; Lunar tables by the same; Longitudes in Maine, Alabama and Florida, by B. A. Gould; various series of Magnetic observations; Solar spots; Bessel's periodic functions; calculation of the depth of the Pacific from the earthquake wave of Simoda; Origin of the Florida Reef, by E. B. Hunt, etc.

8. *Eulogy on Joseph S. Hubbard;* by B. A. Gould. 44 pp., 12mo.—This eulogy was written in compliance with the call of the National Academy of Sciences, which in its constitution provides for such a tribute in commemoration of each member that may be removed by death. Prof. Gould had in his friend a congenial and inspiring theme. High mathematical powers combined with astronomical tastes, an energetic, investigating mind, and a frank and genial disposition tempered by a true Christian spirit, had accomplished much for science. The tribute is admirable as a biographical notice, appreciative, eulogistic but not beyond justice, and of special value as a chapter in the history of American astronomy.

9. *Chambers's Encyclopedia.*—A new volume—the Sixth—of this popular Encyclopedia, a work well filled out and illustrated in the departments of science, as well as in other varied branches of knowledge, has just been issued by the American publishers, J. B. Lippincott & Co., of Philadelphia. The volume (826 pp.) carries the alphabet nearly through N.

10. *Abstracts of Meteorological Observations made at the Magnetical Observatory, Toronto, Canada West, during the years* 1854 *to* 1859, *inclusive.* 136 pp. 4to. Toronto, 1864. *Results of Meteorological Observations made at the Magnetical Observatory, Toronto, during the years* 1860, 1861 *and* 1862. 84 pp. 4to. Toronto, 1864.

11. *Review of American Birds;* by S. F. Baird.—The publication and issue of this work has already reached the 148th page.

12. *Monograph of the Bats of North America;* by H. Allen, M.D.—Smithsonian Miscellaneous Collections, No. 165. Washington. 86 pp., 8vo, with numerous wood-cuts.—The species included in this important monograph are those of North America north of Mexico.

13. *American Journal of Conchology.*—A prospectus has just been issued by G. W. Tryon, Jr., of Philadelphia (625 Market st.), proposing the publication of a Quarterly Journal of Conchology, and soliciting subscriptions. Price ten dollars a year; single numbers three dollars. We commend it to all interested in the progress of science.

The Plurality of the Human Race; by G. Pouchet. Translated from the French by H. J. C. Beavan. London, 1864. Published for the Anthropological Society.

The Phenomena of Hybridity in the genus Homo, by Dr. Paul Broca. Published for the Anthropological Society.

Reptiles of British India; by Albert C. L. Gunther. Published by the Ray Society. 1864.

A History of the Spiders of Great Britain and Ireland; by John Blackwall, F.L.S. Published by the Ray Society. 1864.

Metamorphoses of Man and the lower animals (in French); by A. de Quatrefages. Paris, 1864.

Beschreibung und Eintheilung der Meteoriten; von Gustav Rose. 162 pp., 4to, with 4 plates. Berlin, 1864.

Meteorologische Waarnemingen, of the Netherland Meteorological Institute, for 1863. 326 pp., 4to. Utrecht, 1864.

Proceedings of the American Academy of Arts and Sciences, Vol. VI.—Page 97, On the Aluminates of Baryta; *W. P. Dexter.*—p. 106, Embryology of Asteracanthion berylinus, &c., with a plate; *A. Agassiz* [noticed at p. 130, vol. xxxviii, of this Jour.]—p. 114, On the velocity of light and the Sun's distance; *Lovering.*—p. 143, On the observed motions of the Companion of Sirius; *T. H. Safford.*—p. 166, Analysis of a Dacotah Meteorite; *C. T. Jackson.*—p. 169, On the new form of the Achromatic Object-glass introduced by Steinheil, with a plate; *Bond.*—p. 177, List of new Nebulæ seen at the Observatory of Harvard College; *Bond.*—p. 182, On Streptanthus and the plants referred to that Genus; *A. Gray.*—p. 188, Revision and arrangement (mainly by the fruit) of the North American species of Astragalus and Oxytropis; *A. Gray.*—p. 241, On the causes of Insanity among women; *Storer.*—p. 242, On the R. A. of the pole star as determined from observation; *T. H. Safford.*—p. 251, On a process of organic elementary analysis by combustion in a stream of oxygen gas; *C. M. Warren* [see last vol. of this Jour., p. 387].—p. 263, Observationes Lichenologicæ; *E. Tuckerman.*—p. 288, Investigation on the best approximate representation of all the mutual ratios of *k* quantities by those of simple integers; *Oliver.*

Proceedings of the Acad. Nat. Sci. Philadelphia. No. 4, Sept. and Oct. 1864.—Page 181, Limits and relations of the Raniformes; *E. D. Cope.*—p. 183, Description of a Gar-pike supposed to be new; *A. Winchell.*—p. 186, New genera and species of N. A. Myriapoda; *H. C. Wood, Jr.*—p. 187, Note on the Paralepidoids and Microstomatoids, and on some peculiarities of Arctic Ichthyology; *T. Gill.*—p. 189, Synopsis of the Cyclopteroids of eastern N. A.; *id.*—p. 194 and p. 214, Synopsis of the Pleuronectoids of California and Northwestern America; *id.*—p. 199, on the affinities of several doubtful fishes; *id.*—p. 208, Note on the family of Stichæoids; *id.*—p. 211, Notes on shells, with descriptions of new fossil species and genera; *T. A. Conrad.*—p. 224, On the characters of the higher groups of Reptilia squamata, and especially of the Diploglossa; *E. D. Cope.*—p. 231, On a blind Silurid, from Pennsylvania; *E. D. Cope.*—p. 234, Fasti Ornithologiæ; Obituary of P. L. S. Müller; *J. Cassin.*

Proceedings of the Essex Institute, Salem, Mass.—Vol. III, includes years 1860 to 1863. Vol. IV commences with the year 1864. No. 3 of Vol. IV, (July, August and September,) contains the conclusion of *J. A. Allen's* Catalogue of the Birds of Massachusetts, and a paper on the habits of some species of Humble Bees, by *F. W. Putnam.*

Journal of the Portland Society of Natural History, Vol. I, No. 1. Portland, Maine.—This number is occupied by E. S. Morse's paper on the Terrestrial Pulmonifera of Maine, noticed in the last volume of this Journal (p. 303).

THE

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[SECOND SERIES.]

ART. XVI.—*On Terrestrial Magnetism, as a mode of Motion;* by PLINY EARLE CHASE, M.A., S.P.A.S.[1]

IN a note to a former communication, I expressed my belief that the British Astronomer Royal would find in the mechanical action of the sun's rays, the precise "occasional currents" for which he was seeking, as the probable cause of magnetic storms. Mr. Airy has recently sent me a copy of his very interesting paper (*Trans. Roy. Soc.*, 1863, Art. XXIX), and its perusal has greatly strengthened this belief.

All of my meteorological views rest upon the hypothesis, that the atmospheric changes, whether of humidity, temperature, pressure, electricity, or magnetism, are purely mechanical; and that, being controlled by the laws of motions, their proper explanation does not require the assumption of any peculiar magnetic or electric fluid, but that a single homogeneous, elastic, and all-pervading æther may be both the source and the receptacle of all the various forms of force. In its principal features, this theory harmonizes with the now generally accepted belief in the mechanical origin of light and heat, but in its details it involves some new and interesting special applications, which I have endeavored partially to develop.

It will be readily seen, by a reference to my communication of April 15 (*Proc. A. P. S.*, ix, 367, *et seq.*), that the *mechanical* action of the currents to whose *electric* action Ampère ascribed the origin of terrestrial magnetism, produces two opposite spirals in the air and æther,—the lower moving from the poles to the equator, and against the earth's rotation; the upper from the equator to the poles, and in the same direction as the earth's rotation; the two being connected by innumerable currents of convection, or threads of ascending and descending particles. It will also be evident that at every place there are two principal sets of such double spirals, one with an axis perpendicular to the earth's radius vector, producing a maximum disturbance in the early afternoon, and the other more stable and uniform, with an axis passing through the nearest poles of greatest cold. In addition to the mutual perturbations of these two principal polarizing currents, the rolling of the luni-tidal attraction-wave produces at every instant a greater or less derangement,[1] and I find that the ratio of the lunar-barometric to the lunar-magnetic disturbance, (4·384), is nearly identical with Mr. Welsh's determination of the moment of magnetic inertia (4·4696; *Phil. Trans.*, cliii, 297). From a variety of considerations, it appears that the mechanical polarity or magnetic force thus engendered is a third proportional to two other forces, which may be called, respectively, central and tangential.

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[1] From the Proceedings of the American Philosophical Society, Oct. 21, 1864.

The communication which was presented at the meeting of Oct. 7, in its exhibition of the first numerical relationship that has even been pointed out between the barometric and magnetic fluctuations, showed that A : B : : B : M, a proportion in which A represents a central, B a tangential, and M a magnetic force.

I find a similar proportionality in each of Mr. Airy's summary tables (Op. cit., p. 627, *seq*). Thus, in his "Table II, Algebraic Sums of Magnetic Fluctuations (in terms of Horizontal force) for each Year, from 1841 to 1857, including all Days of Record of Great Magnetical disturbance," the Mean Disturbance is

Westerly force.	Northerly force.	Nadir force.
− ·00028=M.	− ·00146=T.	− ·00057=C.

Here the proportion T : C : : C : M gives for M a

Theoretical value, ...	− ·000222
Observed "	− ·000228
Probable error, . . .	·000080

"Table III. Algebraic Sums of Magnetic Fluctuations (in terms of Horizontal Force) for each Year, from 1841 to 1857, including only those Days of Great Magnetic Disturbance, in which Records were made by the three Instruments."

Theoretical value of M, ...	− ·000287
Observed " "	− ·000257
Probable error, . . .	·000068

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[1] Besides the great disturbing agencies, whose effects may perhaps be determinable by mathematical prediction, every transient local accumulation of heat or cold will exert an influence. Everything that can produce currents or eddies in the atmosphere, may also be presumed to affect the æther, and the inconceivable rapidity of the æthereal motions, as manifested in the velocity of the waves of light and heat, will account for the extreme sensitiveness of the magnetic needle.

Tables V and VI exhibit an approximation to the proportion, C : T : : T : M, but the approximation does not come within the limits of probable error. As no attention is paid in these two Tables to the positive and negative signs, we could not reasonably expect so satisfactory results as in Tables II and III.

"Table VIII. Sums, without regard of sign, of Coefficients of Magnetic Irregularity (in terms of Horizontal Force) for each Year, from 1841 to 1857, including all Days of Record of Great Magnetical Disturbance." The proportion C : T : : T : M, gives for M a

Theoretical value,	·001218
Observed "	·001203
Probable error,	·000066

"Table IX. Sums, without regard of sign, of Coefficients of Magnetic Irregularity (in terms of Horizontal Force) for each Year, from 1841 to 1857, including only those Days of Great Magnetic Disturbance, in which records were made by the three Instruments."

Theoretical value of M,	·001137
Observed " "	·001150
Probable error,	·000081

In addition to these numerical coincidences, the following points in Mr. Airy's paper appear to me to be specially noteworthy.

1. "The Aggregate for the Westerly Force . . . . (taken in comparison with that for the Northerly Force), appears to show that, on the whole, the direction for the Disturbing Force is 10° to the east of south;" p. 628. This indicates a line of mean disturbance about midway between the magnetic meridian (which at London, is about N. 24° W.), and the solar meridian, or midway between the meridians of decussation in the two sets of principal spirals, to which I have referred.

2. "Sometimes two waves in one direction correspond nearly with one in the other direction. . . . . A more frequent relation appears to be, that the evanescence of one wave corresponds with the maximum of the other;" p. 635.

3. "The most striking particulars in the last line (of Tables VIII and IX) are the following:

"First, the almost exact equality of the Mean Coefficients of Irregularity in the three elements. . . . With reference to their physical import, I think it likely that the equality of Coefficients of Irregularity may hereafter prove to be one of the most important of the facts of observation.[1]

"Second, the near agreement in the number of Irregularities for Westerly and for Northerly Force.

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[1] This approach to equality appears to be still more important, in view of the proportionality—C : T : : T : M—P. E. C.

"Third, the near agreement in the number of Irregularities for Nadir Force with half the number of Irregularities for Westerly or for Northerly Force;" pp. 641–2.

4. Tables X and XI (pp. 643–4) show that the disturbances are greatest in the winter months and in the night hours. Table X also appears to indicate minima of fluctuations and inequalities in months when there is the greatest uniformity of temperature, and maxima when the changes of temperature are greatest and most frequent.

5. Tables XI and XII furnish the materials for the following synopsis:

Forces.		Sums of Wave disturbance.		Sums of Irregularities.	Mean Wave disturbances.	Average departure from mean ∓	Mean Irregularity ∓	Frequency of Storms.
		+	−					
Westerly force.	Time of max.,	20 *h.*	10 *h.*	10 *h.*	20 *h.*	13 *h.*	15 *h.*	9 *h.*
	Time of min.,	10 *h.*	21 *h.*	23 *h.*	10 *h.*	2 *h.*	1 *h.*	23 *h.*
	Amt. of max.,	·1170	·2391	·1976	+·00142	·00104	·00162	126
	Amt. of min.,	·0165	·0[illegible]3	·0346	−·00165	·00056	·00074	51
Northerly force.	Time of max.,	5 *h.*	12 *h.*	8 *h.*	5 *h.*	20 *h.*	15 *h.*	8 *h.*
	Time of min.,	22 *h.*	1 *h.*	23 *h.*	22 *h.*	1–2 *h.*	23 *h.*	23–1 *h.*
	Amt. of max.,	·1407	·2917	·1754	+·00038	·00168	·00144	186
	Amt. of min.,	·0038	·0674	·0441	−·00319	·00093	·00077	57
Nadir force.	Time of max.,	7 *h.*	14 *h.*	10 *h.*	0 *h.*	3 *h.*	0 *h.*	10 *h.*
	Time of min.,	22 *h.*	1 *h.*	23 *h.*	17 *h.*	22 *h.*	23 *h.*	1 *h.*
	Amt of max.	·3976	·3128	·1241	+·00570	·00363	·00180	86
	Amt. of min.	·0355	·0306	·0177	−·00380	·00157	·00074	19

"The Soli-tidal character of the principal characteristics of the occasional Magnetic Storms, as to frequency, magnitude, inequalities of wave disturbance, and Irregularities, is seen clearly in this Table." (Table XII) p. 645. There are subordinate maxima and minima, the consideration of which will become interesting, when the laws of the principals have been well ascertained and defined.

6. "In regard to the Wave-disturbance: for Westerly Force, the aggregate is + from 17^h to 6^h, − from 7^h to 16^h; for Northerly Force, the aggregate is + from 3^h to 5^h, − from 6^h to 2^h; and for Nadir Force, the aggregate is + from 23^h to 10^h, —from 11^h to 22^h;" p. 644.

7. Mr. Airy presents some conclusive considerations, "showing that the observed disturbances cannot be produced by the forces of any suddenly created galvanic current or polar magnet," and remarks as follows, respecting his theory: "Its fundamental idea is, that there may be in proximity to the earth something which (to avoid unnecessary words) I shall call a Magnetic Ether; that under circumstances generally, but not always, having reference to the solar hour, and therefore, probably, depending on the sun's radiation or on its suppression, a

current from N.N.W. to S.S.E., approximately, or from S.S.E. to N.N.W. (according to the boreal or austral nature of the ether), is formed in this Ether; that this current is liable to interruptions or perversions of the same kind as those which we are able to observe in currents of air and water; and that their effect is generally similar, producing eddies and whirls, of violence sometimes far exceeding that of the general current from which they are derived;" p. 646.

8. "And in the relation between E. and W. disturbances and vertical disturbances, there is a point which well deserves attention. When a water-funnel passed nearly over the observer, travelling (suppose) in a N. direction, he would first experience a strong current to the E., afterward a strong current to the W. (or *vice versa*), and between these there would be a very strong vertical pressure in one direction, not accompanied by one in the opposite direction; thus he would have half as many vertical as horizontal impulses. This state of things corresponds to the proportion which we have found throughout for the magnetic disturbances, and to the relation found in Article 18. I may also add that the rule at which we have arrived, that the waves of vertical force are few, but that their power, when they do occur, is very great, seems to correspond with what is reported of the whirlwinds of great atmospheric storms; which, violent and even frequent as they may be, occur very rarely at any assigned place;" p. 647.

I add a few considerations from Maj. Gen. Sabine's discussions. (*Phil. Trans.*, cliii, Art. XII.)

9. "The westerly deflections at Kew . . . have a decided double maximum, with an intervening interval of about eight or nine hours.
. . . The conical form and single maximum which characterizes the *easterly* deflections at Kew, belong also to the easterly deflections in all localities in North America, where the laws of the disturbances have been investigated. But . . . at Nertschinsk and Pekin . . . the conical form and single maximum characterize the *westerly* deflections, whilst the easterly have the double maximum. . . . At the two Asiatic stations, the aggregate values of the *westerly* deflections decidedly predominate, whilst in America the *easterly* deflections are no less decidedly predominant; and at Kew, . . . the amount of deflection in the two directions may be said to be balanced;" p. 282.

10. The differences of the weekly from the annual means of declination, indicate "with a very high degree of probability, an *annual variation*, whereby the north end of the magnet points more toward the east when the sun is north, and toward the west when the sun is south, of the equator;" p. 291.

11. The residual errors in the monthly determinations of the Horizontal Force and of the Dip "are thoroughly confirmatory of a semi-annual inequality, having its epochs coincident, or nearly so, with the sun's passage of the equator;" p. 303.

12. There appears to be "an increase of the Dip and of the Total Force, and a deflection of the north end of the Declination magnet toward the West, in both hemispheres, in the months of October to March, as compared with those from April to September. . . . The greater proximity of the earth to the sun in the December compared with the June Solstice most naturally presents itself as a not improbable cause; but we are as yet too little acquainted with the mode of the sun's action on the magnetism of the earth, to enter more deeply into the question at present;" p. 307.

I have neither the leisure nor the ability to undertake an exhaustive analysis of the results thus brought together; but I present them as well worthy of a profound mathematical investigation, as confirmatory in very striking and minute particulars of my mechanical hypotheses, and as furnishing new and strong presumptive evidence of that marvellous simplicity of force to which many independent branches of modern physical research so strongly point. This evidence is strengthened by the existence, as I have shown elsewhere, of the tidal law of sines in the solar-diurnal variation of the magnetic needle, by the magnetic effect of the daily barometric rotation tide, as exhibited in the convergence of lines of equal barometric disturbance toward the hours of high barometer and their divergence from the hours of low barometer, by many points of resemblance between the daily and yearly variation curves, both of temperature and of magnetism, and by certain considerations confirmatory of the reasonable presumption that there may be lunar monthly magnetic tides, somewhat analogous to those which I have pointed out in the barometer. (*Proceedings of the Roy. Soc.*, June 16, and of the *Am. Phil. Soc.*, June 17, 1864.)

Besides the differential or tidal action of the moon, there is a slight tendency to diminish the weight of the air that is nearest the moon, and to increase the weight of that which is most remote. In proportion as this tendency is exerted in conjunction with, or in opposition to, that of the sun, the mean solar-diurnal magnetic currents should be increased or diminished. Slight as the disturbing influence is, and modified as it must be by various causes, both occasional and periodic (*e. g.* the earth's rotation, the cyclical revolution and consequent varying latitude of the moon at the commencement of each new month, the oscillations in the aerial rotation-spheroid produced by lunar attraction, the changes in the average temperature of day and night at different seasons and different years, &c.), it may yet, perhaps, be discernible in comparing the results of a long series of careful and delicate observations. The accompanying tables are deduced from such a comparison of the St. Helena records.

TABLE I.

Solar and Lunar Daily Magnetic Tides, in parts of Force.[4]

Solar and Lunar Hours.	Horizontal force.		Vertical force.		Total force.	
	Solar.	Lunar.	Solar.	Lunar.	Solar.	Lunar.
	·00	·000	·000	·000	·000	·000
0	+1099	+006	−022	−005	+95	+005
1	+0911	−003	+229	+027	+82	−001
2	+0623	−011	+446	+031	+60	−005
3	+0368	−014	+593	+044	+40	−006
4	+0183	−020	+638	+072	+20	−007
5	−0080	−014	+608	+041	+01	−006
6	−0270	−007	+611	+050	−15	+001
7	−0394	−004	+545	+028	−26	+001
8	−0465	000	+300	−012	−36	−002
9	−0511	+022	+219	−011	−41	+018
10	−0530	+032	+074	−017	−45	+025
11	−0522	+031	−011	−037	−45	+022
12	−0481	+019	−100	−003	−48	+016
13	−0449	+017	−165	+005	−41	+015
14	−0405	+013	−224	+019	−38	+014
15	−0376	−009	−289	+048	−36	−001
16	−0352	−011	−345	+051	−35	−002
17	−0329	−008	−398	+029	−34	−003
18	−0298	−009	−465	+013	−32	−006
19	−0154	−001	−513	−011	−20	−005
20	+0130	+003	−582	−053	+03	−005
21	+0470	+006	−491	−050	+34	−002
22	+0803	+013	−427	−054	+63	+004
23	+1019	+009	−214	−067	+85	−001

TABLE II.

Lunar-Monthly Magnetic Tide of Horizontal Force.

Moon's Position. °	Mean Daily Fluctuations of Horizontal Force at St. Helena.[5]				Moon's Position. °	Mean Daily Fluctuations of Horizontal Force at St. Helena			
	1844.	1845.	1846.	1844-6. Average.		1844.	1845.	1846.	1844-6. Average.
0	61·31	53·75	40·88	51·98	180	58·81	53·91	40·86	51·19
15	61·66	53·29	41·24	52·06	195	59·39	53·64	40·27	51·10
30	62·20	52·85	41·78	52·26	210	59·35	53·57	40·70	51·20
45	62·32	53·61	41·81	52·58	225	58·88	52·91	40·74	50·85
60	62·56	52·40	40·66	51·87	240	59·66	52·80	39·79	50·75
75	62·76	52·36	41·34	52·15	255	60·35	52·92	40·70	51·32
90	61·82	52·80	41·54	52·05	270	60·50	53·48	40·65	51·51
105	61·37	53·11	40·42	51·63	285	60·89	53·81	41·10	51·94
120	60·47	53·43	39·97	51·29	300	61·64	52·82	41·41	51·96
135	59·42	53·60	40·46	51·16	315	61·80	53·10	41·34	52·08
150	60·23	53·01	40·14	51·18	330	62·14	53·45	42·39	52·66
165	60·54	53·46	39·72	51·24	345	62·16	52·97	41·85	52·32

[4] The first decimal figures are placed, for convenience, in an upper line.

[5] The value of one scale division is ·00019 of the horizontal force, in 1844 and 1845, and ·00021 in 1846.

Table I is compiled from Maj. Gen. Sabine's Tables 36, 37, 50, 51, 52, 53 (*St. Helena Observations*, ii). It is especially interesting as showing the influence of the opposition of attraction to rotation in producing low solar tides at 10 or 11 P. M., the prompt and direct influence of the sun upon the æthereal currents in the production of a high tide at noon, the double maxima and minima in each of the lunar tides, the additional confirmation of the analogies that I have heretofore pointed out between the spheroids of attraction and rotation, the opposition of the solar and the resemblance of the lunar zenith and nadir effects, and the evidence in the partial "establishment" of the moon's tides that most of her magnetic influence is exerted indirectly on the æther, through the intervention of atmospheric attraction-currents.

Tables II and III were formed by taking the mean of the hourly averages, on the twenty-four days in each lunar month which are most nearly indicated by the angular positions given in the first column. Each of the tabular numbers for 1844 and 1845 represents the average of two hundred and eighty-eight hourly observations; each of the numbers for 1846, the average of two hundred and sixty-four observations, with the few exceptions of holidays and other omitted days, for which the missing numbers were interpolated. Table II indicates a tendency to mean lunar influence between 90° and 105°, and between 270° and 285°, the influence increasing when the moon acts either in conjunction with the sun, or directly upon condensed air and *vice versâ*. It also shows the existence of disturbances, which may be accounted for by some of the causes to which I have already referred. Table III exhibits apparent tendencies to diminution of force near the syzygies, and to increase of force a day or two after the quadratures.

TABLE III.

Lunar-Monthly Magnetic Tide of Vertical Force.

Moon's Position.	Mean Daily Fluctuations of Vertical Force at St. Helena.[*]				Moon's Position.	Mean Daily Fluctuations of Vertical Force at St. Helena.			
°	1844.	1845.	1846.	1844-6 Average.	°	1844.	1845.	1846.	1844-6. Average.
0	48·42	48·51	43·56	46·83	180	47·06	47·77	46·54	47·12
15	48·21	48·55	43·90	46·89	195	47·96	48·02	46·29	47·42
30	47·33	48·53	44·55	46·80	210	48·14	48·26	45·88	47·43
45	47·33	48·25	43·96	46·51	225	47·49	48·26	46·39	47·38
60	47·45	48·47	43·69	46·54	240	48·40	48·60	45·32	47·44
75	47·62	47·88	44·22	46·57	255	48·16	48·52	44·64	47·11
90	47·65	47·43	44·77	46·62	270	48·00	48·08	44·54	46·87
105	47·62	46·92	45·31	46·62	285	47·93	47·70	44·61	46·75
120	47·53	47·42	45·65	46·87	300	48·06	48·26	44·91	47·08
135	47·76	47·40	47·30	47·49	315	48·17	48·56	44·95	47·23
150	47·55	47·50	47·23	47·43	330	48·49	47·91	43·78	46·73
165	47·52	47·70	47·02	47·41	345	48·18	47·44	43·99	46·54

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[*] The value of one scale division varies from ·00051 to ·00091 of the vertical force.

Table IV is a compendium of the tidal differences in the two preceding tables. It shows the effect of temperature in producing maxima and minima when the coolest and warmest portions of the earth are submitted to the direct action of the moon (at or near 240° and 45°), low temperature producing a minimum of horizontal force, with a maximum of vertical force, and *vice versâ*. From the variations of horizontal force $\left(\frac{\Delta X}{X}\right)$ and vertical force $\left(\frac{\Delta Y}{Y}\right)$ given in this table, table V is formed, the mean variations of total force $\left(\frac{\Delta\varphi}{\varphi}\right)$ being obtained by the formula $\frac{\Delta\varphi}{\varphi}=\cos^2\theta\frac{\Delta X}{X}+\sin^2\theta\frac{\Delta Y}{Y}$. I have taken $\theta=-22°$; one scale division of horizontal force $=\cdot000194$; one division of vertical force $=\cdot000792$; which are almost identical with the values employed by Gen. Sabine in the computation of his tables of hourly variation in solar and lunar total force.

TABLE IV.

Lunar-Monthly Magnetic Tide. Differences from Monthly Means.

Moon's Position. °	Horizontal Force.			Vertical Force.			Means.	
	1844.	1845.	1846.	1844.	1845.	1846.	H. F.	V. F.
0	+ ·38	+·54	− ·02	+·59	+ ·51	−1 56	+·30	−·16
15	+ ·73	+·08	+ ·34	+ 38	+ ·55	−1·22	+·38	−·10
30	+1·27	−·36	+ ·83	−·50	+ ·53	− ·57	+·58	−·19
45	+1·39	+·40	+ ·91	−·60	+ ·25	−1·16	+·90	−·48
60	+1·63	−·81	− ·24	−·38	+ ·47	−1·43	+·19	−·45
75	+1·83	−·85	+ ·44	−·21	− ·12	− ·90	+·47	−·42
90	+ ·89	−·41	+ ·64	−·18	− ·57	− ·35	+·37	−·37
105	+ ·44	−·10	− ·48	−·21	−1·08	+ ·19	−·05	−·87
120	− ·46	+·22	− ·93	−·30	− ·58	+ ·53	−·39	−·12
135	−1 51	+·39	− ·44	−·07	− ·60	+2·18	−·52	+·50
150	− ·70	− 20	− ·76	−·28	− ·50	+2·11	−·55	+·44
165	− ·39	+·25	−1·18	−·31	− ·30	+1·90	− 44	+·42
180	−2·12	+·70	− ·04	−·77	− ·23	+1·42	−·49	+·13
195	−1·54	+·43	− ·68	+·18	+ ·02	+1·17	−·58	+·43
210	−1·58	+·36	− ·20	+·31	+ ·26	+ ·76	−·48	+·44
225	−2·05	−·30	− ·16	−·34	+ ·26	+1·27	−·88	+·39
240	−1·27	−·41	−1·11	+·57	+ ·60	+ ·20	−·93	+·45
255	− ·58	−·29	− ·20	+·88	+ ·52	− ·48	−·36	+·12
270	− ·43	+·22	− ·25	+·17	+ ·08	− ·58	−·17	−·12
285	− ·04	+·60	+ ·20	+·10	− ·30	− ·51	+·26	−·24
300	+ ·71	−·39	+ ·51	+·23	+ ·26	− ·21	+·28	+·09
315	+ ·87	−·11	+ ·44	+·84	+ ·56	− ·17	+·40	+·24
330	+1·21	+·24	+1·49	+·66	− ·09	−1·34	+·98	−·26
345	+1·23	−·24	+ ·95	+·35	− ·56	−1·13	+·64	−·45

In a similar manner I have computed Table VI, showing the average hourly variations, both in solar and lunar total force, in each of the three years which have furnished the data for most of my deductions. The first decimal figures are placed in an upper line, as in Table I. Perhaps the principal utility of this table may be found in some future extension of these investigations, but even now it is interesting, inasmuch as it exhibits the probable influence of periodic causes in shifting the hours of the daily maxima and minima, and as it lends added weight to the preceding tables, by showing that the monthly tide is more regular than the daily tide.

TABLE V.

Lunar-Monthly Magnetic Tide of Total Force.

Moon's Position. °	Mean Daily Fluctuations at St. Helena, in parts of Total Force.			
	1844.	1845.	1846.	1844-46. Average.
0	+·00013	+·00015	−·00018	+·00003
15	+·00017	+·00007	−·00008	+·00005
30	+·00016	·00000	+·00007	+·00008
45	+·00018	+·00009	+·00002	+·00010
60	+·00028	−·00003	−·00020	−·00002
75	+·00028	−·00015	−·00003	+·00003
90	+·00013	−·00013	+·00007	+·00002
105	+·00005	−·00014	−·00006	−·00005
120	−·00011	−·00003	−·00010	−·00008
135	−·00026	·00000	+·00017	−·00003
150	−·00015	−·00009	+·00011	−·00004
165	−·00010	+·00001	+·00001	−·00003
180	−·00044	+·00009	+·00015	−·00007
195	−·00024	+·00007	+ 00003	−·00005
210	−·00023	+·00009	+·00005	−·00003
225	−·00038	−·00002	+·00012	−·00009
240	− 00015	·00000	−·00016	−·00010
255	−·00006	+·00001	−·00009	−·00005
270	−·00005	+·00005	−·00011	−·00004
285	·00000	+·00007	− 00002	+·00002
300	+·00014	−·00004	+·00006	+·00006
315	+·00018	+·00004	+·00005	+·00009
330	+·00027	+·00003	+·00010	+·00013
345	+·00024	−·00010	+·00003	+·00006

It seems not improbable that the mutual planetary perturbations which are sufficiently powerful to affect their orbital revolution, may also exert an appreciable influence on their æthereal spheroids, and that numerous cyclical magnetic variations may be thus produced. The disturbance of Jupiter is by far more important than that of any other planet, its mean attractive energy being nearly a third proportional to those of the sun and moon.[7] The annual fluctuations are very great, the intensity being about $\frac{1}{114}$ when Jupiter is nearest the earth, and less than half as great, or only about $\frac{1}{347}$, when most remote. The combined operation of the tropical revolutions of Jupiter, the moon's apsides, and the moon's nodes, should produce a series of disturbances corresponding very nearly in duration with General

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[7] If we take as our unit the moon's attraction for the earth $\frac{M}{D^2}$, the sun's will be about 177, and Jupiter's $\frac{1}{174}$.

Sabine's magnetic "decennial period," and Schwabe's period of solar spots.

The law of varying attraction suggests a plausible explanation for the approximate mean proportionality of the barometric to the tidal and magnetic variations. For the ratios of attraction of any planet when in solar conjunction, at quadrature, and in opposition, vary as $(n + 1)^2$, n^2, and $(n - 1)^2$, respectively, the attraction at the mean distance being nearly a mean proportional between the maximum and minimum attractions. The barometrical fluctuations are occasioned by variations in the gravitation of the air toward the earth's center,—the tidal motions, by the influence of distant heavenly bodies,—and the magnetic, according to my hypothesis, by the oscillations of the air and æther in their efforts to restore the unsettled equilibrium. The three disturbances, therefore, must evidently have nearly the same mutual relations as if they were produced by three forces, one centripetal, and the other two centrifugal, the two latter being nearly equal in amount but diametrically opposed in direction. This leads us at once, theoretically, to the general formula with which we started empirically,

$$A : B :: B : M,$$

and strengthens the conviction that there are none of the phe-

TABLE VI.

Solar and Lunar-Daily Tides of Total Force.

Solar and Lunar Hours.	1844.		1845.		1846.	
	Solar.	Lunar.	Solar.	Lunar.	Solar.	Lunar.
	·000	·000	·000	·000	·00	·000
0	+83	+009	+88	-009	+066	+004
1	+88	+011	+97	-014	+104	+015
2	+67	-005	+85	-016	+097	-004
3	+49	-004	+63	-008	+076	+018
4	+28	-024	+41	-012	+054	+017
5	+15	-032	+20	-001	+031	+024
6	+02	-027	+02	+002	+008	+028
7	-11	-016	-11	+010	-014	+022
8	-22	-006	-24	+003	-026	+022
9	-31	+001	-35	+034	-036	+022
10	-35	+008	-39	+041	-048	+024
11	-37	+029	-46	+031	-047	+021
12	-37	+017	-48	+021	-053	+008
13	-38	+021	-43	+012	-048	+011
14	-39	+045	-44	+012	-040	-001
15	-32	+021	-42	+006	-038	-029
16	-30	+001	-39	+006	-048	-033
17	-29	-004	-39	+006	-041	-027
18	-22	-012	-37	-008	-042	-020
19	-24	-006	-36	-016	-041	-019
20	-16	-014	-24	-008	-027	-011
21	+07	+017	+03	-006	-008	-021
22	+38	+022	+32	-002	+029	-008
23	+64	+016	+63	-008	+063	-010

nomena of terrestrial magnetism which cannot be explained, either by the instantaneously received and instantaneously transmitted impressions which are made directly upon the æther by attraction, heat, or rotation,—by the more sluggish oscillations of the air, which originate from the same sources,—or by the combination of the two.

Every particle is exposed to the influence of these several impressions, the tidal waves of the solid earth having a range, according to Prof. Thomson's calculations (*Phil. Trans.*, cliii, 574), at least two-fifths as great as if the globe were entirely fluid. There is, therefore, good reason to hope, that by the application of mechanical laws to the several phases of the æthereal undulations which produce the phenomena of light, heat, electricity, polarity, aggregation, and diffusion, we may obtain a clearer understanding, not only of all the meteorological changes, but also of seismic tremors, crystallization,[*] stratification, chemical action, and general morphology.

If density is a functional product of superficial magnetism and central attraction, the resemblance of the formula $B = \sqrt{AM}$, to the expression of Mr. Graham's experimental law of molecular diffusion, $\text{Time} = F\sqrt{\text{Density}}$ (*Proc. Roy. Soc.*, No. 56, p. 616–7), and to the general theoretical formula of which Mr. Graham's is a corollary,

$$\text{Velocity} = F\sqrt{\frac{\text{Elasticity}}{\text{Density}}}$$

may be something more than accidental.

If we assume the atmospheric density as our unit, $D' = 1$, and represent the aerial and æthereal elasticities by E', E'', respectively, the proportion

$$\tfrac{1}{5} : 192{,}000 :: \sqrt{\frac{E'}{D'}} : \sqrt{\frac{E''}{D''}}$$

gives an approximate value for the density of the kinetic æther, $D'' = {\cdot}00000000000108\,\frac{E'}{E''}$. The magnetic and barometric fluctuations may perhaps furnish the necessary data for determining the unknown ratio $\frac{E'}{E''}$.

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[*] The phosphorescence that is often observed during the process of crystallization, and the auroral displays in frosty air, are perhaps owing to analogous vibrations. Every chemical, as well as every physical, action produces a ponderable disturbance of equilibrium, which must give rise to æthereal oscillations, to which, in their simplest form, we give the name of electric, magnetic, or galvanic currents.

ART. XVI.—*On the construction of the Spectroscope;* by LEWIS M. RUTHERFURD.

I KNOW of no good substitute for bisulphid of carbon as the dispersive agent in the spectroscope. Flint glass, besides being expensive, when in large masses and of good quality, possesses but half the dispersive power, and the specimens of the denser glass which I have seen tarnish so rapidly, and have so high an index of refraction as to be practically useless. Having devoted much time to the construction and management of bisulphid of carbon prisms, it is quite possible that the results of my experience may be useful to those who may wish to fit up a spectroscope with such prisms, and perhaps I shall best attain the object by describing my own instrument.

The two principal telescopes are provided with objectives of 1·6 inches aperture and 19 inches focal length. The slit or collecting telescope has but one motion about a vertical axis at the side of the platform and just in front of the objective, enabling it to command all parts of the platform. The observing telescope has two motions, one about the central axis of the instrument, and the other about a second vertical axis, which by means of a slide, capable of being clamped, can be placed under the last surface of any prism on the platform; thus commanding by one motion the whole spectrum.

Before the slit is a prism for the comparison of different spectra, and the observing telescope is provided with eye-pieces of various powers. The first circuit consists of six prisms which are of brass faced with plates of glass, cemented with glue and molasses. These are each of about the angle of 60° and present an aperture of 2·9×1·8 inches. The faces to receive the glass are carefully ground to a flat surface and the glass quite thick and free from veins has been selected with reference to the flatness and parallelism of the sides.

Since however it is scarcely possible to find glass with parallel surfaces, care has been taken so to place the glass that the inclination of its faces is perpendicular to the axis of the prism. After grinding the prisms, the bases were so adjusted by filing that the refracting surfaces are rigidly perpendicular to the plane of the platform. This once done removes all necessity for foot screws, which complicate the prisms and add to the expense. The surface to be glazed was washed with an alkaline solution to remove all grease, and then in dilute nitric acid, finally washed in pure water and allowed to dry spontaneously. After being warmed, the prisms were so placed that the surface to be glazed was uppermost and in a horizontal position: the glass, having been cleaned, after the manner of a plate for photographic purposes, also warmed and both surfaces to be in contact dusted with a fine camel's hair brush, was placed in position upon the prism, a hot and fluid mixture of glue and molasses was then applied with a fine brush around the edges of the glass, whereupon a uniform and very thin film of the cement was introduced between the glass and the prism by capilarity. The prism was left untouched, until the cement had hardened so as to admit of its being removed and placed glass downward, for at least a day, when the next surface was treated in like manner. After the expiration of another day, I generally put on another coat of cement, much thicker in consistency than the first. In five days more the prism was ready to be filled through an orifice in the top, to which is fitted a ground stopper which is rendered perfectly tight by a little molasses. It may be thought that my description is needlessly particular, but I have mentioned nothing which experience has not shewn to be necessary to the permanence or performance of the prism.

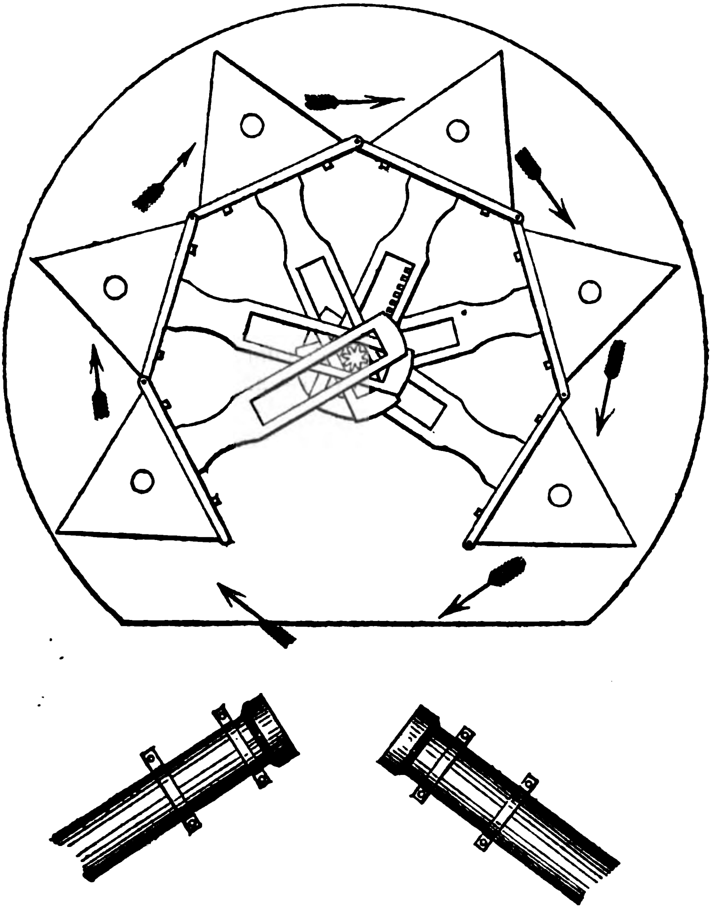
I soon discovered that, after I had made a good prism, its performance would be uncertain, and I finally traced the difficulty to a want of equal density in the bisulphid of carbon, and this peculiarity I have observed not only in the fluid of commerce, but quite as much so in that specially redistilled for the purpose. The fact of this unevenness of density is found in two ways. If a good prism which, with a high power, refuses to define the soda line, (a more stringent test than solar lines,) is violently shaken and then placed in position, it will for a few minutes define beautifully, but gradually settle into its former condition. By covering the aperture of the prism, except a small portion of the upper part, and bisecting the soda line with a spider's web in the eye-piece, all parts of the instrument being clamped, then covering all but the lower portion of the prism, it will be found that the soda line has been carried to a notable extent toward the violet end of the spectrum.

This want of homogeneity in the bisulphid of carbon is entirely different from the disturbance of density by thermal variations. It is a permanent feature of some specimens of the fluid, and is most observable when the prism has been longest at rest in equable temperature. I have one such prism, filled nearly two years since, which defines beautifully for a short time after being well shaken, but soon returns to a poor condition. The difference between the indices of refraction of the upper and lower strata is quite a measurable quantity. My mode of overcoming this obstacle is to filter several pound bottles of the bisulphid of carbon into a long glass jar, having a faucet at the bottom and a ground stopper at the top. After remaining undisturbed two days, the liquid arranges itself according to its density, and I fill the prisms from the faucet, being careful not to shake the jar.

Careful and repeated measures give for the index of refraction of the soda line with the prism first filled from the bottom, 1·62376, and with the ninth prism, filled with the fluid near the upper portion of the jar, 1·62137.

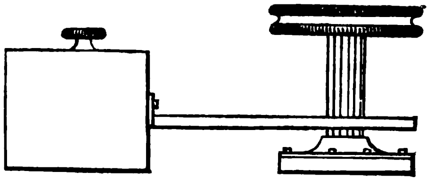
In order to obtain fine definition, it is necessary that the prisms should be placed at the angle of least deviation for the ray under observation. To make the adjustment with several prisms, or to change it when made, is so laborious and troublesome a task as almost to amount to a prohibition of the use of a powerful battery for practical and extended investigations. To remedy this evil I have devised and executed a mode by which I effect the adjustment of all the prisms by one motion of a milled head. An inspection of the accompanying Fig. 1, which represents the

1.



system of prisms without the button, as seen from above, will shew the manner in which this adjustment is accomplished. Upon the glass plate which forms the platform of the instrument, and in the center of the system, is cemented a brass plate, in a cavity of which revolves without shake a pinion provided at the top with a milled head, as seen in figure 2. The prisms are all hinged together at the corners: and from the back of each projects at right angles a brass bar provided with a slot, which, embracing the revolving standard accurately, retains each prism in such a position that, whether nearer or more distant from the center, its back is always perpendicular to the radius connecting it with the center of the standard. The slot of the third prism is provided with teeth which gear into the pinion, so that by turning the milled head this prism is forced to approach or depart from the center: but, from the construction, this cannot take place without imparting a similar motion to each of the other prisms, and thus, at will, their backs are made tangents to a larger or smaller circle, which is the adjustment sought.

2.



This mechanism is capable of adjusting six, or any smaller number, of equi-angled prisms. The outer spiral, when more than six are used, must be adjusted by hand.

New York, Dec. 10, 1864.

Art. XVII.—*Contributions from the Sheffield Laboratory of Yale College.* No. VIII.—*On crystallized Diopside as a furnace product;* by George J. Brush.

Some months since, Mr. Joseph C. Kent, Director of the Cooper Iron Works at Philipsburg, New Jersey, sent to me an exceedingly beautiful crystallized furnace product, which he had observed in one of his furnaces. It consisted of a group of colorless to grayish-white prismatic crystals, associated with a grayish vitreous mass of the same substance, intermingled with fragments of anthracite and traces of graphite and metallic iron. Many of the isolated crystals were transparent and colorless, while others were grayish white and transculent; some of the larger ones were over half an inch in length by one sixteenth of an inch in diameter. Mr. John M. Blake has determined them to have the form of rhombic prisms with brilliant reflecting surfaces. He obtained for the acute angle, in four measurements, with the reflecting goniometer, 86° 50′, 86° 52′, 87°, and 87° 12′. The prism was truncated at each of its obtuse angles by a plane of nearly the same width as the planes of the rhombic prism. The terminal planes were observed on only a few of the smaller crystals, and as they were microscopic were not measured. There is a tendency to cleave apparently in the direction of the rhombic prism, but no reflected image could be obtained from the more or less conchoidal surfaces thus produced.

The hardness of the crystals was about 5·5, and the specific gravity was found to be 3·16 (determined on 138 milligrams of substance). Lustre, vitreous and brilliant. Before the blowpipe in the forceps the substance fuses easily with intumescence to a colorless glass, giving at the same time an intense soda flame.

It is partially attacked by chlorhydric acid, emitting the odor of sulphuretted hydrogen; and entirely decomposed by fusion with carbonate of soda. Analyses made by Mr. Peter Collier, assistant in this Laboratory, gave the following results:

	I.	II.	III.	Mean.	Oxygen.	
Silica,	49·95	49·86		49·91	26·61	28·95
Alumina,	5·03	5·00		5·01	2·34	
Lime, - -	23·71	23·55		23·63	6·75	14·57
Magnesia, -	17·25	17·42		17·33	6·93	
Ferrous oxyd, -	0·39	0·41		0·40	·09	
Potash, -	....	....	1·42	1·42	·24	
Soda, - -	....	....	2·16	2·16	·56	
Calcium, -	0·30	0·33		0·31		
Sulphur, -	0·24	0.26		0·25		
Manganous oxyd,	tr.	tr.		tr.		
				100·42		

Analysis I and II were made by fusion with carbonate of soda, with addition of nitre to oxydize the sulphur to sulphuric acid. The alkalies (III) were determined by Smith's method. The small amount of sulphur was calculated as sulphid of calcium. The oxygen ratio of the mean shows the relation of the protoxyd bases to that of the silica and alumina, considering the latter to replace silica, to be 14·57 to 28·95 or 1 : 2, giving the formula $\dot{R}^3(\bar{A}l, \bar{S}i)^2$. This is the composition of aluminous diopside, and corresponds very nearly with that described by Hunt from Bathurst, in Canada.[1] The crystalline form, as determined by the observations and measurements of Mr. Blake, shows further its relation to pyroxene, and, taken with the chemical composition, leaves no doubt as to the identity of the crystals with the diopside variety of that mineral. Diopside has been previously observed as a furnace product by v. Kobell,[2] and Hausmann,[3] from iron furnaces at Jenbach, near Schwatz, in the Tyrol, and at Gammelbo in Sweden.

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[1] Rept. of Geol. Canada, 1863, p. 467. [2] Münchener gelehrte Anzeigen, xix, 97

[3] Liebig & Kopp, Jahresbericht. 1851, 767.

In regard to the occurrence and formation of this interesting product, Mr. Kent has kindly communicated to me the following facts: "I had one of our large furnaces (21 feet diameter and 55 feet high) in blast for over three years. At the end of the third year the fire brick under the flues commenced burning away, but, unwilling to put the furnace out when it was working well below, I kept on for some weeks, and then took off the blast without 'blowing out,'—as by 'blowing out' the top of the furnace would have been damaged. I then drew the stock out from below, and when the greater part of it had been withdrawn, threw water in to cool the interior and quench the fire. This, of course, left a solid mass in the hearth and exactly in the centre of the furnace, just above the tuyeres, say three and a half feet from the bottom, I found a mass of these crystals clustered within a space of about four cubic feet. I did not find them in any other part of the furnace, and had never before seen or found anything like them."

New Haven, Dec. 9, 1864.

ART. XVIII.—*Introduction to the Mathematical Principles of the Nebular Theory, or Planetology*; by GUSTAVUS HINRICHS, Professor of Physics and Chemistry, Iowa State University.

(Continued from p. 58.)

§ 8. *The condition of the primitive Nebula.*

Whatever may be the distribution of matter in the nebula, and however the particles may move, the beautiful theorem—the magna charta of the nebular theory—obtains, i. e. *the nebula possesses the invariable plane of maximum areas*, or if ω represents the angular velocity, r the projection on the invariable plane of the radius vector drawn out from the center of gravity, m the mass, and A the projection of the area swept over by r in the unit of time, we have, C being a constant,

$$\Sigma m \mathrm{A} = \mathrm{C}\,; \qquad . \quad . \quad . \quad . \quad . \qquad (5)$$

or, as the *centrifugal force* γ of the particle m with respect to the principal axis is

$$\gamma = \omega^2 r \quad . \quad . \quad . \quad . \quad . \quad . \qquad (6)$$

while, at least for a very small unit of time,

$$\mathrm{A} = \tfrac{1}{2}\omega r^2, \quad . \quad . \quad . \quad . \quad . \qquad (7)$$

we have also

$$\Sigma\, m\, r^{\frac{3}{2}} \gamma^{\frac{1}{2}} = 2\mathrm{C}. \quad . \quad . \quad . \quad . \qquad (8)$$

On account of the resistance of the ether, C will not be quite constant, but decrease in time; still it is apparent that, as a first approximation, we may neglect this resistance by considering C strictly constant.

Now, by the mutual attraction of the particles, the nebula is continually becoming more dense, or r is continually decreasing; hence, by (8), *the centrifugal force of any particle in the nebula is continually increasing.*

But the force of gravity at the surface is likewise constantly increasing; for we may without materially erring conceive the mass below the particle to remain constant, but then *gravity* is inversely as the square of the radius, or *rapidly increasing with the progress of condensation.*

But *these two forces determine the figure of the Nebula.* However irregular the figure may be at first, we see that the moulding forces, by constantly increasing, will at length shape the nebula accordingly. From Plateau's Experiments (see above, § 6, result 1, 2) we know this shape to be a flat ellipsoid. Laplace[1] has demonstrated that but *one single oblate ellipsoid of revolution* will be produced by these forces, *i. e.*

$$z^2 + m(x^2 + y^2) = a_1^2, \quad . \quad . \quad . \quad (9)$$

the plane x, y, coinciding with the *invariable plane*, being the equator, z the *axis of rotation* $= 2a_1$ and

$$m = \frac{1}{\sqrt{1+\lambda^2}}, \qquad \lambda = tg\,\theta, \qquad \sin\theta = e, \quad . \quad (10)$$

e being the *eccentricity* of the meridian; hence the equatorial semi-axis

$$a = a_1\sqrt{1+\lambda^2}. \quad . \quad . \quad . \quad (11)$$

Assuming, for a moment, the nebula to be homogeneous, we can determine the eccentricity by the *density* δ, and the *angular velocity* ω (i. e. by (6) proportional to the square root of the centrifugal force g at a unit of distance). Laplace found, if the mass of the whole nebula be M, and its moment of inertia E, that

$$\mathrm{E} = \tfrac{4}{15}\pi\delta a_1^5(1+\lambda^2)\sqrt{g} = \frac{\sqrt{g}}{5}a^2\mathrm{M}; \quad . \quad . \quad (12)$$

$$\mathrm{M} = \tfrac{4}{3}\pi\delta a_1^3(1+\lambda^2) = \tfrac{4}{3}\pi\delta\frac{a^3}{\sqrt{1+\lambda^2}}; \quad . \quad . \quad (13)$$

$$q' = \frac{25\,\mathrm{E}^2}{\mathrm{M}^{\frac{10}{3}}}\left(\tfrac{4}{3}\pi\delta\right)^{\frac{1}{3}}; \quad . \quad . \quad . \quad . \quad . \quad (14)$$

$$\mathrm{arc}\,(tg = \lambda) = \frac{9\lambda + 2q'\lambda^3(1+\lambda^2)^{-\frac{2}{3}}}{9 + 3\lambda^2}; \quad . \quad . \quad (15)$$

which last equation he shows to have *but one* single positive real root, so that λ or e has but one value, if g, the centrifugal force, and δ, the density, are given.

But the density is certainly *not* constant throughout the whole nebula; but as this nebula is a gaseous body, δ will be determined by γ and gravity and, just as in the case of our earth, become uniform in the successive homothetic ellipsoidal shells included between any two successive surfaces. Hence we may consider δ as a function of the equatorial axis of such surface, and, as the density is increasing toward the center, we may, instead of the general law

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[1] Mécanique Céleste, Liv. iii, chap. III, § 21.

$$\delta = f(a), \quad . \quad . \quad . \quad . \qquad (16)$$

take the law assumed by Laplace for the interior of our earth,

$$\delta = \Delta - c\,a. \quad . \quad . \quad . \qquad (17)$$

Plana has shown[1] that this law is the most probable in the case of the earth. Prof. Forchhammer of Copenhagen has lately shown[2] how this law accounts for one of the principal circumstances relating to the succession of geological strata.

It must finally be borne in mind that the nebula may have the various elements at the same place, because the laws of diffusion of gases will apply to the gaseous nebula. Thus far the chemical analyses of meteorites and the spectral analysis of the sun, moon and planets have corroborated this conclusion; still we must not hastily conclude that *some* differences may not obtain, as the diffusion certainly is limited by the sinking of the denser particles. In a nebula from which a whole *cluster* of solar systems has been formed, we may therefore expect to find considerably different elements. We thus decline the imputation of Rutherfurd that homogeneity of original diffuse matter "is almost a logical necessity of the nebular hypothesis," and cannot see any real objection to this hypothesis, if, as he says, "we have now the strongest evidence that they (the stars) also differ in constituent materials" (this Journal, 1863, vol. xxxv, p. 77).

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[1] Note sur la densité moyenne de l'écorce superficielle de la terre. *Astronomische Nachrichten*, 1851, vol. xxv, No. 828. For the density at the center of the earth, he finds 16·3010.

[2] Inledning til en Række af Forelæsninger Stoffernes Kredslöb i Naturen (on the circulation of the elements in nature.) *Nordisk Universitets-Tidskrift*, viii, 1 hefte. Copenhagen, 1862. p. 68-81.

As an instance, *Forchhammer* describes the circulation of *Lime:* first the smaller diurnal orbits between plants and animals and the soil; next the greater annual circulation between land (washed by rain) and the sea; and, being here deposited in large strata through the agency of marine animals, these are again, after long geological periods, put into circulation, especially by the inorganic powers of nature, for these again to recommence a new cycle.—Now, where did the lime at first come from? Forchhammer thinks that as granitic rocks (specific gravity 2·7) are less dense than the dark trap-rocks, (on Bornholm, sp. gr. up to 2·93,) they would, during the first period of the igneous earth, float upon the trap; thus the first solid shell would be formed of granitic rocks, i. e. *free from lime*—consequently unfit to support life, not fossiliferous. By the next revolutions this rock was dislocated and broken through by the underlying trap-rocks containing lime and iron as silicates; the atmosphere being so rich in carbonic acid and being dissolved in the then hot waters would decompose these silicates, and thus bring lime into circulation. Organic life can now first commence—and the first fossiliferous rocks appear. This beautiful idea is further substantiated by the fact that volcanos, after a longer period of rest, commence their eruption with emitting trachytic, i. e. granite-like, masses almost free from lime—which are later succeeded by the *heavy* black lava, containing both lime and iron.

The richest deposit of gypsum, the Triassic period, is succeeded by the extraordinary limestone formation of the Jurassic period, thus giving another link in this chain of inductions, for gypsum, being more soluble, will more rapidly circulate, and thus occasion a greater deposit of limestone.

These views of our illustrious teacher show us what patient investigation yet may accomplish; we see the cause for the succession of granite and trap—see why organic life could not commence earlier than it does—see the cause for abundance of limestone during the Jurassic period, etc.—in the simple circumstance that granitic masses, being lighter than trap, were exterior to the latter in the igneous globe.

In regard to the signification of δ we must remark that, in the following, we use the letter δ to represent *the mean density of the nebula* from the centre to the distance r, while in (17) δ indicates the density of the shell at the very distance r. As (17) is only adduced to serve for a comparison, this course is legitimate. But it is easily demonstrated, that, at least for a spherical nebula, this law (17), if true for the individual shell, will also be true for the mean density of all shells inside of it. For, the actual density varying according to (17), the mean density of the interior body from $r=0$ to r is found to be

$$d=\Delta-c'.r, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

where $c'=\frac{3}{4}c$. This law[4] is evidently the same as (17).

§ 9. *Attraction in the Nebula.*

As the nebula now may be considered made up of homothetic oblate ellipsoidal shells, individually of constant density, and as we know (from *Méc. Cél.*, liv. iii, chap. I, § 2,) that such a shell does not exert any attraction on a point within, we find *the attraction at any point in the nebula determined by the attraction of the ellipsoid whose surface passes through that point.*

This force, at the point x, y, z, is given by the following formulæ (from *Méc. Cél.*, liv. iii, ch. I, § 4), independent of the law of the density (17), and merely depending on the proved uniformity of the density in each separate shell.

If we put

$$Q=\frac{3m}{2\lambda^3}\left[\tan^{-1}\lambda-\frac{\lambda}{1+\lambda^2}\right], \quad . \quad . \quad . \quad . \quad . \quad (19)$$

then the components X, Y, Z, of the attraction (positive toward the origin) are

$$X=Q.\frac{x}{a_1^3}; \qquad Y=Q.\frac{y}{a_1^3}; \quad . \quad . \quad . \quad (20)$$

$$Z=\left(Q-\frac{\lambda^3}{1+\lambda^2}\right).\frac{z}{a_1^3}. \quad . \quad . \quad . \quad . \quad . \quad (21)$$

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[4] The "Density" in Trowbridge's article (this Journal, xxxviii, 354, 1864), is different, because referring to the density at different periods of time.

All of these three components act to *condense* the nebula; but X and Y also determine the revolution of the particles, while Z has no such influence, all motions in the direction of the axis of z mutually destroying each other, because x, y is the invariable plane. Composing X and Y we get

$$R = Q.\frac{r}{a_1^3}, \quad . \; . \; . \; . \; . \; . \; . \; . \qquad (22)$$

and *directed toward the axis of rotation;* $r^2 = x^2 + y^2$.

Substituting the first (13) ($m = M$) in (22) we obtain

$$\left.\begin{aligned} R &= \mu.\delta r, \\ \text{where} \qquad \mu &= \frac{2\pi}{\lambda^3}\left[(1+\lambda^2)\operatorname{arc}(tg=\lambda)-\lambda\right]. \end{aligned}\right\} \quad . \qquad (23)$$

As now μ only depends on λ, i. e. on the eccentricity (10) which is constant, the shells being homothetic, we see that μ is at any given moment for all parts of the nebula the same, hence: *the radial force R in the nebula is proportional to the density δ and the distance r from the axis of rotation.*

This simple result is of very great importance, as we shall see in the sequel.

§ 10. *The orbit of the Planets.*

The particles of the nebula had originally motions in all directions; but as we assumed the existence of a momentum of rotation (§ 6), the principle of the invariable plane will keep up this momentum (§ 8), while all motions at variance therewith will in time mutually destroy themselves. Therefore, *all particles describe circles around the axis of rotation.*

Such as the orbit of the single particles that formed a planet will also be the orbit of the latter; hence the eccentricity and inclination of all planetary orbits ought to be zero. This may also be seen from Plateau's Experiment, and agrees well with the smallness of both the eccentricity and the inclination (see §1, 9° and 11°, § 3, 4° and 5°). Still, neither of these two quantities is actually zero. Are, then, these small deviations from this value accounted for by some accessory conditions of the problem?

We think so, for there are *two* modifying circumstances, the *rupture of the ring*—which it is beyond our power as yet to take into consideration—and the perturbating influence of already separated masses. The latter we may estimate. Representing the eccentricity by e, the inclination of the orbit to the ecliptic by i, to the invariable plane by I, we have from observation [*Humboldt's Cosmos*]:*

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* The numbers in the last column of the following table are not quite exact.—Eds. Journ. Sci.

	e.	i.	I.
Mercury,	·2056	7° 0′	5° 19′
Venus,	·0068	3° 23′	1° 42′
Earth,	·0168	0° 0′	1° 41′
Mars,	·0932	1° 51′	10′
Asteroids,[a]	·160	7° 55′	6° 14′
Jupiter,	·0482	1° 19′	13′
Saturn,	·0561	2° 30′	48′
Uranus,	·0466	0° 46′	55′
Neptune,	·0087	1° 47′	6′
Invar. plane,	....	1° 41′	0° 0′

We see how clearly the principal members of the system *move in one plane*, and that this plane is the *invariable plane* of the system; the great planets deviate less than one degree, the principal of the interior planets, Earth and Venus, only 1⅝ degrees—and even the inclination of the smallest planet, Mercury, amounts to but 5½ degrees! So also in relation to the eccentricity, this being less than one-twentieth for the principal bodies.

As to the deviations, we see that Neptune, which if not the most distant planet, certainly is (or *was*) separated from the next by a very large distance, so that if either could not at all, or but slightly, be disturbed, has indeed the smallest inclination (only 6 minutes!) and about the smallest eccentricity (less than one-hundredth!). Jupiter, which, on account of its enormous mass, could not be much disturbed by other bodies, has an inclination of only 13 minutes, while Saturn and Uranus have—corresponding to their smaller mass—about four times as considerable an inclination (48 and 55 minutes). The eccentricities of these three orbits are about equal; perhaps that of Jupiter is near its maximum, or the eccentricity of Saturn and Uranus near their minimum.

The inclination of the Earth and Venus is greater than that of the exterior planets, for the mass of the former is small as compared to that of the latter; but as Venus and the Earth are the great planets among the interior, we see that the inclination and eccentricity of Mercury's orbit are much more considerable than either, and that Mars has less inclination and eccentricity than Mercury. Is it because Jupiter, the only planet that would exert considerable perturbation on its development, was so far distant?

The orbit of the asteroids is explained in § 5, V.[b] We gave publicity to these views in an address delivered before the physical section, at the meeting of the Scandinavian philosophers, July, 1860.

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[a] Mean of *the first* 72 *Asteroids*, elements given in Table of Smithsonian Report, 1861, p. 218–219.

[b] We intended in this place to give a fuller account of our views concerning the development of the asteroids; but learning from a letter of Mr. Trowbridge that the continuation of his article will contain a solution of this problem, I abstain for the present from publishing my details.

The same principles will apply to the satellites; but we have too few data to make a comparison of this principle with observation profitable.

§ 11. *The periodic time of the Planets; Kepler's third law.*

Since every particle in the same shell revolves around the axis under the influence of a force R proportional to the distance r from the axis (§ 9), we know from mechanics that the periodic time T of such a particle is

$$T=\frac{2\pi}{\sqrt{\mu\delta}}, \quad . \; . \; . \; . \; . \; . \; . \qquad (24)$$

where, it will be remembered (23), μ is the same for the whole nebula, and δ constant for the same shell, so that *the time of revolution is the same for all particles of the same homothetic shell, but for the different shells inversely proportional to the square root of the density.* Thus every shell rotates as if it were solid; and if the whole nebula had the same density throughout it would rotate like one solid. But if the density be different in different parts, some shells will rotate faster than others (§ 12).

Eliminating δ by means of (13) we get

$$\left.\begin{aligned} T^2 &= \mu' \cdot \frac{a^3}{M} \\ \mu' &= \frac{8}{3}\,\frac{\lambda^3}{\sqrt{1+\lambda^2}}\,\frac{1}{(1+\lambda^2)\tan^{-1}\lambda-\lambda} \end{aligned}\right\} \quad . \qquad (25)$$

We know that the ellipticity of the nebula is determined by the centrifugal force, and the latter by the state of condensation (§ 8); and even in case an ellipsoid becomes impossible, we can not but conclude that the figure continues to be determined in the same manner. But the condensation continues—the increase of the centrifugal force depending thereon will also continue and produce a series of rings in a certain succession, just as *one* ring was formed in the experiments of Plateau (§ 7). We see now how the continued increase of the condensation occasions a periodical change in the figure of the nebula. Granting the variation of the figure beyond the possible ellipsoid to be determined by the same circumstances as the ellipsoid itself, we may compare the corresponding stages of the nebula by referring to the same ellipticity e or the same μ' in (25); at any rate, we know that this can be done if we only compare the nebulæ, when within the limits of the possible ellipsoid. But then μ' will be the same for all rings, and as the mass of the planets is but very small as compared to that of the sun, M remains *almost* constant. Then (25) becomes

$$\frac{T^2}{a^3}=\text{constant}; \quad . \; . \; . \; . \; . \; . \; . \; . \qquad (26)$$

or *the squares of the times of rotation of the different rings are as the cubes of their radii.*

If we remember that the possible ellipsoids reach to a proportion of 1 to about 3 between polar and equatorial diameters of the nebula, we can be sure that *this* covers the principal part of the metamorphosis; hence, (26) is rigorously proved for the greatest part of the condensation intervening between the formation of two successive rings; the nebula acquires its principal dimensions while changing in accordance with the ellipsoidic figure, and when abandoning this it quickly passes to the form of a slightly oblate spheroid and a ring. The interruption in our strictly mathematical demonstration cannot, therefore, seriously interfere with (26). But then this or *Kepler's third law is a consequence of the nebular hypothesis*, or *the observations embodied in this law sustain equally the nebular hypothesis and gravitation.*

Again, inductively, we may conclude from Kepler's third law that the interruption in our analytical deductions occasioned by our ignorance of the exact mechanical laws of the metamorphosis of the ellipsoid into the *globe ring* (we might in reference to Saturn find the expression *Kronion-form* convenient) is not of serious consequences.

Thus we may at least conclude from the third of Kepler's great laws that the development of the planets was periodical; for, this law being a fact, and (25) being rigorously true, we *must* have

$$\frac{\mu'}{M} = \text{constant}; \quad . \; . \; . \; . \; . \; . \; . \; . \quad (27)$$

but, as remarked before, M remains essentially constant, hence μ or what is the same λ, i. e. the ellipticity ϵ of the nebula corresponding to the different planets, must have been the same at corresponding epochs, just as we assumed above.

But *if* the metamorphosis of the nebula has been periodic, and not simultaneous, we must ascertain *whether the successive intervals of time were equal or not.* We shall find that *they were equal*, just as it would be the most natural or the simplest to assume.

§ 12. *Spiral Nebulæ.*

In the preceding paragraph we considered the density of the nebula sensibly equal throughout, so that the nebula always rotated like a solid, all particles having sensibly the same period of revolution. This might be done, because the dimensions of such nebula—however immense in reality—are not sufficiently great to produce a very large change in δ (17) in the space allotted to each planet.

But there may be bodies of dimensions so vast as to render it utterly impossible to consider the density approximatively uniform throughout the nebulous mass. Then the nebula will *not* rotate like a solid, but the angular velocity ω of any particle will be

$$\omega = \frac{2\pi}{T}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

or, by (24),

$$\omega = \sqrt{\mu\delta}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

As μ (23) is constant for the whole nebula, we see that *the angular velocity is proportional to the square root of the density*, or, according to (17), greatest near the center of the nebula.

If θ be the angle of position of those particles which are originally (i. e. when $t=0$) in one and the same straight line, we have at the time t,

$$\theta = \omega t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

or by (29)

$$\theta^2 = \mu . \delta . t^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

Remembering that the density is a function of the distance (16) and also of the time on account of the progressing condensation, we see that (31) may be written,

$$\theta^2 = \mu t^2 f(a, t) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (32)$$

At any given moment of time (t constant), all the particles that originally were situated in the same straight line given by

$$\theta = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

will now form the curve

$$\theta^2 = \varphi(a),$$

i. e. *a spiral.* This contains the fundamental principles of a mechanical theory of the *spiral nebulæ.*

Substituting Laplace's law of the density (17) in (31) or (32), we obtain as the equation of the spiral

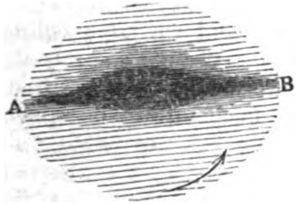
$$a = \frac{\alpha^2 - \theta^2}{C}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (34)$$

wherein $\alpha = \mu . \Delta . t^2$ depends upon the ellipticity (μ), the *density* Δ *at the center*, and the time t, whilst $C = c\mu t^2$ depends upon the same μ and t and the *rate of variation of the density.* We see that these spires are limited, for $0 < \theta < \alpha$; and that the sweep α of the spire increases with the *age* of the nebula, the *density* at its center, and the *ellipticity.*

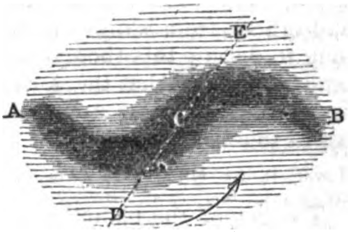
In order that such spiral structure may become apparent in a regular ellipsoidal nebula, the brightness must originally have been different in different meridians, though the density was constant in the same shell, i. e. the same in all meridians. Thus, if the brightness in the nebula was originally greatest in the opposite meridians AC and BC, (Fig. 1.) and it rotates in the direction of the arrow around the axis C, the *spiral nebula,* (fig. 2,) would result. As the age increases, the sweep, or the angle

BCE = α, would increase, whilst A and B remain nearly at the same distance from C: so that an annular nebula with a central core might in time result from a spiral nebula; even several concentric rings might be formed.

1.



2.

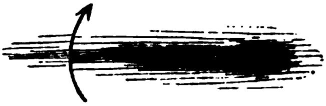


We cannot suppose any nebula to have different brightness in parts of the same density; and neither is it reasonable to assume such vast masses to be already shaped to a regular ellipsoid by the influence of the central forces (see 7).

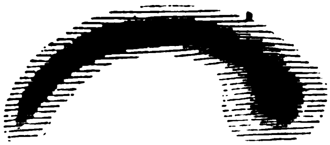
It is much more reasonable to think that the nebulous masses at first were of *any shape*—such as might result from a predominating attraction of those portions where the heaviest elements were formed or collected in greater abundance. Then the formulæ deduced in the preceding paragraphs, though no longer representing the exact conditions of the nebula, still would continue to be approximate; the angular velocity would still be greatest near the central parts, as can also easily be shown directly, by considering the motion of each particle as subject to the attractions of all the others. Then the particles originally in a straight line would still in time form a spiral.

So we see that a nebula originally in the shape of a light rectilinear cloud with a condensation near the middle, like the part AB in fig. 1, would after some time exhibit a spiral like the dark part in fig. 2. The nebulæ, Herschel 1061, and H. 1337, as seen by Lord Rosse,[7] have exactly such a form. If, instead of having the nucleus in the middle, the original nebula had been denser near one extremity, like fig. 3, a simple spire like fig. 4 would

3.



4.



be the resulting spiral nebula, as we see it in H. 327, H. 1946, etc. A nucleus with four branches of different density and magnitude would give a spiral nebula like the beautiful object, Messier 99. If each of the two arms in figure 1 had been subdivided into two branches, H. 2084 would result.

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[7] Prof. G. P. Bond. Director of Harvard College Observatory, kindly sent me copies of a number of Rosse's latest figures of spiral nebulæ—for which important service I here repeat my sincere thanks.

These few remarks must be sufficient at this place. We have already deduced forms as fanciful as H. 1196, H. 131, H. 1744, and others, from simple rectilinear forms, and we hope before long to discuss this theory more at length. We here merely intended to show that the forms revealed to us by the great telescope of Lord Rosse appear to be simple mechanical consequences of the nebular theory, if applied to very large nebulæ.

§ 13. *The Law of the Planetary Distances.*

The law of the planetary distances has not as yet been discovered, though it has been most diligently sought for as the principal element of the "Harmony of the Spheres." The endeavors of Plato were in vain, and Kepler at last ascended to the truth that the present distances are not exactly the original ones. Titius, and after him Bode, came near it [§ 2, (1)]; but the *deviations* from this law remained unaccounted for, thus not giving the conformation most essential to any law.

To find the true law of the planetary distances has been our aim for nearly ten years; we hope the sequel will prove that we at length have found the solution of this problem in the following law:

The mutual distances of the planets correspond to equal intervals of time.

That this is a *fact* we will demonstrate; but *why* these intervals were equal we are not yet able fully to see—still we know that this is the simplest way in which the periodicity in the development of the nebula as found in § 10 can obtain.

Deferring a thorough discussion of the earlier attempts, (some of which are almost contemporaneous with our own solution,) to some future opportunity, we will now give the *inductive reasoning* which leads to our law above stated.

There are a few well known laws in the evolution of the nebula which embody the solution of the problem. We know that the planetary masses are insignificant as compared with the solar mass; hence we see that the orbits of the planets simply mark the equatorial band of the condensing nebula at those definite periods when the radius of the nebula had diminished to the distance of the planet. Thus we see that *the planetary distances must be functions of time.*

Or, if it be more plain, we may say that the original nebula, in contracting, left at certain intervals a few particles behind to mark the limit of the nebula at those instants. But while condensing, the uttermost particle of the nebula describes a spiral curve; and if we can find the relation between the distance *a* of this particle and the time t, we need only to substitute the different intervals corresponding to the formation of the different planets in order to obtain their distances. But as the evolution is regularly periodical (§ 10), it is most probable that these intervals *are equal;* comparison with observation shows this to be the case.

But the motion of such a particle in so rare a nebula is regulated by the attraction of the whole nebula and the resistance of the ether. The first of these forces is inversely proportional to the square of the distance a, since the mass remains sensibly the same, and the particle is considered as on the equatorial *surface* of the nebula. In other words, the force of attraction on the particle is the same as the force of gravitation acting upon a planet. Resistance of the ether will necessarily follow the same law, whether a single particle or a planet be subject to it. But then our analysis[*] of the motion of a planet toward the sun is directly applicable to the motion of the superficial particle in its fall toward the center of the nebula. Formula (10) of that article shows the distance a (radius of the nebula) to be

$$a = A . e^{-2\nu t} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (35)$$

where A is the original distance (or radius of the nebula) and t the time of falling from A to a. Or, if a_t represent the distance of the planet that separated from the principal nebula at a time t *earlier* than the now nearest planet—i. e. the *age* of the planet as counted from Mercury,—the above (35) becomes

$$a_t = \beta . \gamma^t \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (36)$$

where β and γ are constants. But in the analysis leading to (35) the coëfficient ν of resistance

$$\nu = \frac{3}{8} \frac{\delta}{\Delta \varrho} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (37)$$

has been considered constant; here we cannot do so, for though the density δ of the ether and the radius ϱ of the particle may be considered constant, the density Δ of the particle varies very much, about inversely as the cube of the radius of the (homogeneous) nebula. If, therefore, ν' be the value of ν corresponding to the particle at the distance 30·0 of Neptune, ν'' the same at the distance 0·4 of Mercury, we have for a homogeneous nebula

$$\nu' : \nu'' = (\cdot 4)^3 : (30 \cdot 0)^3 = 1 : 422000$$

nearly. If δ increases toward the center this proportion would be diminished; but still we see that ν decreases toward the interior. The formula (36) can therefore only express the principal part of the law; how (36) has to be amended in order to take the variation of ν into account has to be separately investigated. Before we attempt this we will compare (36) with observation.

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[*] On the Density, Rotation and relative Age of the Planets. This Journal, 1864, [2], xxxvii, 36.

Erecting at equal distances (§ 10) ordinates proportional to the actual planetary distances, and interpolating by connecting these points by a curve, we see that this curve has the appearance of a *logarithmic curve* (like those given on the plate appended to our former article, this Journal, 1864, vol. xxxvii); and if drawn with sufficient care, we find that the constancy of the subnormal, characteristic of the logarithmic curve, holds good in the present instance—thus proving that (36) really is applicable to the planetary distances. But it is not the exact law, for the axis in the diagram is evidently too far below the curve, or the distances are too great by a constant α, so that the diagram of the planetary distances will be expressed not by (36) but by

$$a_t = \alpha + \beta . \gamma^t, \quad . \; . \; . \; . \; . \; . \; . \; . \quad (38)$$

and it remains to be seen whether this additional constant α can be accounted for by the variation of ν (37). Before we investigate this, we will see how far (38) represents observation.

We see that it is almost the same as the law of Titius, but while in the latter t *is a mere index*, it is in (38) a variable, the great independent variable of mechanics, *time* or *age!* Besides, (38) deviates from Titius in the case of Mercury. Adapting the constants of Bode to (38), it becomes

$$a_t = 4 + (1 \cdot 5) . 2^t. \quad . \; . \; . \; . \; . \; . \; . \quad (39)$$

Representing by a the actual distance, we have, for comparison with observation,

Planet.	age, t.	Distance calc. a_t.	 obs. a.	Difference.
Mercury, - - - -	0	55	38·7	+ 16·3
Venus, - - - - -	1	70	92·3	− 2·3
Earth, - - - - -	2	100	100·0	0·0
Mars, - - - - -	3	160	152·4	+ 7 6
Asteroids ①–(72), - -	4	280	262·3[8]	+ 17·7
Jupiter, - - - - -	5	520	520·3	− ·3
Saturn, - - - -	6	1000	953·9	+ 46·1
Uranus, - - - - -	7	1960	1918·2	+ 41·8
Neptune, - - - -	8	3880	3003·6	+876·4

We see that the *present distances* a agree with the *original* a_t for the principal planet of both groups, for the Earth and Jupiter. Mars, Saturn, and Uranus are about $\frac{1}{20}$th of their distance too near the sun, having approached the latter so much more on account of their mass being smaller. Mercury and Neptune have even approached still more, the former because of the smallness of its mass, the latter on account of its high age (see this Journal, vol. xxxvii, p. 41). Before the precise influence of resistance was known, these deviations were considered sufficient cause to reject the law of Titius-Bode; but now these very deviations have become essential supports of the truth of that law.

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[8] Calculated from the table in *Smithsonian Report* for 1861, p. 218–219. We found the following interesting fact: mean distance of (1) to (31) = 2·599; of (31) to (56) = 2·679; of (57) to (72) = 2·752, showing that in general *the more distant members of the group of asteroids have been later discovered.*

Another and better test of our law (38), and of the constants of Bode (39), is obtained by directly solving (39) for the age t

$$t=\frac{\log(a_t-40)-\log 15}{\log 2} \quad . \; . \; . \; . \; . \quad (40)$$

and seeing how far t is given by the series 0, 1, 2 ... We thus find

Planet.	Age.	too small.
Mercury,	imag.	
Venus,	1·1066	—·1066
Earth,	2·0000	·0000
Mars,	2·9056	+·094
Asteroids ①–(72),	3·890	+·11
Jupiter,	5·0001	·000
Saturn,	5·9264	+·073
Uranus,	6·9683	+·031
Neptune,	7·6230	+·377

From this table we see that the age of the planets above that of Mercury is as the series of natural numbers, *the deviations not only being but small*, but just such as influence of the mass would make them. This may be easily proved by the formula contained in the article on the age of the planets before referred to.

If the present age of Mercury be m, then the age of the interior planets will be to that of the exterior ones as $m+\frac{6}{4}$ is to $m+\frac{26}{4}$, or as $2m+3$ to $2m+13$. We found this ratio as 1 to 3 (this Journal, vol. xxxvii, p. 43); if true it would follow that $m=1$, or the *total age of any planet would be* $t+1$, *the unit being the age of Mercury.*

After having seen that (38), the modified form of (36), is applicable to the planetary distances, we will demonstrate that this modification is consistent with the signification of t, the time.

If the resistance R be proportional to the velocity v, or

$$\mathrm{R}=\nu v \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (41)$$

we have the tangential force (this Journal, vol. xxxvii, p. 40)

$$\frac{1}{r}\,\frac{d\left(r^2\frac{d\theta}{dt}\right)}{dt}=-\mathrm{R}\cos\eta=-\nu r\frac{d\theta}{dt}, \quad . \; . \quad (42)$$

where r is the radius vector, and θ the anomaly; but Kepler's second law gives

$$r^2\frac{d\theta}{dt}=c, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (43)$$

so that (43) becomes

$$\frac{1}{r}\left[\frac{dc}{dt}+\nu c\right]=o, \quad \ldots\ldots \quad (44)$$

giving *for* ν *constant,*

$$c = C.e^{-\nu t}, \quad \ldots\ldots \quad (45)$$

or, since by Kepler's third law, $c^2 = a\mu$,

$$a = \frac{C^2}{\mu}e^{-2\nu t}, \quad \ldots\ldots \quad (46)$$

all of which formulæ are demonstrated in our article referred to above.

Instead of solving the problem directly, we may indirectly try to find how ν must vary that (36) may become (38), i. e. to add a constant term to (46). In other words, C instead of being constant must be considered a function of t, i. e. (44) must be

$$\frac{1}{r}\left[\frac{de}{dt}+\nu c\right]=\varphi(t), \quad \ldots\ldots \quad (47)$$

so that the resistance now becomes, see (42),

$$R = \nu v - \frac{\varphi(t)}{\cos\eta} = \nu v - \frac{ds}{d\theta}.\frac{\varphi(t)}{r}, \quad \ldots\ldots \quad (48)$$

instead of (41), where $\cos\eta = \frac{rd\theta}{ds}$, and ds is the element of the orbit. The function $\varphi(t)$ can now, by the method of the variation of the arbitrary constants, be so determined that (46) or (36) coincides with (38). Since r is a function of t, we may make

$$f(t) = r\,\varphi(t), \quad \ldots\ldots \quad (49)$$

hence (47) becomes

$$\frac{dc}{dt}+\nu c = f(t). \quad \ldots\ldots \quad (50)$$

Taking the complete differential of (45), i. e. also considering C variable, substituting in (50) and reducing by (45), we obtain for the determination of C,

$$e^{-\nu t}.\frac{dC}{dt} = f(t). \quad \ldots\ldots \quad (51)$$

This gives, by making K an arbitrary constant,

$$C = K + \int e^{\nu t}.f(t).dt, \quad \ldots\ldots \quad (52)$$

which, substituted in (46), gives,

$$a = \frac{e^{-2\nu t}}{\mu}\left[K + \int e^{\nu t}.f(t).dt\right]^2. \quad \ldots \quad (53)$$

This should be identical with (38), i. e. (remembering that t here is counted from the most distant, in (38) from the nearest planet and that γ in (36) is $e^{-2\nu}$ in (35))

$$a = \alpha + \beta.e^{-2\nu t}. \quad \ldots\ldots \quad (54)$$

Equating (53) and (54), and solving for $f(t)$, we find,

$$f(t) = \nu \frac{\alpha\sqrt{\mu}}{\sqrt{\alpha+\beta e^{-2\nu t}}}, \quad . \ . \ . \ . \quad (55)$$

or, by (54),
$$f(t) = \nu\alpha\sqrt{\frac{\mu}{a}}. \quad . \ . \ . \ . \ . \quad (56)$$

But Kepler's third law gives $\mu = a.v^2$ (this Journal, vol. xxxvii, p. 38, note); hence

$$f(t) = \nu\,\alpha\,v, \quad . \ . \ . \ . \ . \ . \ . \quad (57)$$

consequently, by (49), r and a being now the same again,

$$\varphi(t) = \nu\alpha\frac{v}{a}, \quad . \ . \ . \ . \ . \ . \ . \quad (58)$$

or (48), $\cos\eta$ being almost equal to one, the orbit being nearly circular,
$$R = \nu v\left(1 - \frac{v}{a}\right). \quad . \ . \ . \ . \ . \quad (59)$$

Thus we see that (36) becomes (38) if the resistance R, instead of being simply proportional to the velocity (41), is varying according to (59), which may be comprehended in (41) by taking the factor ν to decrease from $\nu\,(a = \infty)$ to $0\,(a = \alpha)$ according to

$$\nu' = \nu\left(1 - \frac{\alpha}{a}\right). \quad . \ . \ . \ . \ . \ . \ . \quad (60)$$

This variation of the coefficient of resistance is conformable to (37), since Δ, according to (16) (then δ), increases as a decreases.

The law
$$a_t = \alpha + \beta.e^{-2\nu t} = \alpha + \beta.\gamma^t$$
is, therefore, but an amplification of
$$a_t = \beta.e^{-2\nu t} = \beta.\gamma^t:$$
in the latter the coefficient of resistance *is constant*, in the former it *varies according to* (60). As now (60) is real, (54) or what is the same (38) is the real law of the planetary distances, t continuing to represent the age, and not, as in Bode's law, a mere index. *And as now finally* (38), *applied to the actual distances, gives values for t that are very nearly as the natural numbers, our law, announced above, holds true, that the planetary distances correspond to equal intervals of time;* or *the consecutive planets were abandoned at equal intervals of time.*

There remain yet two remarkable consequences to be drawn from this exponential law of the planetary distances. If in (38) *it is sufficiently great* (i. e. the corresponding planet far from the center) to make the first term insignificant as compared to the second, we have approximatively

$$a = \beta.\gamma^t,$$
hence
$$a^{t+1} = \beta.\gamma^{t+1},$$

consequently $$\frac{a_{t+1}}{a_t} = \gamma, \quad . \; . \; . \; . \; . \; . \; . \; . \quad (61)$$

or *for the most distant planets their distances approach to a simple geometrical series whose quotient is the base* γ. But this law will again in part be interfered with on account of the action of resistance on the completed system, which, on account of the higher age, has drawn the most distant planets comparatively nearer to the sun than the less distant ones, so as to *diminish* the above quotient γ.

Again, if t is sufficiently small—or the planet sufficiently near the center—the exponential series contained in (38) is highly convergent, so that perhaps the approximation may be sufficient if only the term of the first order is taken, so that (38) becomes, A and B representing constants,

$$a_t = A + B.t, \quad . \; . \; . \; . \; . \; . \; . \; . \quad (62)$$

hence $$a_{t+1} = A + B(t+1),$$

$$a_{t+2} = A + B.(t+2), \text{ etc.}$$

or, $$a_{t+1} - a_t = a_{t+2} - a_{t+1} = B, \quad . \; . \quad (63)$$

i. e. *the innermost planets have a tendency to become equidistant.*

Both of these consequences are very plainly marked in the solar system, especially in the lunar, but also in the planetary orbs. For, as regards (61), we have for the distances of

Saturn	to	Jupiter	as	1·85	to	1.
Uranus	"	Saturn	"	2·01	"	1.
Neptune	"	Uranus	"	1·57	"	1.

For Saturn–Jupiter this proportion is still less than $\gamma = 2$—also because Jupiter both on account of its age and mass has fallen less toward the sun than Saturn; but for Uranus–Saturn the ratio is almost equal to $\gamma \fallingdotseq 2$, while for Neptune–Uranus it is less again, on account of the higher age of the first.

The second circumstance, expressed in (63), seems to be exemplified in the orbits of Mercury, Venus, Earth, the three planets that are nearest to the sun, or for which t is the smallest. Their distances are

	Distance.	Difference.
Mars, - - - - -	152·4	52·4
Earth, - - - -	100·0	26·7
Venus, - - - - -	72·3	33·6
Mercury, - - -	38·7	

We see how Mars, Earth and Venus follow Bode's law exactly, for one-half of 52·4 is 26·2, or very nearly 26·7—but the distance between Venus and Mercury is 33·6 instead of ½ of 26·7 or 13·4. This difference might be considered as a consequence of (63); but we know that it is principally due to the small mass of Mercury.

[To be concluded.]

ART. XIX.—*Periodic action of Water;* by LOUIS NICKERSON.

IN reading, some weeks ago, the article by Prof. Loomis, on the vibrations of water flowing over a dam, I was somewhat surprised at the idea of deriving the peculiar motion from a foreign source, as a column of air; surprised, because, however much the air might effect, by reaction, after the action had commenced, the perturbations of a liquid, in whatever state of motion it may exist, have always been so connected with periodic action as to have given use to the name of its most common attribute "the wave," as the characteristic title of nearly all periodic action. Without an attempt to discuss the question with the distinguished gentleman engaged, I shall endeavor to point out the manner in which the vibrations may be considered simply as the result of a wave peculiarly circumstanced.

I was sitting one day upon the bank of a large river in the West. Before me was a strong ripple, supposed by the people around to have been caused by the lodgment of snags upon the bottom. The sound from it was much louder than the roar of the stream—then in a state of freshet, and itself uproarious. But there was a cadence in it, an easily distinguished division into regular periods, which induced an inclination to pause and watch. My position was just upon the middle of an arc, which a late "caving in" of the bank had indented, each point of the arc running past the average bank of the river toward the center. The one down the river being in the greatest projection gave to the arc the appearance of a crescent, quartering into the bank and with its back down-stream-ward. Now a portion of the main current of the river, striking this lower horn, rounded inward as though to make a whirl within the crescent, but, thrown off, shot over toward the center and up stream. Above, another partial current, cut off from the main stream by a shoal or otherwise, came down along shore, and passing the upper horn bore directly down upon the face of the first eddy. Now they attempt to bear each other back, as though striving for the mastery of the crescent. For an instant there is an equilibrium. Both currents at the place of meeting rise rampantly into waves; both seem to receive reinforcements. They might be supposed to be equally matched, but the upper current receives the most water. For another instant they stand poised and opposed, and then the upper rushes, broken, but conquering, down over the surface of the other. Carrying off, however, not only its superabundance, but dragging along a little more water, so that the lower current quickly regains its ascendency, driving the upper back to be again checked and again overpowered, as before.

Afterward, I watched this place for hours at a time, unfortunately without timing, but yet with so distinct and definite a feeling of the regularity of the periods, that it was easy to estimate in the mind the exact period when, equilibrium having been attained, the lower water would start suddenly back and the accumulated waves from above rush over it, always dragging a sufficient extra quantity of water from the upper current to give for a time the ascendency to the lower. The space thus fought over was three or four feet.

But mill-dams are certainly not in just this situation. The permanent weir opposes no such active resistance as the elastic and moving weir just described. Its characteristic is passive resistance. Must we therefore look to it for continuous action? I take it to be thus:

1st. That a certain quantity of water arrives at the pool, and is all passed over the weir in the end, but in periods.

2d. That the quantity passed between these points, is, at the lesser points of velocity, of greater transverse section. This is obvious.

3d. That the decrement of velocity, and corresponding increment of section, is greater at a point nearly under the point of greatest depression of the curve of amplitude.

4th. That outside of this stream the pool is made up of water in a state of slow motion, at rest, or even in some cases of reaction, whirling or flowing backward.

Now we may examine the curve of hydraulic amplitude and its changes. For this we take the formulæ of permanent motion; which, though not exact, is sufficiently characteristic, as derived from Weisbach,

$$a - a'_1 = \left(\frac{\sin\alpha - z\,\frac{P}{ab}\cdot\frac{v^2}{2g}}{1 - \frac{2}{a}\cdot\frac{v^2}{2g},} \right) l,$$

when

$\sin\alpha$ = slope of original stream
P = whetted perimeter,
al = transverse section,
v = velocity,

l = distance between a and a_1.
a = depth of dam or known point.
a_1 = required depth.
z = coefficient of resistance,
g = 32·2, or a gravitation.

The form of this curve is represented in the works of almost all hydraulic authors, and its equation shows it to be asymptotic to the original surface. It is easily seen above that when $\sin\alpha$ of the original surface becomes equal to $z\frac{P}{ab}\cdot\frac{v^2}{2g}$ and therefore equal to $\sin\alpha'$ of a transverse section of the pool; $a - a_1 = 0$, or the pool is simply a continuation of the stream, and if $\frac{v^2}{2g}$ becomes equal to $\frac{a}{2}$, or the height due to velocity becomes equal to one-half the depth, both of the original stream, then $a-a_1=\infty$, a case which we shall examine more hereafter. We know that when the pool is first filled and the water is just on the point of flowing over the dam, the surface is horizontal, and we call it the hydrostatic amplitude. It is moreover true, that when the flowage commences and the hydraulic amplitude obtains, there is a stream of water passing through the pool, various in its velocities, and with a sheath of water, differently circumstanced, around it, which it in some way affects. We have also the admitted law, (Weisbach, vol. 1, art. 307; D'Aubinson, art. 54), that when any stream of liquid is in motion in any direction, its pressure in all other directions is equal to its hydrostatic pressure, less the pressure in the direction of its motion, and generally, that when a liquid in motion is made to pass through a liquid of less velocity, a part of the latter is dragged along by its greater lateral pressure, and passes off with the stream. In 1797 the engineer Venturi applied this principle successfully to the drainage of public lands. (Ewbank's Hydraulics, p. 478.)

In a pool fed at one point and yielding up the water at another, such as a mill-pond, this state of affairs practically obtains: that there is first a stream of water running some distance into the remou, and another passing out; in long dams only near the ends, perhaps, but in those of ordinary size throughout the whole length. This stream comes in contact with and passes through walls of partly quiescent water, not only that of the dead angles, but also of superimposition. For, says the engineer D'Aubinson, "moreover, the water of flowage seem only to be superimposed above the current, and not to participate wholly in its motion. The engineers who took the levels upon the Weser have observed, at a distance of 3884 feet from the dam, that the velocity of the surface was nearly insensible, whilst that at the bottom was quite strong."

We have now the fact that a stream is running through water much more nearly at rest, and that owing to the difference of pressure, some of the slower water must be dragged along in the course of the faster, in quantity and force varying as the difference of lateral pressure. We must remember that as in the end the weir can only pass over the same amount of water as it has received from the upper end, i. e., the water of the current, there must, then, be a periodic lull until the deficiency caused by this dragging action has been replaced.

It might readily appear that, as the velocity may become less from the interior of the stream to the outside, this might occur and yet a continuous action be kept up. But a little thought will show that as normally the stream passes out exactly the quantity received through its own continuation outside the pool, and as the water which I have shown to be dragged out is an extra quantity, there must be a pause after its exit until the surface, which has been lowered a very small increment by its departure, attains its own proper regimen.

You will notice that this reasoning requires that there should be a rise and fall of the surface of the curve of amplitude. I have seen no such rise myself, nor been able to obtain a confirmation of its occurrence from others. But these causes of its existence appear to me too plain to be disputed. It must by necessity be extremely small,—too small perhaps for observation. For the formula $h = \frac{v^2}{2g}$ when applied to a 4 foot velocity gives us $h = 0{\cdot}25$, and a velocity of 4·3 yields for h only $h = 0{\cdot}263$. By which we see that a rise which would only add a quantity $= 0{\cdot}013$ to the depth would increase the velocity full $\frac{3}{40}$. Then for the ordinate of the parabola of theoretic fall, we should have for the *vis viva* of the horizontal component a quantity varying as the square of velocity, or, for velocities varying from 4 ft. to 4·3 ft., a mechanical effect varying as 16 to 18·5 against the resistance of the atmosphere. That the atmosphere both outside of the falling water and the column inclosed between it and the dam, would assist by its elastic reaction in the vibrated distance, there can be no manner of doubt. But a cursory thought strikes me as I write, that were the action of the air truly isochronous with these vibrations, then the vibrations would have a constant tendency to increase. Partly it assists, partly deadens.

Again let us recur to our formula. Although made by Weisbach only to measure the curve of amplitude, and for the ordinary case of remou, it still contains the very elements which we need, and its changes may at least mark corresponding changes in the law which we discuss. In

$$a_1 - a_0 = \left(\frac{\sin\alpha - z\frac{P'}{ab}\cdot\frac{v^2}{2g}}{1 - \frac{2}{a}\cdot\frac{v^2}{2g}} \right) l,$$

we see that $\sin\alpha$ is the measure of the slope of the original current, $z\frac{P'}{ab}\cdot\frac{v^2}{2g}$ of the resistances of the whetted perimeter of the pool, whilst the denominator marks the changes which occur in the condition of the stream. To use it for our present purpose we must find the value of a_1 for some finite point on the axis of the stream, and then placing $l =$ to an infinitesimal distance from that point, the difference $a_1 - a_0$ should show the fluctuation of height due to a periodic change in the discharge. Now when $\sin a = z \frac{P}{ab} \cdot \frac{v^2}{2g}$, it is also by the law of the formula $= \sin \alpha'$ of that transverse section of which the second member shows the resistance; therefore

$$\sin \alpha = \sin \alpha',$$

therefore the velocity of the pool = the velocity of the stream, and the surface line of the pool is a parallel line with the bed; for $a_1 - a_0 = 0$, therefore it is circumstanced as in the original stream. There is no backwater, no difference of pressure and no vibration.

When $\qquad \sin \alpha = 0$

the surface becomes level, for there is no velocity, no flowage, therefore no resistance, $z \frac{P}{ab} \cdot \frac{v^2}{2g} = 0$; again $a_1 - a_0 = 0$; and there is no action, periodic or otherwise.

So we see that there are two points at which the vibrations cease: namely, when the water is sufficiently high to flow over the dam without much remou, as with a stream undammed, and with its surface a line nearly corresponding with the surface of the original stream; and again when the water is so low as to make the difference between the hydrostatic and hydraulic pressures very small. Of course these limits are much circumscribed by the inertia of a large body of water which has constantly a tendency to absorb and soften these vibrations. The most violent palpitation should then occur when

$$\sin \alpha = \left(z \frac{P}{ab} \cdot \frac{v^2}{2g} \right) m,$$

m being a new quantity to be found by a knowledge of the stream.

Again, if we put $\frac{2}{a} \cdot \frac{v^2}{2g} = 1$, or $\frac{v^2}{2g} = \frac{a}{2}$, or when the height due the velocity of the original stream becomes equal to one-half the depth of the same, we have

$$a_1 - a_0 = \infty.$$

Certainly nature admits no such differences as this. Yet, the fact beautifully follows from this, that when the height due the velocity of the original stream is equal to one half the depth of the same, the back water no longer retains the concave form, but, tending to rise infinitely, is checked by the action of gravity, falls backward upon the original stream, and tends to form a convex wave, with a nearly horizontal surface at a height above the bed of about one and one-half times the hydrostatic height. After this height becomes greater than half the depth, the wave is actually formed, and the water of amplitude flows to and rebounds from the foot of the stream. Bidone discovered this law, and Belanger has applied to it a formula.

The general formula which relates to this action we may gather from what proceeds. If h be the height of the remou, just before greatest action, $h - h_2 =$ height at the beginning of the lull that succeeds. The velocities are then

$$v = v_{,} - v_{,,}$$

And the times $= t = \sqrt{\frac{s_1 - s_2}{g}}$, s_1 and s_2 being spaces due to the two velocities combined with the time, t, of the vibration. Or the time of one vibration is equal to the time in which water falls through the height upon the dam, minus a space due to the velocity of different heights of remou, combined with the time observed. In other words, it is the time required for the quick water to draw a certain portion of nearly quiescent water into its own mean velocity—the quantity of quiescent water so drawn, and consequently the time, depending upon the regime of the remou. To this must be added the time of recuperation.

The action when $a - a_1 = \infty$ would be the formation of a remou, similar to the one described as seen by the writer of this in the first part of this paper, just at the point where the stream runs into the pool, which would cause one set of vibrations there, and perhaps another set similar to those on ordinary dams at the weir.

For a practical solution of this question, to prevent the vibrations, I am only prepared to recapitulate the foregoing examination into the nature of the action.

1st, We may consider that the vibrations become practically small when the inertia of the pool becomes sufficient to absorb them; therefore they are small when the quantity of water running over the dam is small in relation to the pool, and $\sin \alpha = 0$ or nearly.

2d, The same would be true if there were no backwater, or the stream retained its mean velocity unretarded, obtaining when the $\sin \alpha = \sin \alpha'$, as before, or when the surface becomes parallel with the bed; and again, should the pool be so filled as to make the bed become parallel with the surface.[1]

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[1] After the horizontal line (or line of hydrostatic amplitude) which bounds the surface of a pool just on the point of running over its dam is found, and the water begins to flow over, the longitudinal outline of the surface changes from its straight and horizontal form, and becomes a curve, which joins the water of flowage at some distance from the dam, and proceeding backward, after the law of the equation given in this paper, becomes asymptotic to the surface of the original stream. It is called the hydraulic amplitude. This curve marks by its changes every alteration or irregularity in the flowage, either of quantity or velocity or resistance, and having been carefully subjected to mathematical laws, answered as a touchstone to the deductions of this paper. And the law developed, not being dependent upon the form or nature of the weir, is a law for the periodic action of all fluids in a state of motion past an obstacle.

ART. XX.—*Remarks on the Carboniferous and Cretaceous Rocks of Eastern Kansas and Nebraska, and their relations to those of the adjacent States, and other localities farther eastward; in connection with a review of a paper recently published on this subject by M. Jules Marcou,*[1] *in the Bulletin of the Geological Society of France:* by F. B. MEEK.

IT is doubtless known to most of the readers of this Journal, that other explorers have long differed from Mr. Marcou, in regard to several important points in the geology of the Western States and Territories. During the autumn of 1863, it seems that he made an excursion to the West for the purpose of examining some of the localities in Nebraska respecting which he and others could not agree. While making these examinations, he was accompanied by Professor Capellini, an able Italian geologist from Bologna; but, as Mr. Marcou distinctly states that the views set forth in his paper are entirely his own, and that Prof. Capellini may have arrived at very different conclusions, it is due to the latter gentleman, that the geologists and amateur collectors familiar with western geology, who may read this review, should be informed that he is in no way responsible for any of the opinions expressed by Mr. Marcou.

Geological observations made by rail-road or steamboat travel being necessarily disconnected, Mr. Marcou's remarks, in the paper under review, so far as based upon personal examinations, relate to few isolated points along the shores of that part of the Missouri river ascended by him, and lying between St. Joseph, Missouri, and Sioux City, Iowa. By a glance at a map of the West, it will be seen that the distance between these two points, by an air line, is about two hundred miles; and that this part of the river forms the eastern boundary line of the new Territory of Nebraska, consisting of a portion of the southeast corner of a vast area formerly known by the general name of Nebraska Territory.

The first points at which Mr. Marcou seems to have touched, after taking steamboat at St. Joseph, were near Savannah, and in the vicinity of Iowa-point, where he saw outcrops of rock some sixty feet in thickness, consisting of bluish-gray clays, with intercalated layers of gray limestone, all showing a slight inclination of 3° or 4° in a W.N.W. direction. He says that he made no collections here, but that he saw *Productus* and *Terebratula*, showing it to be Carboniferous. (To this formation it had been referred by all others.)[2]

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[1] Une reconnoissance geologique du Nebraska; par M. Jules Marcou. Bulletin Geol. Soc. France; xxi, 132-147, January, 1864.

[2] It is worthy of note here that Dr. Owen collected at this locality species which he referred to *Nautilus tuberculatus*, *Productus Cora*, *Spirifer fasciger*, *Terebratula plano-sulcata*, and *Orthis Umbraculum*. (Report Iowa, Wiscon., and Minn., 135.) The

At another locality farther up the river, in the region of Nebraska City, he saw various exposures of rock, from which he constructed a section embracing some seventy feet of strata, consisting of reddish, gray, greenish and black clays, with a six inch seam of cannel-coal, and intercalated layers of light-colored dolomitic limestone, and toward the top a thin bed of sandstone containing numerous fragments of plants, some of which he thought like *Zamites* and *Walchia*;[3] the whole, according to him, showing a general inclination of only 5° to 7° to the N.N.W. These beds, he says, agree in color and other lithological characters with the Lower Trias of France, (*Dyas* of him,) to which horizon he refers them, and differ from the subjacent Carboniferous strata upon which they repose, according to him, unconformably, (sur lequel elles reposent en discordance de stratification). He does not, however, state that he anywhere saw the junction of these supposed two systems of strata, nor does he explain exactly the nature of the discordance of stratification. But we infer that he alludes to the fact of the Carboniferous beds showing at one place a slight inclination of 3° to 4° to the W.N.W.; and similarly slight inclination of the so-called "Dyassic" rocks, of 5° to 7° to the N.N.W., at another.

Geologists familiar with the numerous local undulations of strata in the west, where they often lie so nearly horizontal that their general inclination can only be distinguished from these local undulations by careful observations over considerable areas, will know how to appreciate such evidence as this. The improbability too (though within the range of possibilities) of a newer rock dipping at a higher angle than that upon which it reposes, and in a different direction, when the inferior rock is so nearly in its original horizontal position, will be apparent.

But from these rocks Mr. Marcou collected a number of fossils, which should clear up all doubts in regard to their age. These consist of the following forms, as identified by him, viz:—*Nautilus*, *Pleurotomaria*, *Murchisonia*, *Bellerophon*, *Panopæa*, *Edmondia*, *Avicula*, *Monotis*, *Ancella*, *Myalina*, *Bakevellia*, *Pecten*, *Lima*, *Orthis*, *Productus Prattenianus*, *Productus*, (undt.); *Chonetes mucronata*, *Spirifer* (*Martinia*) *Clannyanus*, *Spirifer* (undt.), *Terebratula*

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shell he always referred to *Spirifer fasciger*, is now well known to be *S. cameratus* Morton, and that which he called *Productus Cora*, is the *P. æquicostatus* Shumard, two of the most common and characteristic species of our Western Coal-measures.

The species of so-called *Terebratula*, mentioned by Mr. Marcou, is most probably *Athyris* (or *Spirigera*) *subtilita* Hall, as that shell is known to occur there, and at nearly all other places in the same rock, while Mr. Marcou habitually calls it *T. subtilita*.

[3] It should be rememberod that these identifications are not given on the authority of Prof. Capellini, whose opinion on such a question would have been worthy of consideration. I am also gratified to see that, since this gentleman's return to Europe, he has published a work at Bologna, in which he says that he dissents from Mr. Marcou in regard to the age of the rocks at this locality, and thinks that the fossils he saw are more like Carboniferous forms.

near *T. subtilita*, some small Corals, or Bryozoa, an *Apiocrinus*, and a Crinoid near *Encrinus moniliformis.*

That Mr. Marcou may have collected or seen, at Nebraska City, specimens he honestly believes to belong to all the genera and species mentioned by him, and that he did find some of these forms there, the writer is fully prepared to understand from a careful personal study of the same beds and their characteristic fossils at numerous localities in Kansas and Nebraska, as well as from familiarity with collections from that and other localities in the immediate vicinity. For instance, he knows that *Chonetes mucronata* Meek and Hayden, *Productus Prattenianus* Norwood, *Spirigera subtilita* Hall, *Spirifer* (*Martinia*) *planoconvexus* Shumard, *Spirifer cameratus* Morton, *Myalina perattenuata*, *Pleurophorus occidentalis* and *Sedgwickia? concava* M. & H.,[1] together with species of *Aviculopecten*, and one of those forms belonging to the same group as the so-called *Monotis speluncaria* of authors, (genus *Eumicrotis* Meek,) occur there. Dr. Owen also found at this locality (called Fort Kearney in his Report, there being no town there at the time of his visit), in the highest bed of limestone, *Productus costatus*, *P. Flemingii*, and great numbers of *Fusulina cylindrica.* The shell referred by Mr. Marcou to *Spirifer Clannyanus* is beyond any reasonable doubt the same called *S. planoconvexus* by Dr. Shumard, since it is known to occur there and at numerous other localities in the same beds and far below; while it is scarcely distinguishable from *S. Clannyanus.* It is, however, quite as nearly allied to, if not really identical with, the well-known Carboniferous species *S. Urii.* Indeed, specimens of this shell sent to Mr. Davidson from the Coal-measures of Illinois (where it has received from Mr. McChesney the name *Ambocœlia gemmula*) were pronounced by him undistinguishable from British specimens of *S. Urii.* Good wood-cuts of the same shell, from Kansas, sent to Mr. Salter of London for comparison, within the past year, were also referred to *S. Urii.*

Now these species, that is, *Fusulina cylindrica*, *Chonetes mucronata*, *Productus Prattenianus*, *Productus costatus*, *P. Flemingii*, *Spirigera subtilita*, *Spirifer planoconvexus* (or *Urii*), and *Spirifer cameratus* are the most common and characteristic forms of the Coal-measures of Kansas and Nebraska, Northern Missouri and Western Iowa, in the very beds which, it will be seen, Mr. Marcou refers to the Subcarboniferous. They are also there found in the Coal-measures associated with species of *Nautilus*, *Bellerophon*, *Pleurotomaria*, *Murchisonia*, *Edmondia*, *Aviculopecten*, *Monotis*, (so-called), and, especially in Kansas, Nebraska, and northwestern Missouri, with a form so nearly like *Panopæa* as to readily deceive more skillful paleontologists than Mr. Marcou professes to be.[5]

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[1] In describing the latter three species, the writer and Dr. H. thought the bed from which they were obtained might be Permian; but on afterward ascertaining that these shells are there and elsewhere associated with numerous well marked Coal-measure forms, they were satisfied that it does not belong to the Permian.

The name *Ancella*, in Mr. Marcou's list of Nebraska City fossils, is doubtless a mis-print of *Aucella*, there being no such name as *Ancella* known to the writer in Paleontology or recent Zoology. As some authors refer such forms as the so-called *Monotis speluncaria* to *Aucella*, it is probably one of these, which are common in the Coal-measures and Permian rocks of Kansas and Nebraska, to which he alludes. That he found here associated with all the Carboniferous fossils known to occur at this locality, the Jurassic genera *Aucella* (as illustrated and defined by Count Keyserling) and *Apiocrinites*, paleontologists may be pardoned for hesitating to admit.

In regard to *Encrinus*, the writer would remark that he has now before him from the Coal-measure outcrop at Bellevue, Nebraska, (referred to the Subcarboniferous by Mr. Marcou, as will be seen farther on,) the cup of a Crinoid, which in form and the arrangement of its plates, up to the summit of the first radials, as well as in the articulating surfaces for the reception of the succeeding range of pieces, seems to agree exactly with the corresponding parts of the genus *Encrinus*. None of the other parts, excepting as detached pieces, have ever been seen by the writer. The cup of the same or a closely allied species, beyond all doubt the same genus, occurs in the Coal-measures of Illinois; and separate plates of the same are not uncommon through nearly all the Coal-measures of Kansas and Nebraska. That a Crinoid with such a cup may be expected to be found nearly related to *Encrinus*, it is quite reasonable to suppose; but even if it should be found in all respects undistinguishable from that genus, would it therefore be philosophical to refer these beds at Nebraska City, and the Coal-measures of Illinois (placed by Mr. Marcou in the Subcarboniferous) with their great numbers of Carboniferous fossils, in the Permian, or the Dyas as he prefers to call it? If not, then all the arguments based upon the presence of this Crinoid, at Nebraska City, fall to the ground.[6]

Mr. Marcou lays great stress upon the fact that the Crinoids found by him at Nebraska City differ entirely from the numerous American Carboniferous forms hitherto made known. He perhaps forgets that nearly all our Carboniferous species, yet described or illustrated, have come from the Subcarboniferous deposits far below the Coal-measures. Some species, however, are known from our Coal-measures, and the fragments of a number of others have been seen, all of which differ widely from those known from the great Subcarboniferous limestones below the horizon of the Millstone grit.

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[5] The type here alluded to is a widely gaping, edentulous, very thin shell, truncated behind, and, when well preserved, covered with minute granules. In short, it is the type of a new genus related to *Allorisma*, to which the writer has applied the name *Chænomya*, in a work now in the press. The typical species, *Allorisma? Leavenworthensis* Meek and Hayden, occurs in the Coal-measures at Leavenworth City, Kansas, and others range much higher in the same series.

[6] See note at the end of this paper.

The next locality examined by Mr. Marcou is at the village of Plattesmouth, some fifty miles farther up the Missouri by an air-line. Here he saw another exposure of rocks, some forty-five to fifty feet in thickness, composed of grayish and dark colored clays, in places streaked with red, together with a six-foot stratum of yellowish dolomitic limestone; all of which he says agree lithologically with the Lower Trias of France and Germany—Permian of authors, (= *Dyas* of him), to which horizon he refers them. As these beds, however, differ somewhat in color and composition from those seen at Nebraska City, he thinks they belong to another and lower division of the so-called Dyas, which, as its name implies, consists of two divisions in Europe, and consequently must be expcetde to present the same feature in this country.

Here too he collected a number of fossils, which, according to him, eminently represent a Lower New Red, or Dyassic, Fauna, though he at the same time admits that these fossils are very like Carboniferous types. But, to let the fossils speak for themselves, through Mr. Marcou, they were *Chonetes mucronata*, *Productus Calhounianus*, *Spirifer Clannyanus*, *Terebratula* [*Spirigera*] *subtilita*, *T. Mormonii*, *Spirifer* (undt.), *Fusulina cylindrica*, together with (from an upper bed) *Monotis*, *Avicula* and *Pecten*.

We may also add that Dr. Owen collected here from these beds specimens he referred to *Fusulina cylindrica*, *Productus semireticulatus*, *P. carbonarius*, *P. longispinus*, *Orthis Umbraculum*, *Spirifer fasciger* (?), *Chonetes semiovalis*, *Allorisma sulcata*, some Corals, and a small undetermined *Spirifer*. (Report Geol., Iowa, Wisc., and Minn., 133.)

Now *Chonetes mucronata* of Meek and Hayden was found in Kansas, ranging through a great thickness of Coal-measures, and the type specimens upon which this species was founded were collected at Fort Riley nearly, two hundred feet below the lowest well marked Permian beds. This was the highest position at which they met with this fossil, after careful examinations of hundreds of exposures; and at this place it was found directly associated with a shark tooth which Prof. Leidy at once recognized as the same he had described from the Coal-measures of the Alleghany Mountains, Pennsylvania, under the name *Petalodus Alleghaniensis*. (See Meek & Hayden's paper, *Proceed. Acad. Sci. Philad.*, Jan., 1859, p. 17.)

As already explained, the species referred by Mr. Marcou to *Spirifer Clannyanus* is the *S. plano-convexus* Shumard, which was originally described from this very locality. *Spirigera subtilita* all know to be a common characteristic Coal-measure species, from Western Pennsylvania to the Rocky Mountains, and from Nebraska and New Mexico. Mr. Marcou figures it himself, under the name *Terebratula subtilita*, in his *Geology of North America*, even as a Mountain Limestone species, from Utah and New Mexico. His so-called *Terebratula Mormonii*, is a *Retzia*, dedicated by him to the Latter Day Saints, from the fact that he first found it at their Capital City. It is worthy of note, however, that he figures and describes it as a Mountain Limestone species in the work just mentioned. So it would seem this little shell, in migrating eastward, obtained a long lease of life, since it here turns up, according to the same authority, in the so-called Dyas. The identity of the fossil is not questioned; indeed the writer and Dr. Hayden long since identified it, ranging through a great thickness of Coal-measures in Kansas, and northwestern Missouri. The notable point is, that it should be in Utah a Mountain Limestone species, and here at Plattesmouth, part of an eminently characteristic Lower Triassic Fauna!

The *Productus Calhounianus* of Swallow, mentioned by Mr. Marcou, is the same referred by Dr. Owen to *P. semireticulatus*, from which, if separable at all, it is certainly with difficulty distinguished. Whether distinct or not, however, it is known, as stated by Prof. Swallow in describing it, to range far down through all those very Coal-measures in Kansas and Missouri which Mr. Marcou refers to the Mountain Limestone. The geological position of *Fusulina cylindrica*, in this country has already been explained. *Productus longispinus*, *P. carbonaria* and *Orthis Umbraculum*, or at any rate species so referred by Dr. Owen, and sometimes by others, are common to the Coal-measures and the Subcarboniferous beds in the West. The species always called *Spirifer fasciger?* by Dr. Owen, as explained in another place, is *S. cameratus* Morton;—the very specimen of it figured by Owen was from the Plattesmouth locality. It is known to be everywhere characteristic of the Coal-measures, from New Mexico to Nebraska, and from Western Pennsylvania[7] to the Rocky Mountains.

The group of shells to which the name *Monotis* is often applied in this country and England, and by some continental writers, (though generically distinct from the Triassic *Monotis salinarius*, the type of *Monotis* Bronn,) is usually regarded in Europe as a Permian type. It is well known, however, to range through a great thickness of upper Coal-measures in Kansas and Northwestern Missouri, referred by Mr. Marcou to the Subcarboniferous. The names *Avicula* and *Pecten* are used so loosely by paleontologists, that they may be said, as generally understood, to range from the Silurian to our existing seas.

Now, how any geologist, having even a limited knowledge of American Carboniferous rocks and fossils, could regard a group of forms such as those mentioned above from Plattesmouth, as eminently a Lower Triassic (or Dyassic) fauna, seems inconceivable, excepting upon the supposition that he labors under some kind of a hard mental twist or bias on the subject of determining the age of rocks by their lithological characters.

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[7] See Prof. Rogers' Report Pa., ii, 833, fig. 694.

After disposing of the so-called Lower Dyassic rocks at Plattesmouth, Mr. Marcou takes boat again, and ascends the Missouri some fifteen or more miles to Bellevue, north of the broad alluvial valley of Platte river. Here he saw a small exposure of rocks, some fifteen feet in height above the river, composed of whitish and yellowish limestone, and pale blue clays, altogether presenting different lithological characters from the outcrops seen below the Platte, and according to this favorite test of his, belonging to a very different epoch, or in other words to the Subcarboniferous.

Here he collected from the limestones, according to his identifications, the following fossils, viz:—*Productus Flemingii*, *P. semireticulatus*, *P. Cora*, *P. punctatus*, *P. scabriculus*, *P. pustulosus*, *P. pyxidiformis*, *Spirifer striatus*, var. *triplicatus* Hall, *S. Rocky-Montanus*, *S. lineatus*, *Terebratula subtilita*, *T. plano-sulcata*, *T. Royssii*, *T. Utah*, *Myalina*, *Nautilus*, and spines of *Archæocidaris*.

It may be as well to add just here, that Dr. Owen gives the following list of fossils collected by him at this locality, viz: *Fusulina cylindrica*, *Productus punctatus*, *P. Cora*, *P. costatus?*, *P. Flemingii*, *P. Humboldtii?*, *Spirifer fasciger?*, *Orthis Umbraculum*, *Terebratula plano-sulcata*, and a *Bellerophon* near *B. hiulcus*. (Rept. Iowa, Wisc., and Minn., p. 133).

From the same outcrop, the writer has now before him (collected by Dr. Hayden) *Productus costatus*, or a common form of the western Coal-measures generally referred to that species, *Productus Rogersii*, together with the Coal-measure form usually called *Productus punctatus*, *Spirigera subtilita*, *Rhynchonella Utah* (= *Terebratula* Marcou), *Terebratula bovidens* Morton, (= *T. millipunctata* Hall), *Spirifer Kentuckensis*, *S. cameratus*, an *Allorisma*, and the peculiar *Encrinus*-like Crinoid already mentioned.

As has been explained in another place, the shells from these rocks, referred by Owen and Mr. Marcou to *Productus semireticulatus* and *P. Cora*, are the *P. Calhounianus* Swallow, and *P. æquicostatus* Shumard; and, whether distinct or not from the species first named, they are very common in our Western Coal-measures. *P. punctatus* of their lists is *P. tubulospinus* of McChesney; which is scarcely distinguishable from the *punctatus*. At any rate, it is, as remarked by McChesney, very common in the Coal-measures "throughout the Western States." The same shell also occurs in the same position twenty-eight miles below Wheeling, in Ohio. *P. scabriculus* of Marcou's list is, beyond reasonable doubt, the widely distributed Coal-measure species, *P. Rog-*

ersii of Norwood and Pratten, as it is known to occur there, and is figured by Mr. Marcou under the name *P. scabriculus*, in his *N. Am. Geology*. *P. pustulosus* of his list, judging from the figure formerly given by him under that name, may be a variety of the *Rogersii*. *Spirifer striatus*, var. *triplicatus* Hall, (= *S. fasciger* of Owen's list), is *S. cameratus* Morton, everywhere common in the Coal-measures, and unknown from any lower position. *S. Rocky-Montanus* Marcou, is unknown to the writer from any locality east of the Black Hills. *S. lineatus* of Marcou's list is undoubtedly the same shell called *S. perplexa* by McChesney. (*Trans. Chicago Acad.*, 1). It is common in the upper Coal-measures of the West, being, as McChesney correctly states, found "in the Upper Coal-measures, in almost every part of the country where rocks of that age exist." It seems to differ from the Subcarboniferous species generally referred to *lineatus* mainly in being uniformly smaller. *Terebratula plano-sulcata* of Owen's and Marcou's lists is almost beyond doubt the *Athyris orbicularis* of McChesney; at any rate, that is the species figured by Mr. Marcou in his *North American Geology* under the name *Terebratula plano-sulcata* Phillips. It may or may not be identical with Phillips' species; but, as McChesney correctly states, it occurs in "the Coal-measures, particularly the upper portion, extensively distributed in the West." *Terebratula Royssii* is doubtless the *Spirigera* generally referred to that species in the West, but described by McChesney as *Athyris differentis*. Whether identical or not with the *S. Royssii* it is a common Upper Coal-measure form, as remarked by McChesney. *Terebratula Utah* of Marcou is a *Rhynchonella*, and, as long since shown by the writer and Dr. Hayden, is a common companion of the *Retzia Mormonii*, through all the Upper Coal-measures of Kansas and Northwestern Missouri. *Orthis Umbraculum*, more properly *Streptorhynchus Umbraculum*, or at least the shell figured under that name by Owen, is very common in the Coal-measures of Kansas, and ranges up to, if not into, the Permian.

Terebratula bovidens Morton (= *T. millepunctata* Hall) first described from the Coal-measures of Ohio, is a very common species in rocks of that age in the West, and unknown in any lower position. The same may also be said of *Spiriferina Kentuckensis* Shumard.

So we have here a group of fossils which any geologist, or mere amateur collector, acquainted with the forms characterizing our Carboniferous and Subcarboniferous rocks of the Mississippi valley, would at once, and without a moment's hesitation or doubt, refer to the Coal-measures. Some few of them are such as appear to be common to the Coal-measures and Subcarboniferous rocks, or are supposed to be, but all the others are wholly unknown below the horizon of the Millstone grit.

Again, it should be remembered that, of the foregoing lists, *Fusulina cylindrica*, the *Encrinus*-like Crinoid, *Spirigera subtilita*, *Productus semireticulatus*, (or *Calhounianus*), *Productus costatus*, *P. Flemingii*, (= *longispinus*), *Spirifer cameratus*, and *Orthis Umbraculum*, all likewise occur at the very localities, and in the very beds, referred by Mr. Marcou to the so-called Upper and Lower Dyas. It may also be added that, in the interior of Kansas, as well as at some places in Nebraska and Iowa, all the other Bellevue species occur in the very beds called Lower Dyas at Plattesmouth, and some of them, particularly in Kansas, in much higher positions.

In short, all the rocks seen by Mr. Marcou on the Missouri, from St. Joseph to the Cretaceous above Bellevue, belong to one unbroken series of Upper Coal-measures, as was first shown by Prof. Swallow; with possibly the exception of some of the highest out-crops near Nebraska City, where there is a downward undulation, that may have left portions of the Permian on the higher parts of the country. The few little isolated sections seen by him constitute but a mere fractional portion of this series; while his reference of these several outcrops to such widely different epochs, and his supposition that the beds he calls Mountain Limestone, form island-like masses, between those he refers to the Permian or so-called Dyas, were deposited unconformably, however honestly believed by him, may be all set down as purely imaginary. If he had gone out into the interior, where this series is much more extensively developed, and followed carefully up the smaller streams, he would have seen exactly the same beds he at one place calls Subcarboniferous, and at others Upper and Lower Dyas, with intermediate strata, all following each other in regular succession without the slightest physical or paleontological break. He might there also, by the same method of examining isolated sections, and applying the same lithological tests, and loose interpretation of fossil evidence, have found material enough to divide the so-called Dyas into twenty, or as many more, subdivisions as he pleased.

In 1859, the writer and Dr. Hayden, who were directly interested in the Permian discovery, and naturally desired, and confidently expected, to find somewhere a break between the Permian and Carboniferous rocks of that region, spent more than a month in traversing hundreds of miles of the districts in Kansas where these rocks are best developed. They did not hurry from point to point, but followed up the valleys of the streams on horseback, with a camping party, provided with a wagon and team for the transportation of supplies, specimens, &c. Yet, after carefully examining the various beds and seams, inch by inch, collecting all the fossils they could find, and carefully keeping separate those from the different strata and seams, they completely satisfied themselves that there is no where in the whole series any break indicating a marked change of physical conditions. They found, it is true, alternations of clays, limestones, shales, sandstones, &c., but all resting conformably one upon another, and inseparably linked together by their organic remains. Starting from Leavenworth City on the Missouri, where the same Coal-measure rocks which Mr. Marcou will insist belong, in Iowa, Missouri and Illinois, to the Mountain Limestone, occur characterized by such fossils as *Fusulina cylindrica*, *Spirifer cameratus*, *S. plano-convexus*, (= *Clannyanus*, of Mr. Marcou's lists), *Productus costatus*, *P. semireticulatus*, *P. punctatus*, *P. æquicostatus*, (or at any rate the forms usually so-called in the West), *P. Rogersii*, *Spirigera subtilita*, *Terebratula bovidens*, *Retzia Mormonii*, *Rhynchonella Utah*, &c., (but even here also containing a species belonging to the same genus, and near the so-called *Monotis speluncaria*),—they continued their researches through the succeeding strata to the Permian in the interior. Through a great thickness of these rocks, they found the Coal-measure fossils persistent and abundant, but occasionally associated with a Permian type.[*] The higher they ascended in the series, the greater was the proportion of Permian types observed, while the Carboniferous types gradually disappeared, until at last, above a certain horizon, and near the upper part of the series, only Permian forms were met with, with the exception of the ubiquitous *Spirigera subtilita*, or a shell that could not be distinguished from it. (See this Journal, [2], xxvii, 424, 1859; *Proceed. Acad. Nat. Sci. Philad.*, Jan. 1859, p. 8; *Dr. Newberry's Rept. Geol., Ives' Colorado Exped.*, p. 112, &c.)

One of the most remarkable opinions, however, set forth in the paper under review, respecting these rocks is, that all the Coal-measures of Northern Missouri, and of Iowa and Illinois, belong, not as supposed by all others, to the horizon of the true Coal-measures of Europe, but to the Mountain Limestone series. He also thinks that possibly the Coal-measures of Indiana, Ohio and Michigan, and a part of those of Pennsylvania, Virginia and Kentucky, may belong to this lower horizon.

That beds of coal occur in the lower part of the Millstone grit in Pennsylvania, Western Virginia, portions of Kentucky, Tennessee and Arkansas, precisely as in England (see a paper by Messrs. Hull & Green, *Quart. Jour. Geol. Soc.*, Lond., Mar., 1864, p. 246)—while in the western part of Arkansas a great thickness of shale, sandstone, &c., above the conglomerate, is believed to be barren of coal—are well known facts. But to maintain that the Coal-measures of Northern Missouri, Iowa and Illinois belong to this or any lower horizon, to say nothing of those of the other States mentioned, beneath which the Millstone grit is so well developed, is to state a proposition the fallacy of which is manifest to all who have studied these rocks with even a moderate degree of care. In the first place, the whole physical structure of our Carboniferous system, and its relations to the Devonian below, and the Permian above, show at once that the Coal-measures of the Mississippi valley occupy precisely the same horizon as those of Europe. Here, as there, we have first, above the Devonian, the Subcarboniferous or Mountain Limestone group; then the Millstone grit, and above the latter the regular Coal-measures, which in Kansas, shade gradually upward into the Permian. The existence of the extensively developed regular Coal-measures above the Millstone grit in Pennsylvania, Western Virginia, Ohio, Kentucky, Tennessee, Indiana, and Southern Illinois, are facts so well known as scarcely to need mentioning here. It is true that the Millstone grit, which is found from four to five hundred feet in thickness beneath the Coal-measures of Southern Illinois,[*] thins out in a north-westerly direction, so that, farther north in Illinois, in Iowa and Missouri, the Coal-measures are found reposing directly upon the Subcarboniferous rocks. If we take this as an evidence, however, that the Coal-measures there belong to the Mountain Limestone series, we might upon the same principles argue that at other places in these states the coal-bearing strata belong to the Devonian, or even to the lower part of the Lower Silurian; for it is well known that, at some localities there, all the intermediate rocks are wanting, and the Coal-measures are found in direct contact with these older rocks.

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[*] A considerable thickness of these intermediate rocks were included by Prof. Swallow in the Permian. The writer and Dr. Hayden, although satisfied that this intermediate series was not separable by any visible break from the Carboniferous below, or the Permian above, thought that it might be convenient to designate it as Permo-Carboniferous.

If we had no stratigraphical evidence, however, in regard to the parallelism of our Coal-measures with those of Europe, their flora alone would be sufficient to settle this question. Lesquereux, who was especially commissioned to study the fossil plants of the Coal-measures in connection with the State surveys of most of the Western States, containing Upper Carboniferous rocks, including Illinois, says, "if we admit the generic distribution of the fossil plants of the coal, as it has been established by Brongniart in his *Tableau des Genres* (certainly the best that has been attempted, either before or after him), all the European genera, even the undefined genus *Aphlebia* Sternb., have representative species in the Coal-fields of America."[10] He also shows by tables in the same article, that out of a list of about 350 known species of our Coal-measure plants, 150, or approaching one-half, are identical with European Coal-measure species. Dr. J. S. Newberry, an equally good authority in this department of paleontology, had previously arrived at very nearly the same conclusion, from a careful study of the Carboniferous flora of Ohio and some of the neighboring states.[11]

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[*] Mr. H. Engelmann, an assistant in the Illinois State Geographical Survey, gives its thickness in the southern part of that State at five hundred feet, and gives a section showing it to rest upon the upper Archimedes Limestone group, everywhere regarded as the upper member of the Subcarboniferous or Mountain Limestone series. (*Trans. Acad. Sci., St. Louis*, Nov. 1862, p. 188.)

In regard to the identity of the Coal-measures of Illinois, with the regular Coal-measures (overlying the Millstone grit) of Ohio, Pennsylvania, Kentucky, and other neighboring states, it is only necessary to again quote Lesquereux, who has given especial attention to tracing out the parallelism of the subordinate beds at distantly separated localities in the coal fields of the Middle and Western States. On this point he says, (this Jour., [2], xxx, 367,) "Such is, nevertheless, the uniformity of the distribution of the strata of our coal-basins, that a section made in Western Illinois or Western Kentucky, or in any part of the coal-fields in these States, will prove comparatively similar (that is with some differences in the thickness of the strata,) to any section made in the coal-fields of Pennsylvania or Ohio." Now

[10] This Journal, [2], xxx, 65.

[11] The fauna of our Coal-measures has not been compared with that of the equivalent rocks of Europe, in the same detail as its flora. Indeed, judging from the publications on the Mollusks, and other invertebrate remains of the European Coal-measures, our rocks of this age, particularly the upper members in the West, are far richer in animal remains than those of the Old World. Another fact that gives to our Coal-measure fauna a more Subcarboniferous aspect is, that during the deposition of these rocks in this country the sea seems to have much more frequently reoccupied the area of these formations than in the Old World. For, whatever theory we adopt in regard to the formation of coal, to meet all the requirements of the phenomena here presented, we must admit the frequent presence of the sea, since we find Corals, Brachiopods, Crinoids, marine types of Gasteropods, Acephala, &c., occurring through hundreds of feet of Coal-measure rocks in the West. Indeed, during more than twenty years' familiarity with the fossils of the Western Coal-measures, the writer has never met with any fresh or brackish-water types or terrestrial remains, other than plants. The presence of these marine remains is not the exception, but the rule, particularly in the upper and middle members of the western Coal-measures; they are found over wide areas, and through great thicknesses of strata, and in a profusion and state of preservation that precludes the possibility of accounting for their presence by the supposition that they were transported there by currents or earthquake waves. They occur not only in the limestones, shales, sandstones, &c., alternating with the numerous seams and beds of coal, but in some rare instances directly in the coal itself. The writer has a specimen of impure coal from Illinois, in which there is a very thin *Aviculopecten* entirely replaced by pyrites, which, when the block of coal was first broken open, presented almost the brilliancy of burnished gold. In Ohio also, numerous shark teeth and entire specimens of other marine fishes have been found by Dr. Newberry embedded directly in cannel-coal. (See this Journal, [2], xxiii, 212.) Dr. Newberry also found, at another locality in Ohio, specimens of *Solemya* (a marine shell), flattened between the laminæ of good coal. In other instances, beds of coal are known to shade upward into a black, more or less richly bituminous mass, containing great numbers of the shells of Brachiopoda, the delicate, unbroken spines of which can be seen piercing like needles the black matrix.

this conclusion, it should be remembered, is not based upon structure alone, or other lithological characters, but also upon a careful and thoroughly scientific investigation and comparison of the fossil plants characterizing each bed or subordinate stratum.

But it is not alone upon the evidence of structure, and their fossil flora, that the Illinois Coal-measures are known to belong to the same horizon as the regular Coal-measures of Indiana, Kentucky, Ohio, Pennsylvania, &c., for we have also the unmistakable evidence of its group of animal remains. It is true, as in all other formations, species sometimes occur in these rocks in one State that do not in another, but a miscellaneous collection of shells, Corals, Crinoids, Bryozoa, &c., from the Coal-measures of any part of Illinois, would be at once referred to that horizon by any person familiar with the forms characterizing rocks of the same age in any of the States above mentioned. Long lists of species might be cited to illustrate this point, but it is wholly unnecessary; we may remark, however, that *Pleurotomaria sphærulata*, *P. tabulata*, *Euomphalus catilloides*, and *Macrocheilus primigenius*, of Conrad, which are among the most characteristic fossils of the Coal-measures, not only in Illinois, but in Iowa, Missouri, and in part in Kansas, were first described by Mr. Conrad from the regular Coal-measures of Western Pennsylvania.

In relation to the Coal-measures of Illinois belonging to the same horizon as those of Missouri and Iowa, it is scarcely necessary to say anything, as all in this country who have given any attention to the subject know this to be the case, and even Mr. Marcou admits it. Indeed, we have the clearest evidence that the Illinois Coal-field was once continuous with that of Missouri and Iowa, from which it is only separated by the broad valley of denudation scooped out by the Mississippi river. Following the Missouri and Iowa Coal-field westward, we find that it passes uninterruptedly into Kansas and the southeast corner of Nebraska —the valley of the Missouri, owing to the northwestward inclination of the strata, not going deep enough to cut it into two distinct fields.

Throughout all this area, these rocks are characterized by essentially the same fauna. As in all other formations, some species are local in their geographical range, but the majority are not, and, as elsewhere stated, most of them range into the Coal-measures of Kentucky, Indiana, Ohio, and some even into Pennsylvania, while comparatively very few of them have even been suspected of being identical with forms occurring in any lower position.

As much of the evidence relied upon in this review in discussing the relations of the Western Carboniferous and Permian rocks, is derived from the presence or absence of certain types of Brachiopoda, it is but fair that we should not dismiss this part of the subject without letting it be known that Mr. Marcou does not admit the validity of the testimony of this class of Mollusks. His remarks on this point, however, are so extraordinary, that, for fear of being suspected of having done him an injustice in the translation, they are here given in his own words, and in his own language. They are as follows:—"Je suis arrivé à la conviction qu' après les foraminiféres, les brachiopodes sont les plus mauvaises fossiles dont on puisse se servir comme fossiles caractéristiques des formations, et qu'en réalité ils ne sont meme pas du tout les *Leitmuschel.* J' ignore ou les zoologistes placent les brachiopodes, ou meme s'ils sont d' accord entre eux sur la place á leur assigner; mais ces sont certainement des etres tres-inferieurs et plus bas méme dans la série que les coraux, si j'en juge du moins d'après leur utilité pour la geologie pratique."

This is certainly very hard on the Brachiopoda; it was bad enough to place them in any sense of the word below the Corals but to deny them the right, next to the Foraminifera, to testify in regard to the age of the very rocks to which their shells have so largely contributed is still worse. Whatever zoologists may think of this view respecting the rank of the Brachiopoda, they will certainly accord to it the merit of originality. Geologists and paleontologists, however, are not likely to agree with him in regard to the value of this class of Mollusks as a means of distinguishing strata, as their almost united testimony goes directly to the contrary. Various quotations might be made to show this; but one from Mr. Thos. Davidson of London, the highest authority on the Brachiopoda, particularly the fossil forms, will be sufficient. After speaking of their almost universal distribution in marine strata, he says, "their value to the geologist is consequently very great; and, as they so commonly fall under his hammer, where other classes are often but sparingly represented, they must, therefore, be looked upon as excellent data for the age of deposits; for, although some few individual forms pass from one stage to another, the generality are limited to definite horizons."[33]

That in the hands of one who first makes up his mind in regard to the age of a formation from its color and other lithological characters, and then sets to work to interpret its fossils accordingly, without the ability or inclination to discriminate between the genera *Terebratula*, *Spirigera*, *Retzia*, and *Rhynchonella*, the Brachiopoda will be found of little use, and their testimony discordant, is quite natural to suppose. Under other circumstances, however, the result is always very different. Yet after all, the Brachiopoda have less reason to complain of Mr. Marcou's practical, than of his theoretical dealings with them, for it will be remembered that, so far as he relied at all upon paleontological evidence, he identifies the rocks at Bellevue, with a particular formation in New Mexico, and both with the Mountain Limestone of Europe, almost *solely* upon the testimony of certain *Brachiopoda.*

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[33] British Foss. Brach. Genl. Introduction, p. 1.

After completing his researches in the region of Bellevue, Mr. Marcou continued his progress up the Missouri to examine a sandstone formation extending along that stream for some distance below Sioux City, located at the mouth of Sioux River. In regard to the age of this formation, the readers of this Journal will remember that Mr. Marcou and some geologists in this country have differed widely. In 1853, the writer and Dr. Hayden examined it while returning from an expedition to the Bad Lands for Prof. Hall. They found but a few badly preserved fossils in it, but left the locality under the impression that it was probably Cretaceous, as it was immediately overlaid by rock undoubtedly of that age; which opinion was adopted in a paper published by Prof. Hall and the writer in *Mem. Am. Acad. Arts and Sci.*, 1856.

In his Geological Map, however, of N. America, published in 1855, (*Ann. des Mines,* [2], vii), Mr. Marcou had colored this rock here on the east side of the Missouri, *Mountain Limestone*, and along the west side, *New Red Sandstone.*

In papers published in 1856–7, Dr. Hayden and the writer still maintained that it was of Cretaceous age.

Again, in 1858, in reissuing his Geological map, along with his *Geology of North America*, Mr. Marcou left this rock colored as before on both sides of the Missouri, and on page 143 of the *Geology*, says, "the red formation of the vicinity of Sioux City, lying upon the Carboniferous rocks, is of the age of the New Red Sandstone."

About the same time, Dr. Hayden and the writer maintained that the numerous modern types of dicotyledonous leaves found in this rock could not belong to an older epoch than Cretaceous. But in a paper published in the *Archives des Sci. Bibliothèque Universelle*, June, 1858, Mr. Marcou unhesitatingly referred it, from our later description, to the *Jurassic.* Being perfectly satisfied that even this could not be the case, and in order to convince Mr. Marcou and others of the fact, the writer made sketches of some of the leaves found in this rock, and sent them to Prof. Oswald Heer, of Zurich, Switzerland, one of the most eminent botanical paleontologists of Europe, Dr. Newberry, one of our best authorities in this department of paleontology, being then in New Mexico. Before Prof. Heer's reply was received, Dr. Newberry returned, and the specimens of fossil leaves were submitted to him, and he fully agreed with us that the rock must be Cretaceous. The modern affinities of the leaves showed it could not be older, and its position beneath unquestionable Cretaceous strata, in a district where the undisturbed condition of all the rocks precluded the supposition that there might have been an overthrow, settled the question that it could not be newer.

Soon after Dr. Newberry's return, a letter was received from Professor Heer, who was evidently deceived by the unusually modern affinities of the leaves (and doubtless, though unconsciously to himself, in some degree by the theoretical views of Mr. Marcou, who was then in Zurich), stating that he could not regard these leaves of Cretaceous age, but that they appeared most nearly allied to Miocene types. Almost simultaneously, a printed pamphlet was issued by Mr. Marcou at Zurich, addressed to Meek and Hayden, in which he took the ground that they had included in this leaf-bearing formation all sorts of rocks, *excepting* Cretaceous, and endorsing Prof. Heer's suggestion that the beds containing these leaves are of Miocene age.

Notwithstanding the fact that Dr. Hayden and the writer had against them as weighty an authority as Prof. Heer, they continued to maintain that the rock is, nevertheless, Cretaceous.

After having expressed so many and such widely different opinions in regard to the age of this formation, we may readily understand that it must have been with no ordinary degree of interest that Mr. Marcou caught the first glimpse of these sandstone escarpments as he ascended the Missouri; especially as he had with him a skilful botanical paleontologist, to assist in unraveling the knotty question. After a thorough exploration, however, of the various out-crops in the vicinity of Sioux City, and along the Missouri below there, both of these gentlemen completely satisfied themselves that this formation *is* Cretaceous, and *only* Cretaceous, as had been from the first maintained by all in this country. This they both stated in conversation at Washington City, and Mr. Marcou frankly admits it in the paper under review. And thus ends one long mooted question in the geology of the West.

There are a few points, however, in regard to this formation, upon which the writer cannot agree with Mr. Marcou. In the first place, he (Mr. Marcou) considers it a fresh-water formation, because he found in it shells of a *Cyrena;* (for which he proposes the name *C. Nova-Mexicana*, from the supposition that it is identical with a species found by him in New Mexico). This is undoubtedly the *Cyrena arenaria* of Meek & Hayden,[13] which was long since described from the same locality and position. Its presence there, however, by no means proves this to be a fresh-water deposit, since the writer and Dr. Hayden found directly associated with it casts of an *Axinæa* (= *Pectunculus*), figured and described by Hall and Meek in the *Memoirs Am. Acad. Arts and Sci.* Dr. Hayden and the writer have also described from there a *Mactra*, under the name *M. Siouxensis*, and a *Pharella*. As *Pectunculus* and *Mactra* are marine genera, and *Pharella* and *Cyrena* are brackish-water types, it is manifest that this rock was deposited in a bay or estuary, which must have been alternately brackish, and salt enough to sustain marine Mollusks. The nature of the sediment composing it, as well as the numerous leaves and even trunks of trees, at some places found in it, attest the fact of its being a shore deposit.

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[13] We first described it as a *Cyprina*, from imperfect specimens, but afterward referred it to the genus *Cyrena*.

Again, while admitting this to be a Cretaceous rock, Mr. Marcou seems to think he is differing from Meek & Hayden, in not referring it to the oldest Cretaceous, and in placing it on a parallel with the New Jersey Cretaceous. In this, however, he is mistaken. We placed it at the base of our section of the *Cretaceous rocks of the Upper Missouri Country*, which position it undoubtedly holds. But we have not, in our more recent papers, especially referred it, even provisionally, to the horizon of the oldest Cretaceous of Europe. By looking at our paper published in the *Proceed. Acad. Nat. Sci. Philad.*, Dec., 1861, page 418, it will be seen that we there refer this rock to the position of the Gray Chalk of English geologists. It will also be seen there, and in our other papers, that we have, almost from the first, maintained that this formation is the exact equivalent of the leaf-bearing beds on Raritan River, New Jersey, forming the inferior member of the Cretaceous rocks of that State.

Mr. Marcou also has some remarks on the question of the parallelism of this sandstone and the overlying *Inoceramus* beds, with the upper part of his Pyramid Mountain section in New Mexico. We have not the space to enter upon the discussion of this question here, nor is it necessary. The able review of this whole subject in Dr. Newberry's report as Geologist of Ives's Colorado Expedition, and Dr. Shumard's several papers on the geology of Texas, place this question in so clear a light that probably no one but Mr. Marcou has any doubts on the subject.

Note on a new Crinoid, referred to on page 164.—Since the remarks on page 164 were in type, Mr. Worthen has sent me specimens of the Crinoid there alluded to, from the Coal-measures of Illinois, which show that, however much it may resemble *Encrinus*, it still presents differences of structure believed to be of generic importance. These differences consist in the presence of *a series of true subradial pieces;* while it differs from the structure (at least the normal) of *Encrinus*, in having but *two primary radials*, instead of *three*, in each ray. A European specimen of *Encrinus* in the private collection of Smithson, deposited at the Smithsonian Institution, shows five very minute pieces within those generally considered the basal plates. If these minute pieces were developed so as to assume the character of true basal plates, then those generally regarded as such would become subradials, as in our Crinoid. As this, however, seems never to have been the case in true *Encrinus*, it is reasonable to infer that our Carboniferous form presents other differences entitling it to rank as the type of a distinct genus. The genus may be named and characterized as follows:

ERISOCRINUS, Meek & Worthen. Basal plates 5; subradials 5; radials 2 × 5; anal and interradial pieces none. Column and arms unknown. We know the following species:—

1. *E. typus* M. & W.—Body, below the summit of the first radial pieces, basin-shaped, rounded below, and subpentagonal in outline above; composed of thick, smooth, scarcely convex plates; breadth 0·60 inch, height 0·24 inch. Basal pieces small, about half hidden by the column; subradials three times as large as the basal pieces, equally hexagonal: first radial pieces much larger, of equal size and form, being all wider than long, pentagonal in outline, and broadly truncated above for the reception of the succeeding radials, which are nearly as large, equally pentagonal, and support upon their superior sloping sides either the first brachial pieces, or a series of secondary radials.—From the Coal-measures, near Springfield, Ill.

2. *E. Nebrascensis* M. & W.—Differs from the foregoing in having the cup below the summit of the first radials proportionally deeper, in consequence of the larger size of the subradial pieces, but may be only a variety of the same. Breadth, 0·69; height to top of first radials, 0·35 inch.—From the Coal-measures, Bellevue, Nebraska.

ART. XXI.—*Analysis of a Carbonate of Lime and Manganese, (Spartaite of Breithaupt,) from Sterling, Sussex County, New Jersey;* by S. W. TYLER, A.B. Communicated, with remarks, by C. U. SHEPARD.

MORE than a year since I supplied Mr. Tyler (a graduate of this College) with very fresh specimens of this mineral, to be analyzed during his residence at the Göttingen Laboratory. He has just forwarded to me, under date of Nov. 10th, the results he has obtained.

"Sp. gr. =2·815. Before the blowpipe, decrepitates and becomes black without fusing. With soda and microcosmic salt, gives the well-known reaction of manganese. Not acted on by acetic acid, but dissolves easily with effervescence in cold chlorhydric acid.

Analysis gives,

MnO,	13·79	as 1 : 4 : 5,
CaO,	43·65	
CO^2,	42·01	

which leads to $(\frac{1}{5}MnO+\frac{4}{5}CaO)\,CO^2$ as the formula of the species.

	Calculated.	Obtained.
MnO,	13·79	13·79
$4CaO$,	43·49	43·65
$5CO^2$,	42·72	42·01
	100·00	99·45"

This is the same mineral as that referred to by Rammelsberg, page 209 of his *Handbuch der Mineralchemie*, as analyzed by Jenzsch and Richter with the following results:

	By Jenzsch.	By Richter.
Carbonic acid,	40·77	44·04
Lime,	48·75	47·92
Magnesia,	0·92	1·21
Protox. iron	0·38	7·13
Protox. manganese,	6·83	
Oxyd of zinc,	0·38	....
Water,	0·32	....
	98·35	100·30

Rammelsberg gives the following as an approximative formula, based on the foregoing:

$$6\dot{Ca}\bar{C}+\left.\begin{matrix}\dot{Mn}\\ \dot{Mg}\end{matrix}\right\}\bar{C}.$$

Regarding the magnesia, iron and zinc as accidental, I have only to add, respecting the species, that in color it is generally a very pale reddish white, and where associated with dysluite and jeffersonite (hedenbergite) of a bluish gray tint, sometimes (from partial decomposition) yellowish or reddish brown. It cleaves with facility; and the rhombohedrons thus obtained afford, both by the common and the reflective goniometer, angles rather over than under 106°. The planes are striated parallel to their longer diagonals. With borax before the blowpipe the characteristic manganese reaction is speedily produced.

This mineral had for years been recognized in my cabinet as a double carbonate of lime and manganese, and called by me *calcimangite*, a denomination which I have only recently learned has been anticipated by Breithaupt's *Spartaite.* But as this last name is so nearly synonymous with Spartalite, (given in 1852 by Brooke and Miller, to the red oxyd of zinc), I would suggest the substitution of calcimangite for it, as an appellation that may serve to prevent confusion, the more especially as the two minerals occur together.

Amherst College, Dec. 14, 1864.

ART. XXII.—*Contributions to the Chemistry of Natural Waters;* by T. STERRY HUNT, A.M., F.R.S.

IT is proposed to divide this essay into three parts, in the first of which will be considered some general principles which must form the basis of a correct chemical history of natural waters. The second part will embrace a series of chemical analyses of mineral waters from the Paleozoic rocks of the Champlain and St. Lawrence basins, together with some river-waters; and the third part will consist chiefly of deductions and generalizations from these analyses.

I.

CONTENTS OF SECTIONS.—1, atmospheric waters; 2, 3, results of vegetable decay; 4–7, action on rocky sediments; 8, action on iron-oxyd; 9, solution of alumina; 10, reduction of sulphates; 11, kaolinization; 12, decay of silicates; 13, origin of carbonate of soda; 14, Bischof's view rejected; 15, 16, porosity of rocks, and their contained saline waters; 17, saliferous strata; 18, action of carbonate of soda on saline waters; 19, origin of sulphate of magnesia; 20, 21, Mitscherlich's view rejected; 22, 23, salts from evaporating sea-water, composition of ancient seas, origin of carbonate of lime; 25–27, origin of gypsum, carbonate of magnesia, and dolomite; 28, waters from oxydized sulphurets; 29, origin of free sulphuric and chlorhydric acids; 30, of sulphhydric and boric acids; 31, of carbonic acid gas; 32, of ammoniacal salts; 33–35, classification of mineral waters.

§ 1. The solvent powers of water are such that this liquid is never met with in nature in a perfectly pure state; even meteoric waters hold in solution, besides nitrogen, oxygen, carbonic acid, ammonia, and nitrous compounds, small quantities of solid matters which were previously suspended in the form of dust in the atmosphere. After falling to the earth, these same waters become still farther impregnated with foreign elements of very variable nature, according to the conditions of the surface on which they fall.

§ 2. Atmospheric waters, coming in contact with decaying vegetable matters at the earth's surface, take from them two classes of soluble ingredients, organic and inorganic. The waters of many streams and rivers are colored brown with dissolved organic matter, and yield, when evaporated to dryness, colored residues which carbonize by heat. This organic substance, in some cases at least, is azotized, and similar if not identical in composition and properties with the apocrenic acid of Berzelius. The decaying vegetation, at the same time that it yields a portion of its organic matter in a soluble form, parts with the mineral or cinereal elements which it had removed from the soil during life. The salts of potassium, calcium, and magnesium, the silica and phosphates which are so essential to the growing plant, are liberated during the process of decay, and hence we find these elements almost wanting in peat and coal. See, on this point, the analyses by Vohl of peat, peat-moss, and the soluble matters set free during its decay. *Ann. der Chem. und Pharm.*, cix, 185, cited in *Rép. Chim. Appliquée*, i, 289. Also Liebig, analysis of bog-water; *Letters on Modern Agriculture*, p. 44; and, in the second part of this paper, the analysis of the waters of the Ottawa river.

§ 3. At the same time, an important change is effected in the gaseous contents of the atmospheric waters. The oxygen which they hold in solution is absorbed by the decaying organic matter, and replaced by carbonic acid, while any nitrates or nitrites which may be present are by the same means reduced to the state of ammonia (Kuhlmann). By thus losing oxygen, and taking up a readily oxydizable organic matter, these waters become reducing instead of oxydizing media in their farther progress.

§ 4. We have thus far considered the precipitated atmospheric waters as remaining at the earth's surface, but a great portion of them sooner or later in their course come upon permeable strata by which they are absorbed, and in their subterranean circulation undergo important changes. The effect of ordinary argillaceous strata destitute of neutral soluble salts may be first examined. Between such sedimentary strata and the waters charged with organic and mineral matters from decaying vegetation, there are important reactions. The composition of these waters is peculiar. They contain, relatively to the sodium, a large amount of potassium salts, besides notable quantities of silica and phosphates, in addition to the dissolved organic matters and the earthy carbonates, and in some cases ammoniacal salts and nitrates or nitrites. The sulphuric acid and chlorine are moreover not sufficient to neutralize the alkalies, which are perhaps in part combined with silica or with an organic acid.*

§ 5. The experiments of Way, Voelcker, and others have shown that when such waters are brought into contact with argillaceous sediments, they part with their potash, ammonia, silica, and phosphoric acid and organic matter, which remain in combination with the soil; while, under ordinary conditions at least, neither soda, lime, magnesia, sulphuric acid nor chlorine are retained. This power of the soil appears from the experiments of Eichhorn to be in part due to the action of hydrated double aluminous silicates; and the process is one of double exchange, an equivalent of lime or soda being given up for the potash and ammonia retained. The phosphates are probably retained in combination with alumina or peroxyd of iron; and the silica and organic matters also enter into insoluble combinations. It follows from these reactions that the surface-waters charged with the products of vegetable decay, after having been brought in contact with argillaceous sediments, retain little else than sulphates, chlorids, or carbonates of soda, lime and magnesia. In this way the mineral matters required for the growth of plants, and by them removed from the soil, are again restored to it; and from this reaction results the small proportion of potash salts in the waters of ordinary springs and wells as compared with river-waters. From the waters of rivers, lakes, and seas, aquatic plants again take up the dissolved potash, phosphates, and silica; and the subsequent decay of these plants in contact with the ooze of the bottom, or on the shores, again restores these elements to the earth. See a remarkable essay by Forchhammer, on the composition of fucoids, and their geological relations, *Jour. für Prakt. Chem.*, xxxvi, 388.

§ 6. The observations of Eichhorn upon the reaction between solutions of chlorids and pulverized chabazite, which, as a hydrated silicate of alumina and lime, may perhaps be taken as a representative of the hydrous double silicates in the soil, show that these substitutions of protoxyd bases are neither complete nor absolute. It would appear, on the contrary, that there takes place a partial exchange or a partition of bases according to their respective affinities. Thus the normal chabazite in presence of a solution of chlorid of sodium exchanges a large portion of its lime for soda; but if the resulting soda compound be placed in a solution of chlorid of calcium, an inverse substitution takes place, and a portion of lime enters again into the silicate, replacing an equivalent of soda; while, by the action of a solution of chlorid of potassium, both lime and soda are, to a large extent, replaced by potash. In like manner, chabazite in which by the action of a solution of sal-ammoniac, a part of the lime has been replaced by ammonia, will give up a portion of the ammonia, not only to solutions of chlorids of potassium and sodium, but even to chlorid of calcium. It results from these mutual decompositions, that there is a point where a chabazite containing both lime and soda, or lime and ammonia, would remain unchanged in mixed solutions of the corresponding chlorids, the affinities of the rival bases being balanced.[1] Inasmuch, however, as the proportions of ammonia and potash in natural waters are usually small, when compared with the amounts of lime and soda existing in the form of hydro-silicates in the soil, the result of these affinities is an almost complete elimination of the ammonia and potash from infiltrating waters.

§ 7. That the replacement of one base by another in this way is not complete is shown, moreover, by the experiments of Liebig, Dehérain and others, who have observed that a solution of gypsum removes from soils a certain amount of potash-salt, which was insoluble in pure water. In this way gypseous waters may also acquire portions of sulphate of soda, and perhaps of sulphate of magnesia, from silicates.

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[1] This Journal, [2] xxviii, 72.

It is not certain that all the above reactions observed for chabazite are applicable without modification to the double hydro-aluminous silicates of sedimentary strata. Were such the case, important changes might, in certain conditions, be effected in the composition of saline waters. Thus, in presence of a great amount of a hydrous silicate of lime and alumina, solutions of chlorid of sodium might acquire a considerable amount of chlorid of calcium; but it is probable that these reactions, however important they may be in relation to the soil, and to surface-waters with their feeble saline impregnation, have at present but little influence on the composition of the stronger saline waters. It is, however, not impossible that the action of the ancient sea-waters, holding a large amount of chlorid of calcium, upon the hydrated and half-decomposed feldspars which constituted the clays of the period, may have given rise to those double silicates which formed the lime-soda feldspars so abundant in the Labrador series.

§ 8. The reactions just described assume an importance in the case of waters impregnated with soluble matters from vegetable decay, and in this event another and not less important class of phenomena intervenes, which are due to the deoxydizing power of the dissolved organic matter. By the action of this upon the insoluble peroxyd of iron set free from the decomposition of ferruginous minerals and disseminated in the sediments, protoxyd of iron is formed, which is soluble both in carbonic acid, and in the excess of the organic (acid) matter. By this means, not only are great quantities of iron dissolved, but masses of sediments are sometimes entirely deprived of iron-oxyd, and thus beds of white clay and sand are formed. The waters thus charged with proto-salts of iron absorb oxygen when exposed to the air, and then deposit the metal as hydrated peroxyd, which when the organic matter is in excess, carry down a greater or less proportion of it in combination. Such organic matters are rarely absent from limonite, and in some specimens of ochre amount to as much as fifteen per cent.[1] The conditions under which hydrous peroxyd of manganese is often found are very similar to those of hydrous peroxyd of iron, with which it is so frequently associated; and there is little doubt that oxyd of manganese may be dissolved by a process like that just pointed out. A portion of manganese has been observed in the soluble matters from decaying peat-moss; and it seems to be generally present in small quantities with iron in surface-waters.

§ 9. There is reason to believe that alumina is also, under certain conditions, dissolved by waters holding organic acids. The existence of pigotite, a native compound of alumina with an organic acid, and the occasional association of gibbsite with limonite point to such a reaction. That it is not more abundant in solution, is due to the fact that, unlike most other metallic oxyds, alumina, instead of being separated in a free state by the slow decomposition of its silicious compounds, remains in combination with silica. The formation of bauxite, a mixture of hydrate of alumina with variable proportions of hydrous peroxyd of iron, which forms extensive beds in the Tertiary sediments of the great Mediterranean basin, indicates a solution of alumina on a grand scale, and perhaps owes its origin to the decomposition of solutions of native alum by alkaline or earthy carbonates. Emery, a crystalline anhydrous form of alumina, has doubtless been formed in a similar manner. See this Journal, [2] xxxii, 287. The existence in many localities of an insoluble sub-sulphate of alumina, websterite, in layers and concretionary masses in Tertiary clays, evidently points to such a process. Compounds consisting chiefly of hydrated alumina are frequently found in fissures of the chalk in England. On the absence of free hydrated alumina from soils, see Muller, cited in this Journal, [2] xxxv, 292.

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[1] Geology of Canada, p. 512.

§ 10. The organic matter dissolved by the surface-waters serves to reduce to the condition of sulphurets the various soluble sulphates which it takes up at the same time or meets with in its course. These sulphurets, decomposed by carbonic acid, which is in part derived from the atmosphere, and in part from the oxydation of the carbon of the organic matter, give rise to alkaline and earthy carbonates on the one hand, and to sulphuretted hydrogen on the other. In this way, under the influence of a somewhat elevated temperature, are generated sulphurous waters, whether of subterranean springs, or of tropical sea-marshes and lagoons. The reaction between the sulphurets thus formed and the salts or oxyds of iron, copper and similar metals which may be present, gives rise to metallic sulphurets. The decomposition of sulphuretted hydrogen by the oxygen of the air, produces native sulphur, with which are generally found associated sulphates of lime and strontia. By virtue of these reactions, soluble sulphates of lime and magnesia may be completely eliminated from waters, the bases as insoluble carbonates, and the sulphur as sulphuretted hydrogen, free sulphur, or a metallic sulphuret. Moreover, as Forchhammer has pointed out in the paper already cited, sulphuret of potassium in the presence of ferruginous clays is also completely separated from solution, the sulphur as sulphuret of iron, and the alkali as a double aluminous silicate.

§ 11. We have thus far considered the composition of surface-waters as modified by the decay of vegetation, or by the reactions between the matters derived from this source and the permeated sediments. Not less important, however, than the elements thus removed by substitution from sedimentary strata are those which are liberated by the slow decomposition of the minerals composing these sediments.

It has long been known that in the transformation of a feldspar into kaolin the double silicate of alumina and alkali takes up a portion of water, and is resolved into a hydrous silicate of alumina; while the alkali, together with a definite portion of silica, is separated in a soluble state. The feldspar, an anhydrous double salt formed at an elevated temperature, has a tendency under certain conditions to combine at a lower temperature with a portion of water, and break up into two simpler silicates. Daubrée has moreover shown that when kaolin is exposed to a heat of 400° C. in presence of a soluble silicate of potash the two silicates unite and regenerate feldspar. These reactions are completely analogous to those presented by very many other double salts, ethers, amids, and similar compounds. The preliminary conditions of this conversion of feldspar into kaolin and a soluble alkaline silicate, however, still require investigation. It is known that while some feldspathic rocks appear almost unalterable, others containing the same species of feldspar are found converted into kaolin to a depth of many feet from the surface. This chemical alteration, according to Fournet, is always preceded by a mechanical change of the feldspar, which first becomes opaque and friable, and is thus rendered permeable to water. He conceives this alteration to be molecular and to be connected with the passage of the silicate into a dimorphous or allotropic condition.[3]

§ 12. The researches of Ebelman in the alterations of various rocks and minerals have thrown considerable light on the relations of sediments and natural waters.[4] From the analyses of basaltic and similar rocks, which include silicates of lime, magnesia, iron and manganese in the forms of pyroxene, hornblende and olivine, and which undergo a slow and superficial decomposition under atmospheric influences, it appears that during the process of decay the greater part of the lime and magnesia is removed, together with a large proportion of silica. It was found moreover that in the case of a rock apparently composed of labradorite and pyroxene, the removal of the lime and magnesia from the decomposed portion was much more complete than that of the alkalies; showing thus the comparatively greater stability of the feldspathic element. The decomposition of the feldspar in these mixed rocks is, however, at length effected, and the final result approximates to a hydrous silicate of alumina, or clay. This slow decomposition of silicates of protoxyd-bases appears to be due to the action of carbonic acid, which, removing the lime and magnesia as carbonates, liberates the silica in a soluble form; while the iron and manganese, passing to a state of higher oxydation, remain behind, unless the action of organic matters intervenes to give them solubility.

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[3] Ann. de Chimie, [2], lv, 225.

[4] Ebelman, Recueil des Travaux, ii, 1–79.

§ 13. It is to be remarked that, apart from the peculiar and complete decomposition resulting in the production of kaolin, to which orthoclase, oligoclase, and some other feldspathides, as leucite, beryl, and perhaps also the scapolites and albite are occasionally subject, orthoclase is less liable to change than the soda-feldspars, albite, oligoclase and labradorite. Weathered surfaces of these become covered with a thin, soft, white and opaque crust, from decomposition, while the surfaces of orthoclase under similar conditions still preserve their hardness and translucency. The decomposition of feldspathides, and other aluminous double silicates, whether rapid and complete, or slow and partial, apparently yields the same results. A gradual process of this kind is constantly going on in the feldspathic matters which form a large proportion of the mechanical sediments of all formations; and in deeply buried strata is not improbably accelerated by the elevation of temperature. The soluble alkaline silicate resulting from this process is in most cases decomposed by carbonates of lime and magnesia in the sediments, giving rise to silicates of these bases, (which are for the greater part separated in an insoluble state,) and to carbonate of soda. Only in rare cases does potash appear in large proportion among the soluble salts thus liberated from sediments, partly because soda-feldspars are more subject to change, and partly from the fact that potash salts would be separated from the percolating waters in virtue of the reactions mentioned in § 5. Hence it happens that apart from the neutral soda salts of extraneous origin, waters permeating sediments containing alkaliferous silicates generally bring to the surface little more than soda combined with carbonic and sometimes with boric acid, and carbonates of lime and magnesia with small portions of silica.

§ 14. This explanation of the decomposition of alkaliferous silicates and of the origin of carbonate of soda is opposed to the view of Bischof, who conceives that carbonic acid is the chief agent in decomposing feldspathic minerals.[a] The solvent action of waters charged with carbonic acid is undoubted, as shown by various experimenters, especially by the Messrs. Rogers;[b] but this acid is not always present in the quantities required. The proportion of it in atmospheric waters is so inadequate that it becomes necessary to suppose some subterranean source of the gas, which is by no means a constant accompaniment of natron springs. A copious evolution of carbonic acid is observed in the vicinity of the lake of Laach, where the alkaline waters studied by Bischof occur.[7] The same thing is met with in many other localities of such springs, among which may be mentioned the region around Saratoga, where saline waters containing carbonate of soda, and highly charged with carbonic acid, rise in abundance from the Lower Silurian strata; but farther northward, along the valleys of Lake Champlain and the St. Lawrence, similar alkaline saline waters, which abound in the continuation of the same geological formations, are not at all acidulous. From this the conclusion seems justifiable that the production of carbonate of soda is a process, in some cases at least, independent of the presence of free carbonic acid. In this connection, it is well to recall the solvent power of pure water on alkaliferous silicates, as shown more especially by Bunsen, and also by Damour, who found that distilled water at temperatures much below 212° takes up from silicates like palagonite and calcined mesotype, comparatively large amounts both of silica and alkalies. (Damour, *Ann. Chem. et Phys.*, [3], xix, 481.)

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[a] Bischof, Chem. Geol., ii, 181.

[b] This Journal, [2], v, 401.

§ 15 Another and an important source of mineral impregnation to waters exists in the soluble salts enclosed in sedimentary strata, both in the solid state and in aqueous solution, and for the most part of marine origin. In order to form some conception of the amount of saline matters which may be contained in a dissolved state in the rocky strata of the earth, we have made numerous experiments to determine the porosity of various rocks; some few of the results of which may here be noticed. Fragments of the rocks were dried at a heat of 150° to 200° F., in a current of dry air until they ceased to lose weight. They were then soaked in distilled water, and kept under it for many hours beneath an exhausted receiver. When thus saturated they were wiped from adhering water, and weighed; first in air to determine the augmentation of weight from absorption, and secondly, in water to give, by the loss in weight, the volume of the specimens. These data furnish the means of determining the volume of water absorbed, which is given below for 100.00 parts of different rocks from the Paleozoic strata of the St. Lawrence basin.

Potsdam	formation,	(sandstone)	3	specimens	2·26—2·71
"	"		3	"	6·94—9 85
Calciferous	"	(cryst. dolomite)	4	"	1·89—2·53
"	"	" "	2	"	5·90—7·22
Chazy	"	(argill. limestone)	4	"	6·45-13·55
Trenton	"	(gray crys. ")	4	"	1·18—1·70
"	"	(black impalp. ")	2	"	0·30—0.32
Utica	"	(black shale)	3	"	0·75—2·10
Hudson River	"	(arenaceous shale)			—7·94
Medina	"	(argill. sandstone)	2	specimens	8·37-10·06
Guelph	"	(cryst. dolomite)	3	"	9·34-10·60
Niagara	"	(impalpable ")	2	"	9·69-10 92

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[7] Bischof, Lehrbuch, i, 357-363.

The above data might be much more extended, but sufficient have been given to show the porosity of the principal Paleozoic rocks of the basin.[*]

§ 16. If we take from the Potsdam sandstone the mean of the first three trials, giving 2·5 per cent for the volume of water which it is capable of holding in its pores, we find that a thickness of 100 feet of it would contain in every square mile, in round numbers, 70,000,000 cubic feet of water; an amount which would supply a cubic foot (over seven gallons) a minute for more than thirteen years. The observed thickness of the Potsdam sandstone in the district of Montreal, varies from 200 to 700 feet, and the mean of 500 feet may be taken. To this are to be added 800 feet for the Calciferous formation, whose capacity for water may be taken, like the Potsdam sandstone, at 2·5 per cent. We have thus in each square mile of these formations, wherever they lie below the water-level, a volume of 490,000,000 cubic feet of water, equal to a supply of a cubic foot per minute for 106 years. The capacity of the 800 feet of Chazy and Trenton limestones which succeed these lower formations, may be fairly taken at one half that of those just named. But it is unnecessary to multiply such calculations—enough has been said to show that these sedimentary strata include in their pores great quantities of water, which was originally that of the ocean of the Paleozoic age. These strata throughout the great Silurian basin of the St. Lawrence, are now for the greater part beneath the sea-level; nor is there any good reason for supposing them to have ever been elevated much above their present horizon. Wells and borings sunk in various places in these rocks show them to be still filled with bitter saline waters; but in regions where these rocks are inclined and dislocated, surface-waters gradually replace these saline waters, which in a mixed and diluted state appear as mineral springs. These saline solutions, other things being equal, will be better preserved in limestones or argillaceous rocks than in the more porous and permeable sandstones.

§ 17. But besides the saline matters thus disseminated in a dissolved state in ordinary sedimentary rocks, there are great volumes of saliferous strata, properly so-called, charged with the results of the evaporation of ancient sea-basins. These strata enclose not only gypsum and rock-salt, but in some regions large quantities of the double chlorid of potassium and magnesium, carnallite; and in others sulphate of soda, sulphate of magnesia, and complex sulphates like blödite and polyhallite. Besides these crystalline salts, the mother-liquors containing the more soluble and uncrystallizable compounds, may also be supposed to impregnate, in some cases, the sediments of these saliferous formations. The conditions under which these various salts are deposited from sea-water, and their relations to the composition of the ocean in earlier geological periods, are reserved for consideration in § 22. Infiltrating waters remove from these saliferous strata their soluble ingredients; which, together with the ancient sea-waters of other sedimentary rocks, give rise to the various neutral saline waters; while the mingling of these in various proportions with the alkaline waters whose origin has been described in § 18, produces intermediate classes of waters of much interest.

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[*] A great many similar determinations will be found in a Report on Building Stones to the British House of Commons in 1839, by Barry, Dela Beche and Smith. See also Delesse. Bull. Soc. Geol., [2], xix, 64.

§ 18. I have elsewhere described the results of a series of experiments on the mutual action of the waters of these two classes.[9] When a dilute solution of bicarbonate of soda is gradually added to a solution which, like sea-water, contains besides chlorid of sodium, the chlorids and sulphates of calcium and magnesium, the greater part of the lime separates as carbonate, carrying down with it only from one to three-hundredths of carbonate of magnesia; a portion of lime however remaining in solution as bicarbonate. When the chlorid of calcium is wholly decomposed, the magnesian salt is attacked in its turn, and there finally results a solution in which the whole of the earthly chlorids are replaced by chlorid of sodium. A farther addition of the solution of carbonate of soda gives them the character of alkaline-saline waters; which moreover contain abundance of earthy carbonates.

The substitution of neutral carbonate for bicarbonate of soda in the above experiment does not affect the result, except in causing a somewhat larger proportion of magnesia to be thrown down with the carbonate of lime. The resulting liquid still retains large quantities of earthy carbonates in solution.[10]

§ 19. In the saline waters just considered, chlorids generally predominate, the sulphates being small in amount, and often altogether wanting. Some exceptions to this are however met with; for apart from waters impregnated with gypsum, whose origin is readily understood, there are others in which sulphate of soda or sulphate of magnesia enter largely. The soda-salt may sometimes be formed by the reaction between solution of gypsum and natriferous silicates referred to in § 7; or by the decomposition of gypsum by a solution of carbonate of soda; while in other cases its origin will probably be found in the natural deposits of sulphates, such as glauberite, thenardite, and glauber-salt, which occur in saliferous rocks; a similar origin is probable for many of those springs in which sulphate of magnesia predominates. This salt also effloresces abundantly in a nearly pure form upon certain limestones, and is in some cases due to the action of sulphates from decomposing pyrites upon magnesian carbonate or silicate. In by far the greater number of cases, however, its appearance is unconnected with any such process; and is, according to Mitscherlich, due to a reaction between dolomite and dissolved gypsum.

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[9] This Journal, [2], xxviii, 170.

[10] Geol. Survey of Canada, Report, 1853-56, p. 468.

§ 20. In support of this view, it was found by the chemist just named that when a solution of sulphate of lime was made to filter for some time through pulverized magnesian limestone, it was decomposed with the formation of carbonate of lime and sulphate of magnesia. This reaction I have been unable to verify. A solution of gypsum in distilled water was made to percolate slowly through a column of several inches of finely-powered dolomite; and after ten filtrations, occupying as many days, no perceptible amount of sulphate of magnesia had been formed. Solutions of gypsum were then digested for many months with pulverized dolomite, and also with crystalline carbonate of magnesia, but with similar negative results; nor did the substitution of a solution of chlorid of calcium lead to the formation of any soluble magnesian salt. Solutions of gypsum were then impregnated with carbonic acid, and allowed to remain in contact with pulverized dolomite and with magnesite as before, during six months of the warm season, when only inappreciable traces of magnesia were taken into solution. These experiments show that no decomposition of dissolved gypsum is effected by native carbonate of magnesia or by the double carbonate of lime and magnesia at ordinary temperatures.

§ 21. I find however that hydrated carbonate of magnesia readily completely decomposes a solution of gypsum when agitated with it, with formation of carbonate of lime and sulphate of magnesia, and the same result is produced with the native hydrate of magnesia when mingled with a solution of gypsum in presence of carbonic acid. Now there may be dolomites which contain an admixture of hydro-carbonate of magnesia, as there certainly are others which like predazzite, are penetrated with hydrate of magnesia. The reaction between solutions of gypsum and such magnesian limestones, (with the intervention in the case of predazzite, of atmospheric carbonic acid), would suffice to explain the results obtained by Mitscherlich, and the appearance in certain cases of sulphate of magnesia as an efflorescence on dolomites. In the experiments above described, the nearly pure crystalline dolomites from the Guelph and Niagara formations were made use of.

§ 22. When sea-water is exposed to spontaneous evaporation, the lime which it contains separates in the form of sulphate, gypsum being but sparingly soluble in a concentrated brine, and the greater portion of the chlorid of sodium crystallizes out in a nearly pure state. The mother-liquor of specific gravity 1·24, having lost about four-fifths of its chlorid of sodium, still contains dissolved a large proportion of sulphate of magnesia. If the evaporation is continued at the ordinary temperature, till a density of 1·32 is attained, about one-half of the magnesian sulphate separates, mixed with common salt; and by reducing the temperature to 6° C, a large portion of pure sulphate of magnesia now crystallizes out. The further evaporation of the remaining liquor by the heat of summer causes the potassium salts to separate in the form of a hydrous double chlorid of potassium and magnesium, an artificial carnallite.[11]

By varying somewhat the conditions of temperature, the sulphate of magnesia and chlorid of sodium of the mother-liquor undergo mutual decomposition, with the production of sulphate of soda and chlorid of magnesium. Hydrated sulphate of soda crystallized out from such a mixed solution at 0° C., and by reducing the temperature to −18° C. the greater part of the sulphates may be separated in this form from the mother-liquor of 1·24, previously diluted with one-tenth of water; without which addition a mixture of hydrated chlorid of sodium would separate at the same time. If, on the other hand, the temperature of the mixed solution be raised above 50° C., the sulphate of soda crystallizes out in the anhydrous form, as thenardite. By the spontaneous evaporation during the heats of summer of the mother-liquors of density 1·35, a double sulphate of potassium and magnesium separates. These reactions are taken advantage of on a great scale in Balard's process, as modified by Merle,[12] for extracting salts from sea-water.

§ 23. The results of the evaporation of sea-water would however be widely different if an excess of lime-salts were present. In this case the whole of the sulphates present would be deposited in the form of gypsum at an early stage of the evaporation, and the mother-liquor after the separation of the greater part of the common salt, would contain little else than the chlorids of sodium, potassium, calcium, and magnesium.

§ 24. A consideration of the conditions of the ocean in earlier geological periods will show that it must have contained a much larger quantity of lime-salts than at present. The alkaline carbonates, whose origin has been described in § 13, and which from the earliest times have been flowing into the sea, have gradually modified the composition of its waters, separating the lime as carbonate, and thus replacing the chlorid of calcium by chlorid of sodium, as I have long since pointed out.[13] This reaction has doubtless been the source of all the carbonate of lime in the earth's crust, if we except that derived from the decomposition of calcareous silicates. (§ 12). In this decomposition by carbonate of soda, as already described in § 18, it results from the incompatibility of chlorid of calcium with hydrous carbonate of magnesia, that the lime is first precipitated with a little adhering carbonate of magnesia, and it is only when the chlorid of calcium is all decomposed that the magnesian chlorid is transformed into carbonate of magnesia. This latter reaction can consequently take place only in limited basins, or in portions cut off from the oceanic circulation.

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[11] The hydrous double chlorid of potassium and magnesium (carnallite of H. Rose), occurs in large quantities in a stratum of clay overlying a great bed of rock-salt 100 feet thick, at Stassfurth in Prussia. It is associated with considerable quantities of sulphate of magnesia. According to Clemm this sulphate of magnesia, to which the name of *kieserite* has been given, and which occurs also in Anhalt, contains but one equivalent of water, (MgO,SO_3+HO). It is not more soluble than gypsum, and unlike the ordinary sulphate of magnesia, loses the whole of its acid at a red heat in a current of steam, the acid passing off undecomposed. This salt is found in such large quantities as to be of economic importance. (Bull. Soc. Chim. de Paris, 1864, p. 297.)

[12] See my paper in this Journal, [2], xxv, 361; also Report of Juries of the Exhibition of 1862, class II, p. 48.

§ 25. It follows from what has been said that the lime-salt may be eliminated from sea-water either as sulphate or as carbonate. In the latter case no concentration is required; while in the former the conditions are two,—a sufficient proportion of sulphates to convert the whole of the lime into gypsum, and such a degree of concentration of the water as to render this insoluble. These conditions meet in the evaporation of modern sea-water; but the evaporated sea water of earlier periods, with its great predominance of lime-salts, would still contain large amounts of chlorid of calcium, the insolubility of gypsum in this case serving to eliminate all the sulphates from the mother-liquor. Evaporation alone would not suffice to remove the whole of the lime-salts from waters in which the calcium present was more than equivalent to the sulphuric acid; but the intervention of carbonate of soda would be required.

§ 26. In concentrated and evaporating waters freed from lime-salts by either of the reactions just mentioned, but still holding sulphate of magnesia, another process, which I have elsewhere described, may intervene.[14] The addition of a solution of bicarbonate of lime to such a solution gives rise, by double decomposition, to sulphate of lime and bicarbonate of magnesia. The former being much the less soluble salt, especially in a strongly saline liquid, is deposited as gypsum; and subsequently the magnesian carbonate is precipitated in a hydrous form. The effect of this reaction is to eliminate from the sea-water both the sulphuric acid and the magnesia, without the permanent addition to it of any foreign element.

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[13] Canadian Journal for 1858, p. 202; this Journal, [2], xxv, 102, and *Comptes Rendus*, June 9, 1862, p. 1191.

[14] This Journal, [2], xxviii, 174.

§ 27. Gypsum may thus be separated from sea-water by two distinct processes,—the one a reaction between sulphate of magnesia and chlorid of calcium, and the other between the same sulphate and carbonate of lime. The latter, involving a separation of bicarbonate of magnesia, can as we have seen, only take place when the whole of the chlorid of calcium has been eliminated; and if we suppose the ancient ocean, unlike the present, to have contained more than an equivalent of lime for each equivalent of sulphuric acid, it is evident that a lake or basin of sea-water free from lime salts could only have been produced by the intervention of carbonate of soda. The action of this must have eliminated the whole of the lime as carbonate, or at least have so far reduced the amount of this base that the sulphates present would be sufficient to separate the remainder by evaporation, in the form of gypsum, and still leave in the mother-liquor a quantity of sulphate of magnesia for reaction with bicarbonate of lime.

The source of the magnesian carbonate, whose union, under certain conditions, with the carbonate of lime gives rise to the dolomite,[18] may thus be due either to the reaction just described between bicarbonate of lime and solutions holding sulphate of magnesia, or to the direct action of carbonate of soda upon waters containing magnesian salts; but in either case the previous elimination of the incompatible chlorid of calcium must be considered an indispensable preliminary to the production of the magnesian carbonate.

§ 28. To the three principal sources of mineral matters in mineral waters already enumerated, viz., decaying organic matters, decomposing silicates, and the soluble saline matters in rocks, a few other minor ones must be added. One of these is the oxydation of metallic sulphurets, chiefly iron-pyrites, giving rise to sulphate of iron, and more rarely to sulphates of copper, zinc, cobalt and nickel; and by secondary reactions to sulphates of alumina, lime, magnesia, and alkalies. This process of oxydation is necessarily superficial and local, but the soluble sulphates thus formed have probably played a not unimportant part. (§ 9.)

§ 29. Besides these last, which contain chiefly neutral and acid salts, there is another class of waters characterized by the presence of free sulphuric or chlorhydric acid, or both together. These acid waters sometimes occur as products of volcanic action, during which both chlorhydric acid and sulphur are often evolved in large quantities. This latter element generally comes to the surface as sulphuretted hydrogen, which by the oxydation of the hydrogen may deposit its sulphur in craters and fissures. In other cases, as shown by Dumas, the sulphur and hydrogen may be slowly and simultaneously oxydized at a low temperature, giving rise directly to sulphuric acid. Not less frequent, however, is probably the direct conversion, by combustion of the sulphuretted hydrogen into water and sulphurous acid, which afterward absorbing oxygen from the air is converted into sulphuric acid.

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[18] This Journal, [2], xxviii, 180-186, and further, Geol. Survey of Canada, Report for 1859, 214-218.

§ 30. The source of the hydrochloric acid and the sulphur of volcanos is probably the decomposition of chlorids and sulphates at high temperatures. It is known that for the decomposition of earthy chlorids, water and an elevated temperature are sufficient, and at a higher temperature, chlorid of sodium is readily decomposed in presence of siliceous and aluminous minerals, with the intervention of water. Another agency which probably comes into play in volcanic phenomena is that of organic matters, which, reducing the sulphates to sulphurets, enable the sulphur to be subsequently disengaged as sulphuretted hydrogen by the operation of water, either with or without the intervention of carbonic acid or of siliceous and argillaceous matters. Even in cases where this reducing action is excluded, the ignition of sulphates in contact with earthy matters must liberate the sulphuric acid as a mixture of sulphurous acid and oxygen; and these uniting in their distillation upward through the strata, may give rise to springs of sulphuric acid.[16] The reactions similar to those just noticed, involving borates like stassfurthite and hayesine, or boric silicates like tourmaline, etc., are to be ascribed the large amounts of boric acid which are sublimed in some volcanos, or volatilized with the watery vapor of the Tuscan *suffioni*.

§ 31. The action of subterranean heat upon buried strata containing sulphates and chlorids is then sufficient to explain the appearance of hydrochloric and sulphurous acids and sulphur, even without the intervention of organic matters, which are, however, seldom or never wanting; whether as coal, lignite, bitumen, and pyroschists, or in a more divided condition. The presence of hydrogen and of marsh-gas, as observed by Deville among volcanic products, is an evidence of this. The generation of marsh-gas, is, however, in most cases clearly unconnected with volcanic action or subterranean heat.

To the decomposition of carbonates in buried strata by siliceous matters, with the aid of heat, is to be ascribed the great amounts of carbonic acid gas which are in many places evolved from the earth, and impregnating the infiltrating waters, give rise to acidulous springs. The principal sources of this gas in Europe are in regions adjoining volcanos, either active or recently extinct;

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[16] See the note to § 22, on kieserite.

but their occurrence in the Paleozoic strata of the United States, far remote from any evidence of volcanic phenomena other than slightly thermal springs, shows that an action too gentle or too deeply-seated to manifest itself in igneous eruptions, may evolve carbonic acid abundantly. The sulphuric-acid springs of Western New York and Canada, to be described further on, are not less remarkable illustrations of the same fact.

§ 32. The frequent presence of ammoniacal salts in volcanic exhalations is here worthy of notice, especially when considered in connection with the rarity of nitric and ammoniacal compounds in natural waters, except in some local conditions, as in the wells of cities, etc., where they are sometimes observed in comparatively large amounts. The explanation of this is evident: for although nitrates themselves are not directly removed from the water, they are by the reducing action of organic matters converted into ammonia, which is retained by the soil. In consequence of this affinity, the argillaceous strata, whether of the present period or of older formations, hold in a very fixed form a considerable quantity of nitrogen. This, from the slowness with which it is eliminated in the form of ammonia under the influence of alkaline solutions, probably exists as an ammoniacal silicate, (§ 6). The action of acids, however, as well as alkalies may be supposed to liberate it from its combination, and thus generate the ammoniacal salts which are such frequent accompaniments of volcanic phenomena. The numerous experiments of Delesse show that ammonia, or at least nitrogen capable of being evolved by heat and alkalies in the form of ammonia, is present in the limestones, marls, argillites, and sandstones of former geological periods, in quantities scarcely inferior to those in similar deposits of modern times, amounting for most of the ancient sedimentary strata to from one to five thousands of nitrogen;[17] from which it will be seen that the amount of this element thus retained in the rocky strata of the earth's crust is very great.[18]

§ 33. If we attempt a chemical classification of natural waters in accordance with the principles laid down in the preceding sections they may be considered under the following heads:

A. Atmospheric waters.
B. Waters impregnated with the soluble products of vegetable decay.
C. Waters impregnated with the salts from decomposing feldspathic rocks, and holding a portion of carbonate of soda as a characteristic ingredient.
D. Waters holding neutral salts of sodium, calcium or magnesium from strata where they existed as solid salts, or as impregnating brines.

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[17] Ann. des Mines, [5], xviii, 151-523.

[18] For an exposition of the views put forward in the four preceding sections, see my paper in the Canadian Journal for 1858, p. 206.

E. Waters holding chiefly sulphates from decomposing pyrites; copperas and alum waters.

F. Waters holding free sulphuric or chlorhydric acid.

§ 34. The name of mineral waters is popularly applied only to such as contain sufficient foreign matters to give them a decided taste; and hence the waters of the divisions A and B, and many of the feebler ones of C and D, are excluded. Those of E and F have peculiar local sources; but those of C and D are often associated in adjacent geological formations, and their commingling in various proportions gives rise to mineral waters intermediate in composition. In accordance of these considerations, a classification of mineral waters for technical purposes was adopted by me in *The Geology of Canada*, p. 531, including only those of C, D, and F, which were arranged in six classes.

I. Saline waters containing chlorid of sodium, often with large portions of chlorids of calcium and magnesium, with or without sulphates. The carbonates of lime and magnesia are either wanting, or present only in small quantities. These waters are generally bitter to the taste, and may be designated as brines or bitterns.

II. Saline waters which differ from the last in containing besides the chlorids just mentioned, considerable quantities of carbonates of lime and magnesia. These waters generally contain much smaller proportions of earthy chlorids than the first class, and are hence less bitter to the taste.

III. Saline waters which contain, besides chlorid of sodium and the carbonates of lime and magnesia, a portion of carbonate of soda.

IV. Waters which differ from the last in containing but a small proportion of chlorid of sodium, and in which the carbonate of soda predominates. The waters of this class generally contain much less solid matter than the three previous classes, and have not a very marked taste until evaporated to a small volume, when they will be found, like the last, to be strongly alkaline.

Of these four classes, I corresponds to the division D, and IV to C, while II and III are regarded as resulting from the admixture of these in varying proportions. Sulphates are sometimes present in these waters, but never predominate; in their absence, salts of barium and strontium are often met with. The chlorids are generally, if not always, associated with bromids and iodids. Small quantities of potassium salts are also present, while borates, phosphates, silicates, and small portions of iron, manganese and alumina are generally present. These various waters are occasionally sulphurous, and those of the last three classes may be impregnated with carbonic acid.

V. The fifth class includes acid waters remarkable for containing a large proportion of free sulphuric acid, with sulphates of lime, magnesia, portions of iron, and alumina. These waters, which are characterized by their sour and styptic taste, generally contain some sulphuretted hydrogen.

VI. The sixth class includes some neutral saline waters, in which the sulphates of lime, magnesia, and the alkalies predominate, chlorids being present only in small quantities. These waters, like the last, are often impregnated with sulphuretted hydrogen.

The above classification, although adopted originally for the convenient description of the mineral waters of Canada, will, it is thought, be found to embrace all known classes of natural waters, with the exception of those included under E, and some waters from volcanic sources holding muriatic acid. These may constitute two additional classes. In the first three of the classes above described, chlorids predominate; in the fourth, carbonates; and in the fifth and sixth, sulphates. The waters of the first, second, and sixth classes are neutral; those of the third and fourth, alkaline; and those of the fifth acid.

The results of the chemical analysis of various waters of these classes it is proposed to give in the second part of this paper.

ART. XXIII.—*Abstract of a Memoir on Shooting Stars;* by H. A. NEWTON.

(Read before the National Academy of Sciences, August, 6th, 1864.)[1]

1. THE periodical shooting stars, particularly those of August and November, have hitherto very naturally attracted more attention than the sporadic ones, those seen on every clear night throughout the year. Yet these latter are objects of no small interest. There are methods of observing, and of computation, by which much can be learned about them, and observations already made are of use in determining something of their numbers, and of their place in the Solar System. I propose to combine these existing materials, and see to what they lead us. If it shall appear that I use rude processes and inexact data, and reject terms of considerable importance, I must plead that it is a step forward to do anything in this direction, and express the hope that better data will soon warrant the use of more refined processes. It will be necessary to assume certain propositions which are not probably strictly true, and others which may not be universally conceded. Such assumptions are however very distinctly set forth, that future observations may correct them if erroneous, and verify them if true. The details of many of the arguments are necessarily omitted in this abstract.

The table of altitudes of the paths of shooting stars published in the last volume of this Journal, p. 135, is the basis of the computations. Singly, these altitudes are liable to large probable errors; yet, taken together and used with caution, it is believed that they may be safely employed in a first approximation.

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[1] From the Proceedings of the Academy: published by permission.

2. *Distribution vertically of the middle points of the luminous portions of the meteor-paths.*—From the table named, by rejecting the manifestly unreliable cases, and giving weight to the others, the following numbers are obtained to express the relative numbers of the shooting stars at different altitudes above the earth's surface. By altitude is meant the altitude of the middle point of the visible part of the path expressed in kilometers.

Between	**0**	**and**	**30 kil.**	**39**	**sh. stars,**	**Between**	**150**	**and**	**180 kil.**	**57**	**sh. stars.**
"	30	"	60	114	"	"	180	"	210	20	"
"	60	"	90	243	"	"	210	"	240	20	"
"	90	"	120	277	"	"	240	"	270	8	"
"	120	"	150	106	"	Above	270 kil.			12	"

There is evidently a definite upper limit to the region of meteor-paths. Again, the best observations give few if any very low altitudes. The altitudes less than 30 kilometers, and those greater than 180, are therefore disregarded.

I assume that these observed paths are fair examples as to altitude of all visible paths, and hence that the frequency of the middle points of all visible paths at different altitudes above the earth's surface is proportional to, and may be expressed by, these numbers, viz:

From	**30**	**kilometers**	**to**	**60**	**by**	**114**
"	60	"	"	90	"	243
"	90	"	"	120	"	277
"	120	"	"	150	"	106
"	150	"	"	180	"	57

Representing these numbers by ϱ, and the mean altitude of the paths by d, we have approximately,

$$d = \frac{\Sigma \varrho x}{\Sigma \varrho},$$

where in the finite summation indicated by Σ the successive values of ϱ are to be taken, and x is to be successively $\frac{1}{2}(30+60)$, $\frac{1}{2}(60+90)$, $\frac{1}{2}(90+120)$, &c. The value of d, that is, the mean altitude of meteor paths above the earth's surface, is thus found to be 95·55 kilometers, or not quite 60 miles.

3. *Distribution of the paths of Shooting Stars over the apparent heavens in azimuth.*—Theoretical considerations seem to require that the meteors should be nearly uniformly distributed in azimuth. Observations confirm this. Collating the observations of Mr. Herrick and others, as published in this Journal, and preserving those only in which the heavens were divided into four parts, we have of 6,598 observed paths, 800 in N., 733 in N.E., 965 in E., 852 in S.E., 847 in S., 833 in S.W., 889 in W., and 679 in N.W. These numbers imply a small predominance toward the southeast. Since the average zenith distance of the

middle points of paths is about 48°·3, the above numbers give as a center of gravity, or center of distribution, a point about 2° from the zenith in the direction of S. 28° E. We therefore conclude that the relative frequency of occurrence of meteor-paths in different parts of the visible heavens is in the main *a function of zenith distance only.*

4. *Distribution of meteor-paths over the sky in altitude.*—The law distribution of the paths in apparent altitude may be obtained directly by observations properly devised for that object. But such observations would have to be continued for a considerable time, and would involve great labor. I have therefore sought for a method of obtaining the approximate law of distribution from observations already made for other purposes.

If the zenith distances of a large number of paths seen by one observer were measured or computed, we should find them affected by his habits of watching. Thus, one who looked habitually to the zenith would see only those near that point, while one looking low down would see few near the zenith. But, combining the observations of a large number of persons, we might hope that many of these individual habitudes would counteract each other, and that the aggregate results would be affected only by common errors. I have therefore taken for this purpose various sets of observations made by about forty different persons. For a part the distances of the middle points of several paths from the zenith for the instant of appearance were computed. For the remainder the place of the zenith was in each case computed, and the zenith distance carefully measured on a very good sixteen-inch globe. The number of paths thus computed or measured was 1,393. Of these, 30 were within 10° of the zenith, 60 were between 10° and 20° from the zenith, 142 between 20° and 30°, &c., as in the second column of the following table:—

Table illustrating the distribution of meteor-paths over the sky.

Zenith distance.	No. of meteors.	Area of sky.	$\frac{\text{No.}}{\text{Area.}}$	Sec$^3\theta$.	$\frac{\text{No.}}{\text{Area sec}^3\theta}$.
° ° 0-10	30	·01519	1975	1·012	1951
10-20	60	·04512	1330	1·110	1198
20-30	142	·07366	1928	1·348	1456
30-40	197	·09999	1970	1·819	1083
40-50	274	·12325	2223	2·828	786
50-60	304	.14279	2129	5·299	402
60-70	245	·15798	1551	13·25	117
70-80	110	·16837	653	57·68	11
80-90	31	·17365	178	1510	0

If the area of the visible hemisphere is unity, the numbers in the third column give the areas of the corresponding zones. The number of paths divided by the areas gives quotients proportional to the numbers of stars at different altitudes to any unit of surface. These form the fourth column. They increase slowly to about 45°, and then rapidly diminish to the horizon.

The number of paths along an oblique line is greater than that along a perpendicular line. They should be very nearly as the cubes of the lengths of the lines, or, disregarding the curvature of the earth, as $\sec^3\theta : 1$. The cubes of the secants of 5°, 15°, 25°, &c., are given in the fifth column, these angles being taken as the mean zenith distances of the zones. In the sixth column are the quotients of the numbers in the fourth column divided by those in the fifth.

Under any reasonable law the numbers in the last column should diminish from the zenith. The deviations from this uniform diminution that appear in the table are due in part to the small number of paths measured. The first number represents a small area, and should perhaps be combined with the second. Upon the whole, there seems to be a deficiency near the zenith, which may be due to the tendency of observers to look habitually towards points from 30° to 60° high.

From inspection of this column, and from considerations detailed in the memoir, 1,800 is thought to be a reasonable number to assume as that to which the series approaches. The product $1{,}800 \times \sec^3 5° \times \cdot01519 = 27\cdot67$, would then give the number of shooting stars out of 1,393 that should be seen within 10° of the zenith. This is equivalent to one in 50·35. That is, *about one in fifty of all shooting stars seen at a place should have the middle points of their apparent paths within* 10° *of the zenith.*

5. *Number of shooting stars that come into the atmosphere each day.*—Shooting stars are seen in all countries, and any differences of number for different countries, thus far detected, may be easily explained by the personal equations of observers, or by differences in the clearness of the atmosphere. It will be assumed that for a given *considerable* period of time the meteors are equally abundant over all parts of the earth's surface.

Their frequency at different altitudes from the surface of the earth varies. If we suppose ρ_1 to be the number of middle points of visible paths that fall in a given period of time into any cubic unit of the space of the region of meteor-paths, we should have ρ_1 a function of the altitude above the earth's surface. Let x represent the altitude, and R the earth's radius. Suppose now an inverted cone, whose vertex is at the eye of the observer, whose axis is a vertical line, and whose semi-vertical angle is 10°. In general, shooting stars which have the middle points of luminous parts of their trajectories in this cone will have the middle of their apparent paths within 10° of the zenith. The number in this cone in the given time will be expressed very nearly by the formula,

$$\int_a^b \pi\varrho_1 \tan^2 10° x^2 dx,$$

where a and b are the values of x for the lower and upper surfaces of the region of meteors.

On the other hand, the total number of shooting stars within the given period over the whole earth will be equal to

$$\int_a^b 4\pi\varrho_1 (\mathrm{R}+x)^2 dx.$$

The whole number visible at one place is 50·35 times the number seen within 10° of the zenith, and therefore 50·35 times the number within the above described cone. Hence if m is the number in a given period visible at one place, and N the number that would be visible (except for daylight, clouds, moon, &c.,) through the whole earth in the same period, we should have,

$$\mathrm{N} = \frac{m}{50{\cdot}35} \cdot \frac{\int_a^b 4\pi\varrho_1 (\mathrm{R}+x)^2 dx}{\int_a^b \pi\varrho_1 \tan^2 10° x^2 dx}.$$

Now ϱ_1 is a function of x, and, assuming as heretofore that the observed altitudes are fair examples of the real altitudes, we shall have $\varrho_1 = k\varrho$, where k is a constant depending on the period of time assumed, the unit of space assumed, and the abundance of meteors. As we have assumed an equable distribution over the earth's surface, this constant may be removed from the integral sign. Again, without great error finite summation may take the place of integration, in which case the equation easily reduces to the form,

$$\mathrm{N} = 2{\cdot}555\, m \left\{ \frac{\Sigma_a^b \varrho x^2 + 2\mathrm{R}\Sigma_a^b \varrho x + \mathrm{R}^2 \Sigma_a^b \varrho}{\Sigma_a^b \varrho x^2} \right\}.$$

Taking x successively 45, 75, 105, 135, and 165 kilometers, and ϱ also 114, 243, 277, 106, and 57, and R equal to 6370, we find

$$\mathrm{N} = 10{,}460\, m,$$

that is, *the number over the whole earth is to be considered as* 10,460 *times the number visible at one place.*

To obtain this result, it was assumed that the shooting stars were uniformly distributed over the earth's surface, and that the conditions of visibility are uniform. If, however, we regard the actual instead of the theoretical case, we find that the numbers seen vary through the hours of the night. The rapid diminution toward the horizon, already shown, indicates the great influence of mists, &c., in the air, in absorbing the light of these bodies. But for this cause, many more could be seen within ten degrees of the horizon than in the whole of the rest of the heavens; whereas of 1393 only 31 were actually seen in this part of the sky. These mists must of course vary in different climates. Hence the numbers visible in different places may reasonably be expected to differ. Let, however, a locality have an atmosphere of mean purity, let it in other respects be one of medium character with respect to the number of visible meteor-paths, and let m be the mean hourly number of shooting stars seen in a clear sky at that place: then we may consider that the whole number that under clear skies could be seen over the earth in one hour would be 10,460 m.

The value of m is of course to be determined by observation. The hourly number varies through the night. The value of m may be found from observations extending through the night, or from those made near midnight.

Mr. E. Bouvard, in the year between Oct., 1840, and Oct., 1841, observed at Paris on every clear moonless night. (*Comptes Rendus*, xiii, 1029). He always watched between 11 and 1 o'clock. During 74 hours and 22 minutes he saw 572 shooting stars. Allowing one-fourth of a minute for recording each meteor (which is the time estimated by him), and we have an average of 8 meteors per hour.

By what factor we must multiply the number seen by one observer to obtain the whole number visible at the place, we have no observations that I know of to determine. It is probable that this multiplier is as large as four, and that thirty is therefore not too large for the mean value of m.

This would give the average number of meteors that traverse the atmosphere daily, and that are large enough to be visible by the naked eye, if the sun, moon and clouds would permit, equal to 30 × 24 × 10,460, or *more than seven and a half millions.*

6. It will now be assumed that the phenomenon called a shooting or falling star is caused by a small body (probably a solid) which originally was moving in its own orbit in the solar system, or in space; that this body coming into the atmosphere of the earth elicits light by the loss of velocity, and is usually itself dissipated before reaching the earth's surface. The term *meteoroid* will be used to designate such a body before it enters the earth's atmosphere.

7. *Number of meteoroids in the space which the earth traverses.*—Suppose many small bodies to be distributed through an indefinite space, so that there shall be n bodies in a cubic unit. Suppose that these bodies have all an uniform velocity of v units per second in the same direction. Suppose a larger sphere whose radius is R, and which is without attraction, to be at rest in this space. The sphere intercepts in each second as many small bodies as are contained in a right cylinder, whose length is v, and whose radius is R, that is $\pi R^2 nv$ bodies.

If the sphere attracts the small bodies, it may be shown that instead of πR^2nv we should have $\pi R^2nv\left(1+\frac{v_0^2}{v^2}\right)$, when v_0 is the velocity of a particle falling by the attraction of the sphere from rest at infinity, to the surface of the sphere. If the sphere has an uniform motion the same formula applies by making v the velocity relative to the sphere.

This result may be extended to several systems of small bodies. Let there be distributed through the indefinite space, in each unit of it, n' bodies of one system, n'' bodies of a second system, n''' bodies of a third system, and so forth, and let the bodies of the first system move in one direction with a velocity v' relative to the sphere, let the bodies of the second system move in another direction with a velocity v'' relative to the sphere, and so forth, then will the number of bodies which the sphere intercepts in each second be equal to

$$\pi R^2n'v'\left(1+\frac{v_0^2}{v'^2}\right)+\pi R^2n''v''\left(1+\frac{v_0^2}{v''^2}\right)+\pi R^2n'''v'''\left(1+\frac{v_0^2}{v'''^2}\right)+\text{etc.}$$

Call this N′ and we may write,

$$N'=\pi R^2\left(\Sigma n'v'+v_0^2\Sigma\frac{n'}{v'}\right),$$

where the summation indicated by Σ extends to all the systems of bodies.

If V is the mean value of v', v'', &c., for all the bodies and n is the sum of $n'+n''+n'''+$&c., then

$$\Sigma n'v'=nV.$$

The remaining term is the sum of fractions whose denominators vary. We may, however, write,

$$v_0^2\Sigma\frac{n'}{v'}=\frac{nv_0^2}{V}(1+\theta),$$

where θ is a number and is evidently positive. If the values of v', v'', v''', &c., do not vary widely, θ will be small. Making these substitutions, we have

$$N'=\frac{\pi nR^2}{V}(V^2+v_0^2+\theta v_0^2).$$

This formula expresses the number of meteoroids which the earth intercepts by considering the earth with its atmosphere as the supposed sphere, R its radius measured from the upper part of the region of meteor-paths, V the average velocity of the meteoroids when they come into the region of the earth's sensible attraction, v_0 the velocity of a body falling by the earth's attraction from infinity to a distance R from the centre of the earth, N′ the average number of meteoroids coming into the earth's atmosphere in a second, and n the average number in a cubic unit of the space the earth is traversing in a given period.

If m be the average number visible at one place in a unit of time we have found above that $N=10,460\,m$. The volume of a sphere whose radius is R is $\frac{4}{3}\pi R^3$. Let M be the number of meteoroids in a space equal to such a sphere, then $M=\frac{4}{3}\pi n R^3$, and

$$10460\,m=\tfrac{3}{4}\frac{M}{vR}(v^2+v_0^2+\theta v_0^2),$$

or

$$M=\tfrac{4}{3}\cdot\frac{10460\,m\,Rv}{v^2+v_0^2+\theta v_0^2},$$

where m denotes the average number (or fraction of a number) seen in one place per second. If the hourly number is, as before assumed, equal to 30, then $m=\frac{1}{120}$, and $M=116{\cdot}2\,\frac{Rv}{v^2+v_0^2(1+\theta)}$.

8. *The mean length of apparent paths.*—I have computed or measured the lengths of 1016 paths, observed by a large number of persons. The mean length is found to be 12°·6.

9. *Numbers of Telescopic shooting stars.*—Shooting stars are of all degrees of brilliancy, and there are many very faint ones. Almost every hour that a person watches, he sees, or thinks he sees, flights that are yet so faint as to leave him in doubt whether they are shooting stars, or only illusions. We may therefore reasonably conclude that large numbers of shooting-stars are entirely invisible to the naked eye, which yet might be seen by the telescope.

This conclusion is verified by observation. In 1854, Messrs. Pape and Winnecke observed[1] together at Göttingen for 32 hours, on nights between the 24th of July and the 3d of August. Pape saw with the naked eye 312 shooting stars, and Winnecke saw 45 in the same time with a comet seeker. The diameter of the field of view is not given, but in observations at the same time diameters of 53′ and 36′, with powers of 30 and 60, were used.

If the apparent length of a meteor-path is l and the breadth of the field of view of a telescope is b, and if the axis of the telescope is directed towards any part of the area whose length is l, and whose breadth is b, the meteor-path would cross the field of view. If all paths were of the same length, b, and were equally distributed over the heavens, a telescope would then command a portion of the whole heavens expressed by the fraction *lb* ÷ *surface of sky*. Meteor-paths diminish somewhat in length with apparent brightness. On the other hand a path may perhaps be longer when viewed by aid of the telescope than when seen by the naked eye. Hence for the approximate mean value of l may be taken 12°·6, the mean value of the length of the apparent meteor-paths visible to the naked eye. Let b be 53′

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[1] Astron. Nachrichten, xxxix, 113.

and the ratio of those actually seen through one telescope to those which are bright enough to be visible in it is $53 \times 12^\circ{\cdot}6 : 360 \times 60 \times 180 \div \pi$, or, 1:1853. I have selected the larger diameter of the telescope that the ratio may not be too large. For the same reason I prefer to reject in the divisor that part of the surface of the heavens within 15° of the horizon. This makes the ratio $1 \div 1371$.

We have seen that according to Bouvard's observations one person should see an average of eight shooting stars per hour. Hence if $\frac{45}{312}$ is taken as the ratio of the number seen in a comet seeker to those seen by one person with the naked eye, there should be in each hour $\frac{8 \times 45 \times 1371}{312}$, or 1582 shooting stars hourly that might be visible through a comet seeker if the whole heavens could be watched. The ratio between those visible at one place and those visible somewhere over the whole earth is $1 \div 10{,}460$ for common meteors. If the same ratio applies to telescopic shooting stars (doubtless not a very reasonable supposition), we have for the whole number of meteoroids coming daily into the air, at least, $1582 \times 24 \times 10460$, or more than 400,000,000. There is, moreover, no reason to doubt that a farther increase of optical power would reveal still larger numbers of these small bodies.

10. *Mean distance of the meteor-paths from an observer.*—Although the data in our possession are inadequate to furnish the mean distance of the meteor-paths from an observer, yet limits to this mean distance can be obtained. Not so large a portion of the meteor-paths along an oblique line can be seen as along a vertical line. Now from the distribution of the paths in absolute altitude, and their distribution over the apparent heavens, can be computed the mean distance on either of two suppositions:

1st, That those paths along the oblique line that become invisible are always the most distant ones;

2d, That the paths which become invisible are distributed along the oblique line in proportion to the numbers along the line.

It is evident that the first supposition affords a mean distance less than the truth, and that the second affords one that is too great. These two limits are found to be about 232 and 140 kilometers. It must, however, be admitted that in this computation the unavoidable errors arising from using finite summation for integration, and from other sources, are considerable.

11. *Mean foreshortening of the meteor-paths by perspective.*—To determine the effect of perspective in shortening the apparent paths we have need to use the following geometrical proposition:

If a sphere whose radius is a be supposed to have an indefinite number of diameters, and the extremities of these diameters are distributed uniformly over the surface of the sphere, and if O be a point without the sphere whose distance from the center of the sphere is b, then will the mean value of the angles at O, subtended by all these diameters, be equal to $\frac{\pi}{2}\frac{a}{b}$.

The mean effect of the foreshortening by perspective may therefore be thus expressed. Let a diameter of the sphere be bent into the arc of a circle whose radius is b; then the angle it thus measures, is to the mean value of the angles at O, as any square, is to its inscribed circle.

This effect is independent of the ratio $a:b$, except that it must be less than unity. Hence the proposition may be applied to any number of short lines viewed by an observer, if the directions of the lines be properly distributed.

If shooting stars came directly downward we should see all that were coming toward us, since they would be near the zenith. We should see few, if any, whose paths are at right angles to the line of vision, for those would be down near the horizon, and would be concealed by mists and smoke. It seems probable that in this case the mean effect of foreshortening would be a little greater than that of the diameters of a sphere.

Again, if the paths were all parallel to the horizon we should see an undue number moving nearly at right angles to the line of vision. For those which are diminished most by the foreshortening would be near the horizon, and hence mostly hidden.

But the directions of the meteor-paths are from all parts of the heavens from horizon to zenith. It seems reasonable to conclude that the mean effect of foreshortening is intermediate also between that for paths coming from points in the horizon, and that for paths coming from the zenith. Hence it ought to be nearly represented by the diameters of the sphere.

The conclusion may be thus expressed. The mean length of the observed paths of shooting stars is found to be 12°·6. If every path was supposed to be turned about its middle point, and bent into an arc of a circle of which the observer's eye was the center, the mean apparent length would be increased, in the ratio of a square to its inscribed circle; that is, it would be equal to $12°{\cdot}6\times\frac{4}{\pi}$, or about 16°·04.

12. *Mean length of the visible part of meteor-paths.*—From the mean distance and mean angular length of the meteor-paths can be computed approximately their mean lengths. For if l be the mean length, and b the mean distance, we have

$$\frac{l}{b}=\frac{\pi\times16°{\cdot}04}{180°},\quad\text{or}\quad l=0{\cdot}28b.$$

The two limits for the mean distance given above indicate that the mean length is less than 65, and greater than 39, kilometers.

These results should, however, be diminished a little, since the *mean* distance and *mean* angular length give too large a *mean* length. The smaller of the two limits given is doubtless much nearer the truth.

13. *Mean duration of flight, and the mean velocity of the shooting stars.*—Mr. Wartmann of Geneva gives the duration of the flights of 368 shooting stars observed at Geneva during one night by six observers. The aggregate is 180ˢ·33, which gives a mean of 0ˢ·49 for each flight. The mean of 499 estimates made in August and November last is 0ˢ·418. The mean duration of the 867 flights is 0ˢ·45.

If the estimates of those observers who were accustomed to astronomical observation, and hence to judging of small intervals of time, had been alone taken, the result would have been very nearly the same. Almost all observers agree that the mean duration of flight is not greater than half a second.

A mean duration of half a second, and a mean length of path between 39 and 65 kilometers seem to imply a mean velocity between 78 and 130 kilometers per second. The smallest of these (more than forty-eight miles,) is twice and a half the velocity of the earth in its orbit about the sun. This cannot consist with the supposition that most of the meteoroids move in closed orbits about the sun. Hence, we must accept as highly probable one or more of these three conclusions, viz:—

1st, That the length of track is too long; which seems to involve that the altitudes on which our computation is based are, on the whole, too large. All the altitudes greater than 150 kilometers, might, I think, have been safely rejected.

2d, That the estimates of time are, in general, too small. This is quite probable. The mind may not make proper allowance for the time that elapses after the shooting star is seen, before the eye is directed to the place of the path.

3d, That very many of the meteors move in hyperbolic orbits about the sun. Whatever may be said of the sporadic meteors, this cannot be true of the members of the August and November groups.

14. *The sporadic shooting stars cannot all belong to one narrow ring which has a diameter nearly equal to that of the earth's orbit.*—Such a ring would have to be but little inclined to the ecliptic in order to furnish meteors throughout the year. The bodies could not have a retrograde motion, else shooting stars would be seen only in the morning hours, and would, moreover, have a very distinctly marked radiant.

They cannot have a direct motion. For their velocity must then be nearly equal to, and yet a little less than, that of the earth, in order that more be seen in the morning than in the evening. Their relative velocity on entering the atmosphere would be not much greater than that of a body falling to the earth from an infinite distance—that is, not much greater than 11 kilometers per second. So small a mean velocity is entirely inconsistent with direct observations, as well as the conclusions given above.

We might, it is true, suppose the ring to have a considerable breadth, in which case the meteors would have a larger mean relative velocity. But if the breadth be such as to furnish a velocity at all consistent with observation, we have no longer a ring lying between the orbits of Mars and Venus, but a disk extending much beyond these planets.

15. *A large portion of the meteoroids must, when they meet the earth, have absolute velocities greater than the earth's velocity in its orbit; or else, the sporadic meteors have a series of radiants at some distance from the ecliptic, and hence come from a series of rings considerably inclined to the earth's orbit.*—For shooting stars cannot have relative motions upward from the earth's surface when they enter the atmosphere. If then the absolute velocities of the meteoroids were all less than those of the earth, their relative motions (neglecting the earth's attractions,) would all be from points of the heavens less than 90° from that point toward which the earth is moving. To an observer early in the evening but a small part of this hemisphere is above the horizon, while in the morning almost all of the hemisphere is visible. Hence, either the number of those seen in the morning should be *very much greater* than that of those seen in the evening, or else there should be a radiant in that part of the sky which is above the horizon in the earlier hours of the night. If the earth's attraction be considered the disproportion between the morning and evening hours would be slightly diminished. But even with this allowance the increase through the night would be greater than the observed increase, unless one of the two suppositions above given is true.

16. *Distribution of the orbits of the meteoroids in the solar system.*—There are at least three suppositions respecting the distribution of the orbits of the meteoroids in the solar system which are naturally suggested. Either of them may be considered as plausible, and one does not exclude another.

1st. They may form a number of rings, like the August group, cutting or passing near the earth's orbit at many points along its circuit. The sporadic shooting stars may be outliers of such rings.

2d, They may form a disk in or near the plane of the orbits of the planets.

3d. They may be distributed at random, like the orbits of the comets.

According to the first of these suppositions there should be a succession of radiants corresponding to the several rings. Dr.

Heis and Mr. R. P. Greg believe that they have detected such a series. Continued observation directed to this end will probably decide whether the meteoroids belong entirely, or mostly, to rings.

According to the second supposition the apparent paths of the sporadic meteors should, if produced in the direction of the motion, all cut the ecliptic below the horizon. For the absolute motion of a meteoroid and the earth's motion being both in, or near, the ecliptic, the resultant relative motion should be from some point in the ecliptic. That point should be above the horizon, since the body must move downward to enter the atmosphere. And as the apparent path produced backward cuts this point, it will, if produced forward, cut the ecliptic below the horizon.

We have thus a simple means of determining whether the sporadic meteors come exclusively, or even largely, from a disk or a lenticular shaped group about the sun, like that which the zodiacal light is often supposed to indicate.

If the third supposition is true, the mean velocity of the meteors is a function of the numbers of shooting stars in the different hours of the night. For, if the velocity is very small, few would be seen in the evening, while if it is very large, there should be nearly as many in the evening as in the morning.

Consider a small spherical space in the upper region of the earth's atmosphere as receiving the meteors.

By supposition the absolute motions of the meteoroids are directed in equal numbers from all parts of the celestial sphere. If their velocities are all equal and represented by v', the earth's velocity being v, if N_0 represents the numbers of those whose absolute motions in a given period are from the visible celestial hemisphere, n the whole number that should come from all parts of the celestial sphere, and θ the distance from the zenith to that point of the heavens to which the earth is moving, then we find between these quantities the equation,[1]

$$N_0=\frac{n}{2}\left(1+\frac{v}{v'}\cos\alpha\right).$$

Let l be the latitude of an observer, and h the hour-angle counting from the time when the point toward which the earth is moving is on the meridian, then when that point is on the equator we have $\cos\alpha=\cos l\cos h$. This will correspond to the mean of the year. Computing now the values of $N_0\div n$, on the several suppositions $v'=v\surd 2$, $v'=v$, and $v'=\frac{1}{2}v$, for the latitudes of New Haven, and of Paris we have the following table. When $v'=v$ we have orbits whose major axes equal those of the earth. When $v'=v\surd 2$ we have parabolic orbits.

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[1] After reading this memoir, I found that Mr. A. S. Herschel had obtained the same formula.

Table showing the hypothetical distribution of the shooting stars through the hours of the night.

Hour of the night.	New Haven.			Paris.		
	$v'=v\surd 2$	$v'=v$	$v'=\cdot 8v$	$v'=v\surd 2$	$v'=v$	$v'=\cdot 8v$
6	·765	·875	·970	·782	·829	·911
7	·756	·862	·954	·725	·818	·897
8	·730	·825	·907	·701	·785	·856
9	·687	·765	·832	·664	·732	·791
10	·633	·688	·735	·616	·665	·706
11	·569	·597	·622	·560	·585	·606
12	·500	·500	·500	·500	·500	·500
1	·431	·403	·378	·440	·415	·394
2	·367	·312	·265	·384	·335	·294
3	·313	·235	·168	·336	·268	·209
4	·270	·175	·093	·299	·215	·144
5	·244	·138	·046	·275	·182	.103
6	·235	·125	·080	·268	·171	·089

The velocities have been considered as uniform. But it is evident that if the shooting stars have various velocities, the hourly distribution should correspond approximately to that for the mean velocity.

The number entering a spherical space has been considered. In fact we have to consider the number entering the region of the atmosphere commanded by observers at one place. The two are not identical. For if so, we ought, on the nights of 9–12th of August to have the conformable meteors equally distributed through those hours of the night in which the radiant is above the horizon. But there is in fact an increase of numbers till dawn. Hence the difference between the numbers seen in morning hours, and those seen in the evening hours, should be greater than is given by the preceding table.

On the other hand, the effect of the earth's attraction is to render the numbers a little more uniform through the night. Again, there should be less difference between the morning and evening during the half year from July to December, than in the other half year, for the point to which the earth is then moving is north of the equator.

Mr. Herrick estimated that there were about three times as many shooting stars in the morning as in the evening. Mr. Coulvier-Gravier gives the following[4] as the mean hourly numbers of shooting stars at Paris:—

5h—6h	7·2	10h—11h	8·0	3h—4h	15·6
6 —7	6·5	11 —12	9·5	4 —5	13·8
7 —8	7·0	12 — 1	10·7	5 —6	13·7
8 —9	6·3	1 — 2	13·1	6 —7	13·0
9 —10	7·9	2 — 3	16·8		

How much reliance is to be placed on these numbers of the Paris observer we know not.

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[4] Recherches sur les météores, p. 220.

This estimate, and these numbers, taken strictly imply a mean velocity greater than that of a parabolic orbit. The character of the data does not allow the argument to be pressed. Yet we must regard as almost certain (on the hypothesis of an equable distribution of the directions of absolute motions), that the mean velocity of the meteoroids exceeds considerably that of the earth; that the orbits are not approximately circular, but resemble more the orbits of the comets.

17. *Number of meteoroids in the space which the earth is traversing.*—We have found that, of the space through which the earth is moving, a volume equal to that of the earth (atmosphere included), contains a mean number of meteoroids expressed by the equation

$$\mathrm{M}=116{\cdot}2\,\frac{\mathrm{R}\,\mathrm{V}}{\mathrm{V}^2+v_0^2(1+\theta)}.$$

In this equation V is the mean relative velocity of the meteoroids. If their absolute velocities were equal, and the points from which they come uniformly distributed over the heavens, we should evidently have

$$\mathrm{V}=\frac{1}{2}\int_0^\pi(v^2+v'^2-2vv'\cos\omega)^{\frac{1}{2}}\sin\omega d\omega=v+\frac{v'^2}{3v}.$$

For v' we may use as an approximation the mean absolute velocity. If $v=v'$, then $\mathrm{V}=\frac{4}{3}v$. If $v'=v\surd 2$, then $\mathrm{V}=\frac{5}{3}v$. As the velocity seems to be greater than that of the earth, and more nearly equal to that of bodies moving in parabolic orbits, the latter value is preferred for V. On this supposition, and by neglecting θ, we find M greater than 14,000. If allowance be made for the space occupied by the earth's atmosphere we find that, in the mean, *in each volume of the size of the earth, of the space which the earth is traversing in its orbit about the sun, there are as many as* 13,000 *small bodies, each body such as would furnish a shooting star visible under favorable circumstances to the naked eye.* If telescopic meteors be counted, this number should be increased at least forty-fold.

There seems to be little reason for supposing that the space near the earth's orbit is very much more thickly strewn with these bodies than other parts of the solar system. That they are grouped according to some law, is altogether probable. But a velocity different from that of the earth implies, of necessity, that they are not grouped closely about the earth's orbit.

These bodies cannot be regarded as the fragments of former worlds. They are rather the materials from which the worlds are forming. If astronomy furnishes any measure of their total mass we may therefrom obtain some idea, rude though it be, of the mean mass of the individuals.

ART. XXIV.—*Action of Binoxyd of Lead and Sulphuric Acid on Hippuric Acid;* by Dr. JULIUS MAIER, Chemical Assistant, School of Mines, Columbia College.

Hipparaffin, C_8H_7NO.

HIPPURIC acid, at a moderate temperature, is treated with binoxyd of lead and sulphuric acid, care being taken to avoid an excess of either. A strong evolution of carbonic acid ensues, and the mass congeals to a thick paste. This is placed on a filter, and washed with a dilute soda solution till the wash-water shows no further reaction of sulphuric acid. This mass is treated with alcohol, which dissolves the hipparaffin, and evaporated in a water-bath to one-half its volume. On cooling, the entire liquid congeals to a crystalline mass. The separated hipparaffin is collected on a filter, and washed with boiling water, until all acid reaction has disappeared, and is then redissolved in hot alcohol, and allowed to crystallize. Hipparaffin forms a network of white silk-like needles, which melt at 210° C., subliming previously at 100° C. They are entirely insoluble in water, that reagent not even causing them to become moist; they are readily soluble in alcohol and ether. Hipparaffin owes its name to the fact of its not being perceptibly decomposed by acids or alkalies. It is not soluble in solutions of potassa, and develops ammonia only after continued fusing with caustic potassa; it is easily dissolved by a mixture of sulphuric and nitric acids, and is not separated from this solution by water. Fuming nitric acid dissolves it with liberation of gas. This solution, after neutralization with carbonate of soda and evaporation, yields benzoic acid on addition of chlorhydric acid. Upon being heated in a stream of chlorhydric acid gas, a colorless oil passes over, which congeals to a crystalline mass. Schwarz,[1] who first described hipparaffin gave it the incorrect formula $C_{16}H_8NO_2$. I found its composition to be C_8H_7NO, the formation of which is explained as follows:

$$\overset{\text{Hippuric acid.}}{C_9H_9NO_3}+\overset{\text{Binoxyd lead.}}{Pb_2O_2}+\overset{\text{Sulphuric acid.}}{S_2H_2O_4}=\overset{\text{Hipparaffin.}}{C_8H_7NO}+\overset{\text{Carbonic acid.}}{CH_2O_3}+\overset{\text{Sulphate of lead.}}{S_2Pb_2O_4}+\overset{\text{Water.}}{H_2O}$$

Hipparin, $C_8H_9NO_2$.

When an excess of sulphuric acid is used in the preparation of hipparaffin, an oily liquid is separated which rises to the surface of the alcoholic solution, and on cooling congeals to a crystalline mass, on the addition of water. This mass dissolves in hot water, and crystallizes in long, white, glassy needles, grouped in the shape of fans. These needles melt at 49° C., and become solid at 20° C. They are soluble in hot water, alcohol and ether. Their formation may be explained as follows:

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[1] Schwarz Annal., lxxv, 201.

$$\overset{\text{Hippuric acid.}}{\text{Є}_9H_9N\Theta_3}+\overset{\text{Binoxyd of lead.}}{Pb_2\Theta_2}+\overset{\text{Sulphuric acid.}}{S_2H_2\Theta_4}=\overset{\text{Hipparin.}}{\text{Є}_8H_9N\Theta_2}+\overset{\text{Carbonic acid.}}{\text{Є}H_2\Theta_3}+\overset{\text{Sulphate of lead.}}{S_2Pb_2\Theta_4}$$

Hippuric and Benzoic acids are entirely decomposed by heating them with an excess of superoxyd of lead and sulphuric acid, all carbon being driven off in the form of carbonic acid.

The following are the products of the decomposition of hippuric acid by binoxyd of lead and sulphuric acid:

1. $\text{Є}_9H_9N\Theta_3+Pb_2\Theta_2+S_2H_2\Theta_4=\text{Є}_8H_9N\Theta_2+\text{Є}H_2\Theta_3+S_2Pb_2\Theta_4$
2. $\text{Є}_9H_9N\Theta_3+Pb_2\Theta_2+S_2H_2\Theta_4=\text{Є}_8H_7N\Theta+\text{Є}H_2\Theta_3+S_2Pb_2\Theta_4+H_2\Theta$
3. $\underset{\text{Hippuric acid.}}{\text{Є}_9H_9N\Theta_3}+\underset{\text{Binoxyd lead.}}{Pb_6\Theta_6}+\underset{\text{Sulphuric acid.}}{S_6H_6\Theta_{12}}=\underset{\text{Benzamid.}}{\text{Є}_7H_7N\Theta}+\underset{\text{Carbonic acid.}}{\text{Є}_2H_4\Theta_6}+\underset{\text{Sulphate of lead.}}{S_6Pb_6\Theta_{12}}+\underset{\text{Water.}}{H_4\Theta_2}$

Action of Bromine and Iodine on Hippuric Acid.

Bromohippuric acid, $\text{Є}_9H_8BrN\Theta_3$.

An alcoholic solution of hippuric acid, at a boiling heat, is treated with bromine; the solution is allowed to boil a few minutes and water added, and then evaporated on a water-bath to one-half its volume. The yellowish brown color of the solution gradually disappears, and, on cooling, a white crystalline deposit is formed, which consists of fine needles plainly visible under the microscope. These needles are difficultly soluble in water, alcohol or ether, when cold, but readily soluble in all of these media upon boiling. They are colorless; when moistened and allowed to stand for a short time at an ordinary temperature, they undergo decomposition and turn brown, owing to the separation of bromine. The aqueous solution has a strongly acid reaction, and is not precipitated by lime, baryta, or the salts of copper. Upon being heated with caustic lime, the crystals emit a distinct odor of benzonitril, and a violet colored oily liquid was formed, previous to which benzoic acid having a red color passed over. The crystals dried in vacuo and burned with oxyd of copper, and finally in a stream of oxygen, gave the following results:

0·2735 grms. gave 0·4160 grms. carbonic acid and 0·083 grms. water.
0·2760 grms. burned with caustic lime, gave 0·1995 grms. bromid of silver.
0·6855 grms. burned with soda-lime, gave 0·0405 grms. ammonia.

		Calculated.	Found.		
			I.	II.	III.
9. Carbon,	108	41·86	41·50	....	....
8. Hydrogen,	8	3·10	3·40	....	....
1. Bromine,	80	31·00	....	30·91	....
1. Nitrogen,	14	5·43	....	....	4·86
3. Oxygen,	48	18·61	....	....	....
		100·00			

The salts of potash and soda could not be obtained in crystals. The salt of lime, $Є_9H_7CaBrNΘ_3$, crystallizes in fine white needles, is difficultly soluble in cold water, readily so in hot water; dried between 90° and 100° C. it was not decomposed. The analysis gave the following result: 0·3835 grms. of the dried crystals gave 0·0665 grms. carbonate of lime, the formula as calculated requiring 0·0692 grams.

Iodohippuric acid, $Є_9H_8INΘ_3$.

This acid was obtained in the same manner as the bromine compound, employing iodine instead of bromine. To the alcoholic solution water was added, and it was filtered from the separated iodine; the filtrate was allowed to stand over night. The solution which was at first brown became colorless, and fine needles were deposited. These are white, odorless, difficultly soluble in water, alcohol and ether, in the cold, but readily so in all of them upon boiling. They are decomposed at 90° C., turning yellow, owing to the separation of iodine. All the salts with the exception of that of silver are soluble in water.

The crystals, when burned with oxyd of copper, and finally in a stream of oxygen, gave the following results:

0·2430 grms. gave 0·3080 grms. carbonic acid and 0·0605 grms. water.
0·2080 grms. gave 0·1590 grms. iodid of silver, when burned with caustic lime.
0·4760 grms. burned with soda-lime gave 0·0240 grms. ammonia.

		Calculated.	Found.		
			I.	II.	III.
9. Carbon,	108	35·41	34·57	....	....
8. Hydrogen,	8	2·62	2·76	....	....
1. Iodine,	127	41·64	....	41·30	....
1. Nitrogen,	14	4·59	....	....	4·16
3. Oxygen,	48	15·74	....	....	....
		100·00			

New York, Dec. 1st, 1864.

Art. XXV.—*On the Preparation of Oxalate of Ethyl*; by M. Carey Lea, Philadelphia.

The processes described in the text books for the preparation of oxalate of ethyl are liable to objection. If we heat binoxalate of potash with sulphuric acid and alcohol, we introduce into our distillate the decomposition-products of sulphuric acid in addition to those of oxalic acid and alcohol. It has also been recommended[1] to heat oxalic acid in a retort until it begins to emit white fumes, and then allow alcohol to drop in through the tubulure. As the temperature at which the acid begins to sublime is considerably below that at which it melts, this process is apt to result in breaking the retort.

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[1] Gmelin Handbook, Cav. ed., ix, 179.

I was therefore led to seek for a better process, and have used the following repeatedly, with very satisfactory results.

A tube of an inch or more in diameter, and of a proper length, is drawn out at one extremity, and at the same time bent to an angle, somewhat as in the ordinary form of a combustion tube, except that the bent part is made less slender, and shorter ($\frac{1}{4}$ inch diameter at extremity). The tube is then completely filled with dehydrated oxalic acid, except for the last two inches at the open end. This is closed with a cork penetrated by a tube. For this purpose the cork is not bored parallel with its axis, but in an inclined direction, so that the smaller tube when inserted two or three inches may touch the side of the larger. This last is then supported in a horizontal position over a gas combustion furnace, so that the bent end may be directed upwards, and the cork turned so that the small tube may touch the upper side of the interior of the large one. The tube should be supported about six inches above the wire gauze of the furnace. The bent end of the tube is now connected with a Liebig's condenser, and the small tube with a flask containing alcohol of 42 B. The furnace is then lighted, and as soon as the acid melts, a stream of alcohol vapor is sent over it by heating the flask. The gas-furnace is maintained at a heat such that a very gentle ebullition shall be kept up in the tube. At the end of an hour to an hour and a quarter the operation is terminated, and the oxalic acid in the tube is converted into oxalic ether. The distillate also contains a little oxalic ether which cannot be separated by water, but which may be used for preparing oxamid. Or the distillate may be gently evaporated to one-tenth its bulk, and then may be mixed with a little water.

This process gives an abundant product, and requires no attention after the proper temperature is once adjusted. In filling the tube with the oxalic acid, it is not necessary to leave an air passage; the material settles in fusing sufficiently without. The oxalate of ethyl, which is a little brownish, and contains the usual impurities of the crude product, may then be purified in the ordinary manner.

ART. XXVI.—*Method of applying the binocular principle to the eye-piece of a Microscope or Telescope;* by ROBERT B. TOLLES.

To apply the binocular principle to the eye-piece of a microscope or telescope, it is only necessary to make use of the erecting form of eye-piece, and to place the dividing prism at the point where the pencils composing the whole bundle of rays proceeding from the object cross in the eye-piece, which is the point where, in any erecting eye-piece, the diaphragm proper is correctly placed.

If the theory of the erecting eye-piece of common form were generally understood, no demonstration that binocularity can be given to such an eye-piece would be necessary.

Suffice it to say that, since any one pencil of light proceeding from any point of the object through the *whole area* of the object-glass *does* at this point equally fill the whole area of the diaphragm (that being of proper aperture) substantially in the same manner, therefore the division for binocular vision, if made here by the appropriate prism, must be a very nearly equal division of every particular pencil, and give a similar and satisfactory image of the entire field in each eye-tube. This is a sufficient expression of the whole theory of the binocular eye-piece.

It is, however, important in order to avoid pseudoscopic effects, to adopt the proper form of dividing prism; and this form is precisely that best suited to that kind of binocular microscope in which the dividing prism is placed immediately above the objective. The natural presumption has been—contrary to this—that prisms of rectangular form would give the proper effects in the eye-piece, because of the pseudoscopic effects produced by their use in the microscope of binocular body. But this is an error, inasmuch as the pencils proceeding to form the second image in the erecting eye-piece reach the small dividing prism under conditions suitable for correct vision of the object *were the eye placed there*, and, accordingly, the same false appearances obtain with the eye-piece of rectangular prisms, *having oculars above*, as if such division were made immediately above the objective; the effect being, that the order in which the rays proceeding from the sides of the object or image viewed reach the eyes, as to right and left, is reversed from that which exists in natural vision; the left eye receiving a preponderating portion from the right side, and the right from the left side of the object.

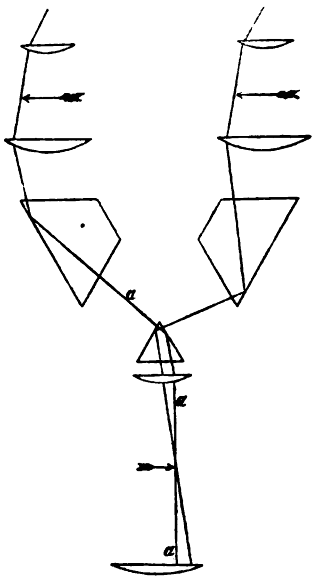
It is to be noted, however, that the eye-piece with rectangular prisms, arranged after the first method of Prof. Riddell, does not uniformly produce conversion of relief, or that inversion of perspective which obtained in that first experimental arrangement for a binocular microscope. Such a *binocular eye-piece* used in the microscope upon transparent objects only occasionally gives the view in depth thus inverted. With low powers, and considerable thickness of the transparent object, the view is usually pseudoscopic. With medium and higher powers, it is otherwise; and the effect is much controlled in this respect by the direction of the light upon the object.

When the binocular eye-piece formed with rectangular prisms is used in the telescope to view a landscape, the perspective is not *throughout* inverted, but portions of the field appear interposed between the eye and nearer objects in a singular and somewhat startling manner.

By arranging the compound rectangular dividing prism so that the optical pencil is divided in the *plane of vision*, instead of vertically, the pseudoscopic effect is almost entirely obviated.

In constructing the binocular eye-piece, the prisms and arrangement of Nachet have been found to answer every condition and requisite of binocular vision. The dividing prism being placed, as before stated, at the point of crossing of pencils in the erecting eye-piece, each pencil of light will enter the small dividing prism and impinge upon its reflecting surfaces in a manner similar to that illustrated in the Nachet binocular microscope. The binocular eye-piece has greatly the advantage over the other arrangement. For when the prisms are placed *in the binocular body immediately above the objective*, their position, in order to secure a *proper division* of each transmitted pencil, should change with every change of objective used—which can be easily provided for in the case of low powers but is rather impracticable with the higher numbers, it being very difficult to bring the prisms sufficiently near to the posterior combination of the objective. On the contrary, when the binocular arrangement is embodied *in the eye-piece*, the prism being once fixed in proper position, as before described, is correctly placed for every power of objective, and the eye-piece, thus binocular in form, is as applicable through the whole range of powers as if it were monocular. Applied to high powers, only one condition would be distinguishingly critical in the case of the eye-piece,—that of the centricity of the central prism. The form of *erecting eye-piece* found most advantageous in this binocular adaptation is a duplication of the ordinary Huyghenian negative eye-piece, wherein the small dividing prism is very nearly at the eye-hole point of such a negative eye-piece as is ordinarily applied in the monocular microscope. At a proper distance above this is placed another negative eye-piece, in which is formed a second image of the object viewed.

This form of erecting eye-piece gives less extension above the body of the microscope than the positive form, and for that reason is preferred.



The annexed diagram illustrates the division of one lateral pencil proceeding from one point of the first image formed in the eye-piece, and its general course to emergence at the two eye surfaces of the eye-piece. When the eye-piece is constructed of the form outlined in the annexed diagram, (which is about half size in the superior portion), the field is produced very satisfactorily, and of tolerable expansion; and does not necessitate more than 4·50 inches extension beyond the microscope body. The draw-tube can be as well withdrawn, and the eye-piece occupy its place, thus diminishing somewhat the total extent of the instrument. With proper modifications of the system of lenses placed before the prisms in the eye-piece, the whole binocular arrangement can be brought still nearer the objective, and retain also all the characteristics of the binocular eye-piece as contradistinguished from the binocular microscope known and in use.

The objection that loss of light must occur on account of the additional front system of lenses pertaining to an erecting eye-piece, (the lower system in the diagram,) of course militates against this arrangement; but there are, on the other hand, incidental advantages in the use of the erecting form. And moreover, if desirable, the "eye-lens" of the lower system can be successfully united with the dividing prism, thus eliminating two surfaces; and it is practicable to make the field-lens of each upper system solid with the opposite prism. The binocular erecting microscope would thus have no more refracting surfaces than the instrument of negative form and binocular body. As the object is, for the sake of efficiency with a high power objective, to give as large area to the transmitted pencil as possible at the point where it undergoes division in the small prism, therefore the power of the front system should be kept down, and amplification, as far as necessary in the eye-piece, be produced after the division has taken place. The accompanying cut is in this respect not in strict proportion—the lower system of lenses being represented not more than half the distance apart that the most favorable arrangement for use with high powers (and equally with low) would call for, and the standard length of microscope body admit of. But the aperture of the lenses might remain about the same.

Having obtained by this means a pencil (or beam rather) transmitted through the eye-piece of the greatest possible dimension or area, at the point of binocular division, greater amplification in the eye-piece, as to its total power, might be advantageously effected by means of lenticular immergent and emergent surfaces of the upper prisms; the lower face of each prism to be *convex*, the upper emergent surfaces concave, giving achromatized refraction in each case. By this means, a larger field, together with a minimum length of tubes above the prisms, would be secured.

By thus appropriating every surface of all the prisms not a reflecting surface, for the purpose of lenticular refraction, the greatest aggregate advantage appears to be secured.

Applied to the telescope, the binocular eye-piece should of course have, and practically has, the same characteristics as when used in the microscope. In this case, however, the front lens of the eye-piece can be dispensed with. Its utility, as thus applied to those telescopes too large to be conveniently in the double form, is too obvious and striking to need remark. The view thus obtained is truly stereoscopic.

Canastota, N. Y., Dec. 1864.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On the wave lengths of Fraunhofer's lines.*—ANGSTRÖM has communicated to the Royal Swedish Academy of Sciences a memoir on the wave-lengths of the principal fixed lines in the solar spectrum, which possesses great value not merely as a revision of Fraunhofer's determinations, but also as containing data not hitherto to be obtained. The measurements which form the basis of the work were executed by means of an optical theodolite by Pistor and Martins, reading by means of two microscopes to $2''\cdot1$ of arc. The eye-piece was also provided with a micrometer, the head of the screw being divided into 100 equal parts, each division of the scale corresponding to $1''\cdot308$ for parallel rays. The ruled glass was by Nobert, and contained in a space of 9·0155 Paris lines, 4501 divisions, drawn with a diamond. The error of the division according to Nobert's measures, was less than 0·00002 of a Paris line. With this ruled glass Fraunhofer's lines could be seen in the third and fourth spectra, the lines being themselves more numerous and distinct than in the spectrum produced by a flint glass prism. The observations were calculated according to the well known formula

$$e \sin \theta = m \lambda,$$

in which e is the distance between two successive lines $= 0{\cdot}000166954$ of a Paris inch; λ the wave length sought, θ the observed angles of diffraction, and m the number of the spectrum. The influence of variations of temperature and pressure of the atmosphere was found to be insensible; that of the temperature of the ruled glass, which varied from 13° C. to 18° C., was also found too small to require the application of a correction. The same statement applies to a variation in the angle θ, arising from the absolute motion of the instrument in the direction of the path of the incident ray. As means of several determinations the following results were obtained for the lines measured by Fraunhofer, the unit of measure being $\frac{1}{10000000}$ of a Paris inch.

B	2539·73	b	1916·50	H	1467·18
C	2426·29	F	1797·27	H_1	1453·98
D	2178·59	G	1592·34	—	......
E	1948·24				

The difference in wave lengths for the two D lines was 2·226; between the two E lines only 0·395. (The numbers given above doubtless refer to the least refrangible lines in the cases of D and E.) The author gives a careful discussion of the results of the two series of measurements made by Fraunhofer, and arrives at the conclusion that the measurements made with Nobert's ruled glass deserve the greater degree of confidence. The following table gives the results of Angström's determinations of the wave lengths of other lines in the spectrum, the unit being the same as above.

A	2812	
B	2539·7	
C	2426·26	
a	2312·2, a strong atmospheric line.	
	2287·3	A group of strong lines produced by iron and calcium.
	79·6	
	76·8	
	69·4	
	67·7	
	62·1	
	55·1	
D	2179·70	sodium lines.
	2177·48	
1.	2076·1	Iron.
2.	71·3	Iron.
3.	69·7	
4.	2068·3	Iron.
5.	65·4	
6.	60·1	“
7.	16·9	“
8.	13·6	“
	13·1	
9.	07·3	“
10.	05·3	“
11.	1998·4	Iron.
	97·9	
12.	85·8	“ strong.
	85·3	“ faint.
	84·2	“
	83·5	“
13.	1974·2	“ double.
14.	69·6	“
15.	68·1	“
16.	65·3	“
17.	1953·2	“
E	1948·44	Iron and calcium.
	48·04	
	46·8	“ “ double.
	34·6	Iron.
	36·4	“
	19·6	“ double.
b	1916·50	Magnesium.
$b_{\prime}$	12·39	“
$b_{\prime\prime}$	1911·10	Iron and magnesium.
	10·49	Iron.
	03·4	“
c	1832·70	“
	19·1	“
	18·4	“

	08·3	Iron, double.		71·2	Iron.
	01·1	"	*g*	62·4	Calcium, double.
F	1797·27	Hydrogen.		32·0	Iron, double.
f	1632·2	Iron.	*h*	15·9	No metallic line.
	28·5	"		05·3	Iron, strong.
	20·4	"		02·0	"
	04·3	Hydrogen.		1495·2	"
	1598·8	Iron.		80·4	Strong, no corresponding metallic line.
G	1592·34	Iron.			
	79·1	"	H	67·2	Calcium.
	74·7	"	H_1	54·0	"

In the concluding section of his memoir the author points out a method of determining the motion of the solar system by observing the interference bands produced by a ruled surface of glass. The principle of this method was laid before the Swedish Academy, Oct. 6, 1860, and was proposed by Babinet two years ago in the French Institute. It depends simply upon the fact that if we assume that the rays which pass through the openings in the ruled surface are propagated independently of the motion of the instrument, the same must be true of the interference bands, and consequently, in observing with the telescope, an aberration must ensue proportional to the relation between the motion of the telescope perpendicular to its axis and the velocity of the light in the direction of the axis. The results obtained in testing this method appear to confirm the correctness of the theory, but are not sufficiently numerous to be decisive. The author proposes to resume the subject under more favorable circumstances of weather.—*Pogg. Ann.*, cxxiii, 489. W. G.

[*Note.*—The annexed table of wave-lengths, taken in connection with that of Angström, will present to the reader, it is believed, all, or almost all, the exact knowledge which we have at present of the subject. The unit is here the $\frac{1}{10,000,000}$ of a millimeter.

		Wave-lengths.				Wave-lengths.	
1.	A	= 7680	Mascart.	18.	α' Hg	5759	Plücker.
2.	B	6897	Fraunhofer.	19.	γ CO_2	5599	"
3.	α L	6763	Müller.	20.	γ $SnCl_2$	5584	"
4.	β N	6610	"	21.	β Hg	5461	"
5.	C	6559	Fraunhofer.	22.	α Cl	5451	"
6.	α H	6533	Plücker.	23.	δ I	5337	"
6*b*.	α PCl_3	6493	"	24.	δ $SnCl_2$	5333	"
7.	α $SnCl_2$	6445	"	25.	β O	5328	"
8.	α $SiCl_2$	6329	"	26.	E	5265	Fraunhofer.
9.	α O	6150	"	27.	β Cl	5216	Plücker.
10.	11 N	6089	"	28.	δ CO_2	5190	"
11.	β PCl_3	6024	"	28*b*.	γ O	5185	"
12.	β $SiCl_2$	5978	"	29.	α Br	= 5169	"
13.	β I	5947	"	30.	ε I	5167	"
14.	D	5888	Fraunhofer.	31.	γ $SiCl_2$	5050	"
15.	β $SnCl_2$	5794	Plücker.	32.	F	4856	Fraunhofer.
16.	α Hg	5782	"	33.	β H	4843	Plücker.
17.	17 N	5762	"	34.	β Br	4793	"

35.	γCl	4792	Plücker.	46.	γHg	4359	Plücker.
36.	γBr	4766	"	47.	γH	4339	"
37.	δBr	4691	"	48.	G	4296	Fraunhofer.
38.	ζI	4661	"	49.	ιI	4215	Plücker.
38*b*.	δS_2	4631	Müller.	50.	H	3963	Fraunhofer.
39.	ηI	4629	Plücker.	51.	L	3791	Esselbach.
40.	γPCl_3	4591	"	52.	M	3657	"
41.	$\epsilon SnCl_2$	4524	"	53.	N	3498	"
42.	ζCO_2	4501	"	54.	O	3360	"
43.	θI	4446	"	55.	P	3290	"
44.	ηCO_2	4382	"	56.	Q	3232	"
45.	δO	4367	"	57.	R	3091	"

W. G.

2. *On the phenomenon of interference in the prismatic and diffraction spectra.*—STEFAN has communicated to the Viennese Academy an interesting paper on the interference bands discovered by Talbot and employed by Esselbach,[1] and subsequently by Bernard,[2] for the measurement of the wave-lengths of the ultra-violet rays. Talbot's method of obtaining these bands consists in placing a thin plate of mica before the eye in such a way as to cover one-half of the pupil, leaving the other half free. On looking at a prism so placed that the spectrum is distinctly seen, dark bands will be seen parallel to the edge of the prism, provided that the plate of mica covers the half of the pupil which is upon the same side with the edge of the prism producing the spectrum. Stefan remarks in the first place, that the mica may be placed anywhere between the eye and the prism, and may even be cemented to the prism, provided that it cover the side next the edge. No bands are seen when the rays pass either through the covered or uncovered part of the prism above, but only when the eye receives rays which have passed through both these portions. The plate of mica may be cemented to the farther surface of the prism, or placed at any point between the prism and the slit, provided that the part of the bundle of rays which traverses the plate also passes near the edge of the prism. The same bands may be seen in the diffraction spectra produced by a plate of ruled glass: when the mica covers the left half of the pupil the bands are seen in the spectra lying to the right, and *vice versâ.* When two plates of mica are cemented to the same ruled glass, one to the right and the other to the left, with a narrow space between them which occupies the center of the pupil, bands are seen on both sides in all the spectra. These bands are similar when the plates are of equal thickness; they disappear when the plates are brought together, unless one plate is thicker than the other, when the bands are seen toward the side where the plate is thinnest. The plate of mica must not be too thick in these experiments, as otherwise the number and fineness of the lines become so great that they can no longer be distinguished by the eye. For a prism of 45° the author uses a plate of mica 0·1 millimeter in thickness; with a prism of 60° glass plates up to 0·15 mm. may be used, and a plate of this thickness gives about 120 lines between B and H. The lines produced by thicker plates are best seen by looking at the spectrum with a telescope and placing the inter-

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[1] Pogg. Ann., xcviii, 513.

[2] This Journal, Sept., 1864.

ference plate, as Brewster and Airy have done, between the eye and the eye-piece of the telescope, only, when the astronomical or inverting eye-piece is used, the plate of glass or mica must be placed upon the side which is away from the edge of the prism. Glass plates of the kind used as covering glasses for microscopic preparations are more even than plates of mica and give much sharper lines. The interference plate may be placed anywhere between the eye and the slit of the collimator. By attaching it to the objective on the side toward the refracting edge of the prism the lines are seen at once over the whole spectrum. It is important, in the case of thick plates, that the surfaces should be parallel. A glass plate about three millimeters in thickness with a flint glass prism of 60°, gave about 2500 very sharp and distinct bands between B and H. The bands near B are produced by the interference of rays whose difference of path amounts to over 2700 wave-lengths, while the interval between those near H amounts to more than 5000 wave-lengths. By inclining the interference plate its optical thickness is of course changed; an increase of the inclination causes the lines to run from the violet to the red end of the spectrum, and *vice versâ*. The lines have their maximum deviations when the plate is perpendicular to the direction of the ray, and this affords an easy method of adjustment. As the plate may be placed anywhere between the objective and the prism or between the prism and the collimator, we have the means of determining the influence of physical changes produced in the plate by heat or pressure. The same statements apply in general to the phenomena as seen by the diffraction spectrum. Finally it is possible to produce the interference bands by placing the plate in front of the slit so as to cover one-half of the opening. In this case it is a matter of indifference upon which side of the slit the plate is placed.—*Pogg. Ann.*, cxxii; from *Sitzungs Ber. der kais. Akad. der Wiss.*, xlix. W. G.

3. *On thermo-electric batteries of remarkable power.*—Bunsen has found that kupferkies possesses a far higher place in the thermo-electric series than bismuth, and that pyrolusite is also superior to the metal though inferior to kupferkies. When kupferkies is combined with an alloy of two parts of antimony and one of tin, or, in order to employ high temperatures, with copper, a couple is produced which, under equal circumstances, gives far more powerful currents than any combination hitherto known. In four experiments the ratio between the electro-motive forces in a kupferkies-copper element and in Daniell's battery was found to be as 1 : 14·7, 1 : 11·9, 1 : 10·9, and 1 : 9·7, while the essential resistances to conduction were 0·74, 0·79, 0·72, and 0·72. During the experiments the temperature of one end of the element, which was plunged into water, was about 60° C., while that of the other end was above that of melting tin. The battery gives a ten times more powerful action than a bismuth-antimony element of the same resistance heated to 0° and 100°. Ten pairs of the kupferkies-copper elements fully replace a Daniell's pair of fourteen square centimeters of active copper surface. Kupferkies is easily fused at a high temperature and may be cast in a mould, but its place in the thermo-electric series is then far below that of bismuth. The native mineral, which is easily worked, must be employed. Pyrolusite combined with platinum gives a battery, the electro-motive force of

which may be easily brought to $\frac{1}{10}$ of that of Daniell's battery without decomposing the mineral. In one experiment Bunsen found the electromotive force of pyrolusite and platinum not less than $\frac{1}{9\cdot8}$ of that of Daniell's battery, but the resistance to conduction was 18·4 times as great as that of the Daniell's cell used for comparison.—*Pogg. Ann.*, cxxii, 505.

W. G.

4. *On the thallic alcohols.*—Under this term LAMY has described the compounds which are formed by replacing an equivalent of hydrogen in wood-spirit, common alcohol, &c., by an equivalent of thallium, and which are represented respectively by the formulas $\left.\begin{matrix}C_2H_3\\Tl\end{matrix}\right\}O_2$, $\left.\begin{matrix}C_4H_5\\Tl\end{matrix}\right\}O_2$, $\left.\begin{matrix}C_{10}H_{11}\\Tl\end{matrix}\right\}O_2$, &c. Methyl-thallic alcohol is solid and crystalline. The ethyl compound is a heavy colorless oily liquid; its density at 0° is 3·550; its indices of refraction for B and H are respectively 1·661 and 1·759, the difference between these numbers which measures the dispersive power is 0·098, while the corresponding difference for bisulphid of carbon is 0·079. The amyl-thallic alcohol is also a colorless oil, but its refractive and dispersive powers are much less than those of the corresponding ethyl compound, which is at once the heaviest, the most refractive and the most dispersive of organic compounds. The thallic alcohols are decomposed by boiling; they give by distillation pure hydrogen, alcohol, the corresponding acid and a large residue of metallic thallium. All three compounds burn in the air with a green, more or less brilliant flame, and all are soluble in the corresponding alcohol or in ether. Chloroform also dissolves thallic alcohols, but the solution is soon decomposed with deposition of protochlorid of thallium. Water decomposes these compounds, producing a hydrate of the oxyd of thallium and regenerating the alcohol proper. Acids also decompose them, but carbonic acid forms a solid and definite combination. The analogy between this class of compounds and the corresponding bodies containing potassium and sodium is striking, and confirms the correctness of the view which associates thallium with the alkaline metals and separates it from lead.—*Comptes Rendus*, lix, 780.

W. G.

5. *The Correlation and the Conservation of Forces.*[1]—Under this title Dr. Youmans has presented a collection of memoirs by Grove, Helmholtz, Mayer, Faraday, Liebig, and Carpenter, together with a well written introduction and brief biographical notices of the various writers whose works he has collected. The work is a very welcome addition to our scientific literature, and will be particularly acceptable to those who wish to obtain a popular, but at the same time precise and clear view of what Faraday justly calls the highest law in physical science—the principle of the conservation of force. To scientific men the book will also be most useful for convenience of reference. Sufficient attention has not been paid to the publication of collected monographs or memoirs upon special subjects. Dr. Youmans's work exhibits the value of such collections in a

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[1] The Correlation and Conservation of Forces. A Series of Expositions, by Prof. Grove, Prof. Helmholtz, Dr. Mayer, Dr. Faraday, Prof. Liebig, and Dr. Carpenter. With an Introduction, and Brief Biographical Notices of the Chief Promoters of the New Views. By Edward L. Youmans, M.D. 12mo, pp. xlii, 438. N. Y.: D. Appleton & Co.

very striking manner, and we earnestly hope that his excellent example may be followed in other branches of science. Meantime we will permit ourselves to beg that the faithful laborer in the cause of science and the liberal publisher to whom we owe this volume will still further add to the obligations under which they have laid men of science by giving a complete collection of the mathematical papers which have appeared upon the subject of the mechanical equivalent of heat, and which are now scattered through many volumes of scientific journals and transactions of learned societies, so as to be accessible to few and inconvenient for all.

6. *A new developing solution.*—Some experiments recently undertaken in company with Mr. A. B. Crockett for the purpose of ascertaining the quality of different developing solutions, have led us to the conclusion that a mixture of double sulphate of potash and iron with double sulphate of ammonia and iron, produces better results than any hitherto used. The negatives made by this combination are remarkable for clearness of the shadows, and beauty of half tones, without an excess of intensity in the high lights. The solution acts with remarkable regularity and uniformity, and has little or no tendency to produce fogginess, or a thickening of the fine lines, even when kept upon the plate for a long time.

In practice the following formula was found most convenient. Although not theoretically exact, it produces excellent results.

Pure water, - - - - - - -	32 oz.
Neutral sulphate of potash, - - - - -	½ "
Proto-sulphate of iron, - - - - - -	¾ "
Double sulphate of ammonia and iron, - -	1 "

To this solution add two drops of ammonia, two ounces of acetic acid, and alcohol sufficient to make it flow evenly over the plate. A. E. V.

II. GEOLOGY.

1. *Note on the Geological age of the New Jersey Highlands as held by Prof. H. D. Rogers;* by J. P. Lesley.—The last sentence of an article in the January number of the American Journal of Science, (at p. 97,) extracted from the Proceedings of the Natural History Society of Montreal, seems to me to do great injustice to Professor H. D. Rogers, and other geologists, whose views of the structure and relationships of the rocks of the Highlands have long been published, and are in this article both ignored and misrepresented. The sentence to which I refer reads as follows:—

"This conclusion, although opposed to the views of Mather and Rogers, who looked upon the crystalline rocks of the latter region [the Highlands, to which the interesting mineral region of Orange county and the adjacent parts of New Jersey doubtless belongs] as altered Lower Silurian strata, is in accordance with the older observations of Vanuxem and Keating, and with the more recent ones of Professor Cook, according to all of whom the gneiss and crystalline limestone of Orange county and of New Jersey underlie unconformably the Lower Silurian strata."

Now Prof. Rogers, to my certain knowledge, not only never expressed such an opinion, at any time, but has always maintained just the reverse; and proved, thirty years ago, what he published on page 43, (Chap. 2,) of his New Jersey Report, in 1840, viz: that the "sedimentary rocks [of the Lower Silurian system] repose in immediate contact with the gneiss, presenting from the attitude of their beds, abundant evidence that they were precipitated upon it [that is, upon the system of the Highlands in New Jersey] while it was yet only in part elevated above the waters." The Canadian geologists will find in these words a clear statement of precisely the view which they themselves have learned to entertain by their week's excursion last summer.

Whatever errors Rogers may have made—and we all make errors—no one can charge him justly with so tremendous a blunder as that of regarding the Highland range as a metamorphic repetition of the Silurian sediments. He mistook indeed the crystalline limestones engaged among the Highlands for metamorphosed synclinal outliers of No. II, as at Franklin (see page 62, New Jersey Report); but this is an exceptional case, as any one can see by studying the true outlying Silurian synclinals represented as sunk among the gneiss hills on Rogers's long sections in the New Jersey Report; or by referring to the many sections (made very nearly at the same time) of the synclinal Silurian valleys among the Durham hills of Pennsylvania. Professor Cook has seen horizontal Potsdam or Calciferous beds overlying these upturned Franklin limestones; (see this Journal, [2], xxxii, 208); and there are exposures of similar crystalline limestones, west of New Jersey, where the evidence is rather in favor of, than against, their subsilurian age. But Prof. Rogers's synthesis of Appalachian geology was on too grand a scale to permit him to overlook *as a whole* and *in the main* the mutual relationship of the Silurian and Azoic Systems. The words which I have quoted above are immediately followed by others showing how clear and large a view he took of the subject, a view which the Canadian geologists are only illustrating:—

"As the same strata [the Silurian], moreover, hold a similar relation to our primary rocks throughout their entire range, from Vermont to Alabama, separated from them by no other group of strata yet discovered, claiming an earlier origin, I have deemed it expedient, for the sake of classification, to confer upon them the title of the *Older Secondary* strata of the United States . . . . with the synonyme of the *Appalachian System of Strata*." (p. 44.)

On page 12, Rogers designates the Highland rocks as the "*Gneiss system*," and, on page 13, carefully distinguishes them from the Staten Island, Trenton, Philadelphia rocks, on the south, as he subsequently distinguishes them from the Potsdam, Trenton, and Hudson river Silurian on the North. On page 11, he gives his column of Formations, and places the "*Primary Rocks*" of the Highlands, underneath No. 6, of the "*Lower Secondary Rocks*;" "a white quartzose sandstone, somewhat coarse and friable: occurs only in a few localities"; and pages 12 to 43 are devoted to a Chapter on these rocks, under the heading "*Primary Rocks of the State: Geology of the Highlands*."

In fine, no geologist, even of small capacity, could study the New Jersey Highlands, the Durham Hills of Eastern Pennsylvania, and their continuations in the South Mountain and Blue Ridge, and even conceive the suspicion, much less publish it as a conviction, that their strata were metamorphic representatives of the Silurians. We know very well that to say that Rogers thought or said such a bêtisme is to do him gross injustice.

East of the Hudson, there seems to have been made by Rogers, Hall, Logan and many others, all, in fact, except Emmons, a gross mistake of this kind, owing to peculiar circumstances, principally the presence of a great fault of unknown size and location, secondly, by the obliteration of the usual fossils from the top-slates ("Hudson River") of the Lower Silurian System, and thirdly, by a serious change in the Lower Silurian group itself, viz.: its expansion in thickness toward the northeast, which made it at first to be confounded with the true Highland gneiss-range running behind it. But in Southern New York, New Jersey, Pennsylvania, and farther south, no such mistake was possible, and at least was never made by Rogers. In fact he had enjoyed such advantages for studying the phenomena of this very belt that he was just the last man in the world who could have made it.

I think the above reclamation is due, in his absence, to Professor Rogers, whose untiring energy and devotion to Appalachian geology did so much to lay fundamental views, which our Canadian friends will find it beyond their power to do more than illustrate. I wish they would try their hand at bearing the veritable *crux criticorum* of our geology, the age and relationships of the Philadelphia-Baltimore belt. There is also something to do about *our* Potsdam sandstone. For as to the patches of so-called Potsdam on the North face of the Highlands at Reading—the long ridge of Potsdam crossing the Schuylkill, 15 miles above Philadelphia—the cliffs of so-called Potsdam at Chicques rock, on the Susquehanna, two miles above Columbia—the supposed Potsdam at the anticlinal of the Nittany Valley, crossing the Juniata near the foot of the Alleghanies—I think it is very doubtful, in spite of the *Scolithus linearis* which they contain, whether these correspond *exactly* to the New York bottom layer of the Silurian. The structure of the Chester county valley, of white marble, with a Potsdam barrier on the north and a mica slate and serpentine country on the south, is a great puzzle. If Sir William Logan and Prof. Hall will take *these* in hand they will run small risk of doing injustice to anybody.

At the same time, it is very satisfactory to have a well trained Canadian eye, familiar with Laurentian lithology, after taking a good look at our gneissic mountains, pronounce them certainly Laurentian and not Huronian, and to receive from the same high authority the announcement that the Sillery sandstones and other prominent members of the Quebec group disappear, coming this way, at about the latitude of the Hudson Gap. For we have been looking for their representatives in Pennsylvania, in vain; although Emmons is understood as identifying his Taconic system with the rocks of the southeast side of the Great Valley.

Philadelphia, Jan. 18, 1865.

2. *Skulls of the Reindeer Period from a Belgian bone-cave, indicating a superior, as well as an inferior, race of primitive men in Europe.*—Prof. VAN BENEDEN, the distinguished zoologist of Belgium, in recent explorations in caverns, has found, as he writes to John Lubbock, Esq., of Chiselhirst, Kent, crania of two distinct pre-historic races, of the Reindeer period, and one of them, that least well preserved, "est franchement brachy-

cephale et prognathe, mais avec une belle boite cranienne." The cavern is in the Carboniferous limestone, 30 or 40 meters above the level of the Lesse. A large number of human remains were found.—*Reader*, *Jan.* 7.

3. *Casts of fossils.*—Prof. H. A. WARD, of Rochester, New York, is contributing greatly to the diffusion of a knowledge of geological science through the country, by furnishing casts, made under his direction, of fossils of various kinds, from those of the Megatherium, Dinotherium, Glyptodon, and others of gigantic dimensions, to those of Ammonites, Trilobites, eggs of Æpiornis, Gold nuggets, etc. The casts are remarkably well made, firmly secured against breaking, and of high finish. All institutions in which geology is taught, would find it greatly to their advantage to supply themselves with some of the Rochester casts.

4. *Geological Survey of Canada*, Sir W. E. LOGAN, *Director*. *Figures and Descriptions of Canadian Organic Remains*, Decade II: *Graptolites of the Quebec Group*, by JAMES HALL. 152 pages, 8vo, with 21 plates and many wood-cuts. Montreal, 1865: *Dawson Brothers;* London, New York, and Paris: *Bailliere.*—The first, third and fourth Decades of the figures and descriptions of Canadian Organic Remains, published as a part of the Reports of the Geological Survey of Canada, were issued in the years 1858, 1859, as announced in this Journal in vol. xxvi, at p. 299, and vol. xxviii, at p. 148. The second Decade, just now published, contains an elaborate and extended memoir on the Canadian Graptolites by James Hall. The wonderful variety of these fossils afforded by the Quebec group has enabled Prof. Hall to throw great light on the growth, development and structure of Graptolites. The numerous plates contain full and excellent illustrations of the species. The number of species recognized in the Quebec group is over *fifty*, and none of these are known higher in the series. In the Trenton and Hudson River groups the whole number known is about *thirty*, and in the Upper Silurian there are but *two* (and these occur in the Clinton group), excepting the species of the genus *Dictyonema*, here referred by Hall, of which there are *three* species in the Quebec group, *one* in the Trenton, *three* in the Niagara, *one* in the upper Helderberg and *two* in the Hamilton.

5. *Alger's Cabinet of Minerals.*—The excellent Cabinet of minerals made by the late Francis Alger, of Boston, has been purchased for Alleghany College, Meadville, Pennsylvania.

III. BOTANY AND ZOOLOGY.

1. *Harvard University Herbarium.*—This establishment is noticed in the Annual Report of the President of the University to the Board of Overseers, made in January last, as follows:—

"Dr. Asa Gray has presented to the University his invaluable Herbarium and his Botanical Library; which have been safely transferred to the fire-proof building furnished, at a cost of over twelve thousand dollars, by the generosity of Nathaniel Thayer, Esq., of Boston. A fund has also been raised by subscription, for the support and increase of the collection. . . . The gift of Dr. Gray cannot be estimated in money, but it embraces the results of many years' labor faithfully given by that distinguished botanist, aided by the generosity of his collaborators and correspondents in various parts of the world."

The collections were formally presented by the following letter:—

"Botanic Garden, Cambridge, November 30, 1864.

"To the Rev. Dr. Hill, President of Harvard University,

"*My Dear Sir:*—I have the pleasure to inform you that the Herbarium and Botanical Library, which a year ago I offered to present to the University, are now safely deposited in the building erected for their reception by Mr. Thayer. I have regarded them as belonging to the University from the beginning of the present year: but I wish more formally to make them over to the President and Fellows, as the foundation of the Harvard University Herbarium.

"The Herbarium is estimated to contain at least 200,000 specimens, and is constantly increasing. From the very large number of typical specimens it comprises, its safe preservation is very important.

"The Library, from the rough catalogue which has been made out, contains about 2200 botanical works—perhaps 1600 volumes, and nearly as many separate memoirs, tracts, &c.

"The current expenses of the establishment for the first half of the year now drawing to a close have been defrayed by Dr. Jacob Bigelow, who placed in my hands a special donation of two hundred dollars for this purpose.

"I had stated that the income of a capital sum of $10,000 would be required to defray the current expenses of the Herbarium, i. e. for the purchase of certain collections and books not obtainable by exchange, for freights and charges, paper, alcohol, fuel, &c. I am informed that this sum, which Mr. George B. Emerson undertook to raise by subscription, is substantially secured. It is desirable, but probably not at this time practicable, that this endowment should be so far extended as to provide for the services of a Curator, so that I could myself devote valuable time to the prosecution of important botanical works for which I am prepared, and to which I am pledged.

"I have the honor to be, with great respect, very truly yours,

Asa Gray."

We understand that extensive collections of botanical specimens, to be added to the herbarium, have recently accrued. Among them are—

A full suite of Mr. Charles Wright's collections, (about 2500 specimens,) made in Cuba during the past four years, and just now arranged and distributed among botanists.

A very interesting set of plants recently collected, chiefly by Professor Brewer, in the Geological Survey of California under Professor Whitney.

The numerous and important duplicate *Carices*, (and other *Cyperaceæ*,) of the late Dr. Boott, presented by Mrs. Boott; the proper herbarium, set of *Carices* having been bequeathed to the herbarium of the Royal Gardens at Kew.

A large collection of plants of Mauritius and Madagascar, and a continuation of the distribution of the British East Indian herbaria of Griffith, Helfer, &c., presented by the directors of the Royal Botanical Gardens and Herbaria at Kew.

A similar distribution (in continuation) of plants of the Dutch East Indies and Japan, from the Royal Netherlands Herbarium, Leyden, now under the charge of Professor Miquel.

A selection from the Mexican collections of the late Professor Liebmann (Oaks, Ferns, *Cyperaceæ*, &c.); from the Royal Danish Botanic Garden, Copenhagen.

An extensive set of authentically determined plants of Persia, Siberia, and Northern China, from Professor Bunge of Dorpat; and of Algerian plants, &c., from Dr. Cosson.

A set of Mandon's plants of the Andes of Bolivia; acquired by purchase.

A fine general collection of *Algæ*, from Professor Agardh of Lund, authentically named, according to his new *Species Algarum*.

But the most notable accession is the munificent gift which has just been made by John A. Lowell, Esq., of all the botanical books of his own library which the new establishment did not already possess, being chiefly very large, choice, and costly works, such as the Flora Danica, Sibthorp's Flora Græca, Bateman's Orchidaceæ of Guatemala, the Botanical Register, Botanical Cabinet, Botanical Repository, Richenbach's Icones, the large edition of Duhamel, the great works of Jacquin, and others of the same character,—in all 335 volumes, the pecuniary value of which must be reckoned at several thousand dollars.

The building, to which these treasures are consigned, is of brick, upon a granite foundation, with freestone facings, is 32 feet in front and 56 feet deep, a story and a half high, the walls all hollow and ventilated, for greater security from dampness. On the side next the residence of the Professor (with whose private study it is connected by a cold conservatory, 18 feet in length), the wall is entire, except at the private entrance, which is guarded by a double set of wrought iron doors. The room for the herbarium (for greater security against fire) is also separated from the small working-rooms and library and entry in front, by a hollow brick partition, through which passage is had only by heavy iron doors. In the half-story over the front rooms are two other commodious working or store rooms. The principal room, for the herbarium, is about 30½ by 35 feet, and 19 feet high to the vaulted ceiling. It is lighted by a domed skylight, and by a large double window in the north-western end. At the height of about 8 feet, an iron gallery, four feet deep, surrounds the apartment, interrupted only by the large window. The space between the floor and the gallery is completely filled by the herbarium-cabinets, except upon one side of the entrance, where a sort of furnace or stove, of soapstone, chiefly in the front room, comes through the partition wall, and supplies warm air by registers. The walls above the gallery are reserved for a second and similar tier of cabinets, to be constructed when needed. The cabinets, casings, and all the woodwork except the floors (which are of hard pine, bedded in mortar) are of chestnut wood. The building was constructed under the superintendence of the architect, Mr. Edward D. Harris, in a thorough and durable manner, and it is hoped will form a safe and permanent place of deposit for the collections which have been and may hereafter be consigned to it.

2. *Story about a Cedar of Lebanon.*—In the *Edinburgh Review* for October last, turning over the pages of an article on Coniferous Trees, we read, with more of amusement than instruction, a detailed account of the famous Cedar of Lebanon, in the Jardin des Plantes, which, the Review-

er informs us, is "well-known from an anecdote connected with its arrival there."

"It appears that about one hundred and thirty years ago, viz. in 1737, M. Bernard de Jussieu, the celebrated botanist, *when travelling in the Holy Land*, had brought away with him, from among the cedars of Mount Lebanon, a little seedling. Being unprovided with better means of conveyance, *he made a flower pot of his hat, in which he planted it.* He got it safely on board the vessel in which he sailed for France; but tempestuous weather and contrary winds drove the ship out of her course, and prolonged the voyage so much that the water began to fail," .... And the narrative goes on, at great length, to say how the passengers were reduced to the allowance of half a glassful of water a day,—how Jussieu, "all through the lengthened voyage, under the bright sun of the Mediterranean, shared his half glassful of water with his little plant,"—how "his own strength began to sink under the prolonged privation, but he never flinched, and arrived at Marseilles with his own health damaged, but with that of his little plant uninjured,"—how it is said that he came near losing his treasure through the incredulity of the custom house officers, who suspected a contrivance for smuggling jewels or prohibited articles under the roots of the seedling;—how his entreaties and eloquent appeals at length prevailed, and he was allowed to carry his seedling undisturbed to the *Jardin des Plantes*, where it was planted, and became a great and famous tree;—how it grew and flourished until it reached one hundred years of age and 80 feet in height; and, finally, how "in its hundredth year (1837) *it was cut down to make room for a railway, and now the hissing steam-engine passes over the place where it stood.*" (The italics are our own.)

Of course, it is almost unnecessary to tell our readers that Bernard Jussieu never visited the Holy Land, and was not likely, if he had, to come home bare-headed, using his hat the while for a pot; that the fact, or at least the accepted tradition, is merely this, that he brought the seedling Cedar from *England* to Paris in his hat. The story of the voyage from the Levant to Marseilles appears to be an adaptation of one about the three Coffee-plants, which *Antoine* de Jussieu, in the year 1720, sent from the *Jardin des Plantes* to the vessel commanded by Capt. Declieux, who was charged by the French Government with the duty of transporting them to Martinique. The voyage being unusually long, the water is said to have given out, two of the precious plants died, and the remaining one is said to have been kept alive by the devotion of the Captain, who bestowed upon it his own scanty ration of water, and so preserved the ancestor of all the coffee-plantations of the Antilles. For this devotion, we presume, his name is commemorated in the genus *Declieuxia*, of the Coffee Family.

What are the other ingredients of this *pot-pourri* we are unable to conjecture. But the naturalists of the *Jardin des Plantes* may be somewhat astonished to learn that a railway traverses their peaceful grounds, and that a hissing steam-engine runs over the steep little hill upon which flourished, and as they fondly imagine still flourishes, Bernard de Jussieu's Cedar of Lebanon.

We learn from this same article, farther on, that "poor Douglas," the botanical explorer of Oregon and California, "perished at last in a pit-fall set for *bears or buffalos in a North American* forest; and that the *average height* of full-grown trees of the Mountain Redwood, (*Sequoia gigantea*, which they still call *Wellingtonia* in Scotland) is full three hundred feet!

3. *Calluna vulgaris in Newfoundland.*—Mr. Murray, late of the Geological Survey of Canada, and now engaged in a survey of Newfoundland, has brought to Montreal specimens of this plant, which were collected by Judge Robinson, on the east coast of Newfoundland, near Ferryland (lat. 47°, long. 52° 50′), and which are stated to be from a small patch of the plant not more than three yards square.

4. *Preservation of Starfishes with natural colors;* by A. E. Verrill.—Starfishes may be dried, so as to retain their natural colors almost unimpaired, by immersing them in alcohol of moderate strength for about a minute, or just long enough to destroy the life and produce contraction of the tissues, and afterward drying them rapidly by artificial heat. The drying is best effected by placing them upon an open cloth stretched tightly upon a frame and supported a few feet above a stove. Care should be taken not to raise the heat too high, as the green shades change to red at a temperature near that of boiling water. By this process I have succeeded in preserving the delicate shades of red, purple and orange of the species found on the coast of New England, including *Solaster papposus*, *S. endeca*, *Cribella*, *Asteracanthion pallida*, *A. littoralis*, and various other species, specimens of which are in the Museum of Yale College.

The same process is equally applicable to Echini and Crustacea.

5. *A new American Silkworm.*—After numerous experiments, Mr. L. Trouvelot, of Medford, Mass., has succeeded in rearing successfully, and in great numbers, *Attacus Polyphemus* Linn., and in preparing from its cocoon an excellent quality of silk, possessing great lustre and strength, and pronounced superior to Japanese and all other silks, except the best Chinese, by competent judges.

The silk is unwound by a simple process perfected by Mr. Trouvelot, each cocoon yielding about 1500 yards. This insect is very hardy, being found throughout the Northern States and Canada; and, as it feeds upon the leaves of oak, maple, willow, and other common forest trees, may be reared easily in any part of the country.

Mr. Trouvelot has gradually increased his stock from year to year, by raising young from the eggs of the few individuals first captured, until he has at present seven wagon-loads of cocoons, the entire progeny of which he proposes to raise during the coming season.

The thanks of the country are due to the ingenious and persevering author of this successful attempt to introduce a new and interesting field for industry and enterprise, which cannot fail to be a source of profit to those who intelligently engage in it, and of increased wealth and prosperity to the people, should it be developed to the extent that now seems possible.

The first public notice of his experiments with this insect was given by Mr. Trouvelot at a meeting of the Institute of Technology, at Boston, about a year ago, when he exhibited specimens of silk manufactured from it, both natural-colored and dyed. A. E. V.

IV. ASTRONOMY AND METEOROLOGY.

1. *Shooting Stars of Nov.* 11–14, 1864.—Since the last No. of the Journal of Science was issued the Committee on Meteors of the Connecticut Academy of Arts and Sciences have received, by letter from Prof. B. Silliman, dated in California, the following observations obtained by him, while in that part of the country:—

First ; Observations at Virginia, in Nevada, made by Mr. RICHARD H. STRETCH:—*Nov.* 12*th.* One observer. Three hours watch, from 1^h 15^m A. M. to 4^h 15^m A. M. Eighteen shooting stars were observed. Twelve were conformable to the well-known radiant in Leo, although four of the number exhibit large deviations. But the deviations are about equal, on either side of the radiant. Of the number that were not conformable, five had their courses directed from an area, 3½° in diameter, in the same general quarter of the heavens, but far to the north,—in fact, centered about 1° N.W. of β in *U. Major.* Only one—and that of small magnitude—exhibited no tendency from the common radiant. All were yellow, except one that exploded with a beautiful green light. Nearly all appeared as brilliant points of light,—and six of the number were as conspicuous as stars of the first magnitude; also four of the latter were remarkable either for scintillations, or explosion, or broad trains attending them. These four were of the twelve first above mentioned.

Nov. 13*th.*—The same observer. His watch was begun at 1^h 45^m A. M. In one hour and eight minutes twenty-two falling stars were seen through the moonlight. Afterward twenty-four more were observed, during a time not stated, but ending, not improbably, about 4^h A. M.—as on the morning preceding. Excepting No. 22, at 2^h 53^m, all appeared as points of yellow light. Mr. Stretch has illustrated the phases of this No. 22, by three sketches. It exploded like a rocket, its tail dropped, and grew shaggy and broad, it turned from yellow to red, shaping itself, at first, like the horn of an ox, and finally like a corona; or, more nearly like a blunt crescent occupying three-fourths of a circle, with the left hand branch exceeding the other one-half in length, but of much less curvature. The duration was three-quarters of a minute.

It is remarkable that the entire group was conformable to a common radiant; although eight of the forty-six—and only eight—show a large deviation—a part to the east, but more to the west. This radiant, however, was in A. R. 156° and N. Dec. 24°,—or nearly seven degrees of arc eastward by northward of its ordinary situation. This position, and the same of the 12th, are derived from the very neat charts which accompany the observations of Mr. Stretch, and which have every meteor's flight traced clearly and numbered upon them.

Second ; Observations at Shasta, Nevada, made by Mr. G. K. GODFREY. *Nov.* 14*th.*—Time, from midnight until 2^h A. M., certainly, and probably until later. At midnight Mr. Godfrey had his attention attracted, "while riding into Shasta, by the falling of innumerable stars." Of these he counted "some fifty," but the time of counting is not stated. Occasionally a star "shot off to the east with great velocity, leaving a brilliant trail behind. There were some which "cast a bright scintillation of a violet color," another group "were of a deep crimson, and much larger, apparently, than the fixed stars, and described a small arc in the sky, and were confined to a small space in the heavens." These last expressions agree (color excepted) with the striking appearances, in 1833, of a limited space around the radiant; but Mr. Godfrey has not stated, even in general terms, the position of the "small space" which he has described. All that can be certainly inferred from his account is the simple fact that shooting stars appeared, at Shasta, on the morning of Nov. 14th last, in such numbers, and of such brilliancy, as to fix Mr. Godfrey's attention upon them for, at least, two hours without any previous notice or expectation of such an occurrence. A. C. T.

New Haven, Jan. 5.

2. *Meteor and Meteorites of Orgueil.*—On the evening of the 14th of May, 1864, a very bright fireball was seen in France throughout the whole region from Paris to the Pyrenees. Loud detonations were heard in the neighborhood of Montauban, and a large number of stones came down near the villages of Orgueil and Nohic. The passage of the meteor was witnessed by a large number of intelligent observers since it occurred early in the evening. Numerous accounts of its appearance have been published in the *Comptes Rendus.*

This fall of meteorites is of peculiar interest. Whilst we have over a hundred large fireballs and detonating meteors whose paths through the atmosphere have been computed, with more or less precision, there are only four or five of them from which stones have been known to come. Of these four or five only one, the Weston meteor, has been so well observed that we can speak with confidence of its path.

The published accounts show that the Orgueil meteor was first seen at an altitude greater than 55 miles, that it exploded at an altitude of about 20 miles, and that it was descending in a line inclined at the least 20° or 25° to the horizon. The velocity must have been not less than 15 or 20 miles per second. This example affords the strongest proof that the stone-producing meteors and the detonating meteors are phenomena not essentially unlike. H. A. N.

3. *Chemical and mineralogical characters of the meteorite of Orgueil.*—Daubrée, Cloëz, Pisani and Des Cloizeaux have communicated to the French Academy papers on the physical and chemical characters of the Orgueil meteorite. In outward appearance it resembles an earthy lignite. The dark mass contains minute grains of a bronze yellow substance having a metallic lustre and a high density, which permits its easy separation from the main portion of the meteorite by levigation; observed under the microscope, these particles are seen to be distinct hexagonal tables; they are strongly attracted by the magnet and have all the physical and chemical properties of magnetic pyrites. A very marked characteristic of the meteorite is, that when placed in water it falls to powder, and a portion of it is in such an extreme state of mechanical division that it remains a long time in suspension in the water, and passes through the closest filter paper. The density of the meteorite taken in pure benzine gave Cloëz 2·567. An analysis by this chemist shows it to contain a considerable amount of magnetic oxyd of iron, besides silicates, protosulphid of iron, traces of nickel and chromium, 5·92 per cent of carbon

(probably of the graphitic form), 9·06 p. c. of water and 5·30 p. c. of matter soluble in water, consisting of chlorids of ammonium, sodium and potassium, and sulphates of magnesia, soda, etc. It is decomposed by chlorhydric acid with evolution of sulphuretted hydrogen giving a greenish yellow solution and leaving a black residue amounting to 7·6 p. c.; this residue when heated with excess of air burns and leaves a gray substance amounting to 2·2 p. c.

Pisani confirms the observations of Cloëz as to the peculiar comportment of this stone when treated with water, and calls attention to its porosity, and to the fact that this accounts for the facility with which the sulphids have become oxydized to sulphates and hyposulphites, and for the avidity which a dried specimen of it has for water. An experiment showed that a specimen dried at 110° C. absorbed 7 p. c. of water in a few hours when simply exposed to the air. Pisani found 3·35 p. c. of matters soluble in water in operating on about 18 grams of the undried substance. This contained hyposulphurous acid 0·48, sulphuric acid 1·40, chlorine 0·08, magnesia 0·30, lime 0·16, potash 0·60, soda, ammonia, etc., and loss 0·77. Alcohol took up 0·37 p. c. which proved to consist chiefly of sulphur. Pisani's tabulated results give for the composition of the whole meteorite:

$\dddot{\mathrm{Si}}$	$\dot{\mathrm{Mg}}$	$\dot{\mathrm{Fe}}$	$\dot{\mathrm{Mn}}$	$\dot{\mathrm{Ca}}$	$\dot{\mathrm{Na}}$	$\dot{\mathrm{K}}$	$\dddot{\bar{\mathrm{Al}}}$
26·08	17·00	6·96	0·36	1·85	2·26	0·19	0·90,

together with chromic iron 0·49, magnetic iron 12·03, nickeliferous sulphid of iron 16·97, water and supposed organic substances 14·91 = 100. This gives for the oxygen ratio of the oxyds and silica 9·98 : 13·90 = 3 : 4.

Pisani proved the presence of magnetic iron by dissolving the mineral in hot nitric acid which decomposed the silicate and the sulphids, and left a black magnetic residue. The nickel was proved to exist as sulphid by treating the substance with sulphid of ammonium, which dissolved out sulphid of nickel. The stone contains 55·60 p. c. of silicates, and taking into account the water determined by Cloëz, the oxygen ratio of the silicate is that of serpentine. If the alumina is due to anorthite it will give 2·42 p. c. of this feldspar contained in the meteorite. Subsequent observations by Des Cloizeaux, Pisani, Daubrée and Cloëz prove that this remarkable meteorite contains minute rhombohedral crystals of a double carbonate of magnesia and iron, and Cloëz obtained a little more than half of one per cent of carbonic acid from a portion of the meteorite operated upon.—*Comptes Rendus*, May 30, July 18, and Nov. 14, 1864.

G. J. B.

4. *Shooting Stars of Jan. 2d.*—On the morning of Jan. 2d, 1865, shooting stars were sufficiently numerous to attract the attention, at New Haven, of those who were not aware that unusual numbers were looked for on that morning.

5. *Discovery of another Asteroid, Alcmene,* (82).—Another small planet was discovered by Luther, at Bilk, on the 27th of November, 1864. It has been named Alcmene. Oppolzer of Vienna gives the following elements of its orbit, computed from observations of Nov. 27th, Dec. 3d, and Dec. 8th.

Epoch Dec. 0·0, Berlin m. t. $i = 3° \; 3' \; 14''$

$M = 308° \; 40' \; 1''$ $\varphi = 11 \; 25 \; 41$

$\pi = 136 \; 47 \; 50$ $\mu = 781''·152$

$☊ = 28 \; 49 \; 28$ $\log a = 0·438181.$

6. *Comet IV*, 1864.—On the 11th of December, 1864, Mr. Bäker at Nauen, near Berlin, discovered a telescopic comet.

7. *Spectrum of a shooting star.*—Herschel has recently observed the spectrum of a shooting star. It appeared near Capella, and was almost as brilliant as that star. He followed it for more than a second in its rather slow motion, and ascertained that its spectrum was as continuous a spectrum as that of Capella, and a little more extended, and, therefore, that it consisted of a solid or liquid substance and not of a gas or incandescent vapor, as Mr. Huggins has suggested with regard to some nebulæ.

V. MISCELLANEOUS INTELLIGENCE AND BIBLIOGRAPHY.

1. *Patent regenerative Gas-furnaces, of* C. W. & F. Siemens.—Professor Faraday, in his lecture at the Royal Institution, on the 20th June, 1862, describes these furnaces in the following terms:—

"The gaseous fuel is obtained by the mutual action of coal, air, and water at a moderate red heat. A brick chamber, perhaps 6 ft. by 12 ft., and about 10 ft. high, has one of its end walls converted into a fire-grate, *i. e.*, about half way down it is a solid plate, and for the rest of the distance consists of strong horizontal plate bars where air enters, the whole being at an inclination such as that which the side of a heap of coals would naturally take. Coals are poured, through openings above, upon this combination of wall and grate, and being fired at the under surface, they burn at the place where the air enters; but as the layer of coal is from 2 to 3 feet thick, various operations go on in those parts of the fuel which cannot burn for want of air. Thus the upper and cooler part of the coal produces a large body of hydrocarbons; the cinders or coke which are not volatilized, approach, in descending, toward the grate; that part which is nearest the grate, burns with the entering air into carbonic acid, and the heat evolved ignites the mass above it; the carbonic acid, passing slowly through the ignited carbon, becomes converted into carbonic oxyd, and mingles in the upper part of the chamber (or gas producer) with the former hydro-carbons. The water, which is purposely introduced at the bottom of the arrangement, is first vaporized by the heat, and then decomposed by the ignited fuel, and re-arranged as hydrogen and carbonic oxyd; and only the ashes of the coal are removed as solid matter from the chamber at the bottom of the fire-bars.

These mixed gases form the gaseous fuel. The nitrogen which entered with the air at the grate is mingled with them, constituting about a third of the whole volume. The gas rises up a large vertical tube for 12 or 15 feet, after which it proceeds horizontally for any required distance, and then descends to the heat-regenerator, through which it passes before it enters the furnaces. A regenerator is a chamber packed with fire-bricks, separated so as to allow of the free passage of air or gas between them. There are four placed under a furnace. The gas ascends through one of these chambers, whilst air ascends through the neighboring chamber, and both are conducted through passage outlets at one end of the furnace, where mingling, they burn, producing the heat due to their chemical action. Passing onward to the other end of the furnace, they (*i. e.*, the combined gases) find precisely similar outlets down which they pass; and traversing the two remaining regenerators from above downward, heat them intensely, especially the upper part, and so travel on in their cooled state to the shaft or chimney. Now the passages between the four regenerators and the gas and air are supplied with valves and deflecting plates, which are like four way-cocks in their action; so that by the use of a lever these regenerators and air-ways, which were carrying off the expended fuel, can in a moment be used for conducting air and gas into the furnace; and those which just before had served to carry air and gas into the furnace now take the burnt fuel away to the stack. It is to be observed, that the intensely heated flame which leaves the furnace for the stack always proceeds downward through the regenerators, so that the upper part of them is most intensely ignited, keeping back, as it does, the intense heat: and so effectual are they in this action, that the gases which enter the stack to be cast into the air are not usually above 300° F. of heat. On the other hand, the entering gas and air always pass upward through the regenerators, so that they attain a temperature equal to a white heat before they meet in the furnace, and there add to the carried heat that due to their mutual chemical action. It is considered that when the furnace is in full order, the heat carried forward to be evolved by the chemical action of combustion is about 4000°, whilst that carried back by the regenerator is about 3000°, making an intensity of power which, unless moderated on purpose, would fuse furnace and all exposed to its action.

Thus the regenerators are alternately heated and cooled by the outgoing and entering gas and air, and the time for alternation is from half an hour to an hour, as observation may indicate. The motive power on the gas is of two kinds—a slight excess of pressure within is kept up from the gas-producer to the bottom of the regenerator to prevent air entering and mingling with the fuel before it is burnt; but from the furnace, downward through the regenerators, the advance of the heated medium is governed mainly by the draught in the tall stack, or chimney.

Great facility is afforded in the management of these furnaces. If, whilst glass is in the course of manufacture, an intense heat is required, an abundant supply of gas and air is given; when the glass is made, and the condition has to be reduced to working temperature, the quantity of fuel and air is reduced. If the combustion in the furnace is required to be gradual from end to end, the inlets of air and gas are placed more or less apart the one from the other. The gas is lighter than the air; and if a rapid evolution of heat is required, as in a short puddling furnace, the mouth of the gas inlet is placed below that of the air inlet; if the reverse is required, as in the long tube-welding furnace, the contrary arrangement is used. Sometimes, as in the enameller's furnace, which is a long muffle, it is requisite that the heat be greater at the door end of the muffle and furnace, because the goods, being put in and taken out at the same end, those which enter last, and are withdrawn first, remain, of course, for a shorter time in the heat at that end; and though the fuel and air enters first at one end and then at the other, alternately, still the necessary difference of temperature is preserved by the adjustment of the apertures at those ends.

Not merely can the supply of gas and air to the furnace be governed by valves in the passages, but the very manufacture of the gas-fuel itself can be diminished or even stopped, by cutting off the supply of air to the grate of the gas-producer; and this is important, inasmuch as there is no gasometer to receive and preserve the aeriform fuel, for it proceeds at once to the furnaces.

Some of the furnaces have their contents open to the fuel and combustion, as in the puddling and metal-melting arrangements; other are enclosed, as in the muffle furnaces and flint-glass furnaces.

The economy in the fuel is esteemed practically as one-half, even when the same kind of coal is used either directly for the furnace or for the gas-producer; but, as in the latter case, the most worthless kind can be employed—such as slack, &c., which can be converted into a clean gaseous fuel at a distance from the place of the furnace, so, many advantages seem to present themselves in this part of the arrangement."

Faraday concludes his lecture with the following conclusive figures:—

"Carbon, burnt perfectly into carbonic acid in a gas-producer, would evolve about 4000° of heat; but, if burnt into carbonic oxyd, it would only evolve 1200°. The carbonic oxyd, in its fuel form, carries on with it the 2800° in chemical force, which it evolves when burning in the real furnace with a sufficient supply of air. The remaining 1200° are employed in the gas-producer in distilling hydro-carbons, decomposing water, &c. The whole mixed gaseous fuel can evolve about 4000° in the furnace, to which the regenerator can return about 3000° more."

2. *National Academy of Sciences.*—The fourth session of the National Academy of Sciences was held at the Capitol in Washington on the 3d, 4th, 5th, and 6th of January of the present year. The three vacancies in the Academy were filled by the election of Prof. O. N. Rood and Gen. M. C. Meigs to the Class of Mathematics and Physics, and Prof. J. P. Kirtland to the Class of Natural History. The following papers were read:

1. On a chronograph for measuring the velocities of projectiles; by J. E. HILGARD.
2. Homologies and classification of the Cephalopods; L. AGASSIZ.
3. Geographical distribution of North American Birds; S. F. BAIRD.
4. On the tables of the Moon; BENJ. PEIRCE.
5. Metamorphoses of some Malacopterygians; L. AGASSIZ.
6. On chemical classification; W. GIBBS.
7. Progress of the Geological survey of California; J. D. WHITNEY.
8. On a method of exhibiting certain statistics of hospitals; J. L. LECONTE.
9. Note on the changes which have taken place in the bar of Charleston harbor since the sinking of obstructions in the main channel, as developed by the U. S. Coast Survey; J. E. HILGARD.
10. Glacial phenomena and present configuration of the State of Maine; L. AGASSIZ.

11. Dimensions and proportions of American soldiers; B. A. GOULD.

12. On a regulator for maintaining uniform motion, and an apparatus for recording time-observations in type; J. E. HILGARD.

13. Mineral lands of the United States, and the relation of the government to their management; J. D. WHITNEY.

14. Origin and formation of sedimentary rocks; J. S. NEWBERRY.

15. Origin and distribution of petroleum in the U. S.; J. S. NEWBERRY.

Alex. Braun, G. B. Airy, Astronomer Royal of England, R. Owen, Prof. F. Wöhler, Sir R. I. Murchison, and M. V. Regnault, were elected Foreign associates. The next session of the Academy will be held at Northampton, Mass., on Wednesday the 23d of August, 1865.

3. *Lawrence Scientific School, at Cambridge, Mass.*—The Lawrence Scientific School has received, as a New Year's gift, (Jan. 1, 1865) fifty-two thousand five hundred dollars, twenty-five hundred to be expended at once for the equipment of the laboratory, and the balance to endow equally the Chemical and Engineering departments.

OBITUARY.—4. Capt. JAMES M. GILLISS, U. S. N.—Captain Gilliss, the Superintendent of the Washington Observatory, died suddenly, at Washington, of apoplexy, on Thursday, the 9th of February. The Naval Observatory, under his charge at the time of his death, was constructed from his plans, and equipped with its original instruments by him, during the years 1843, 1844, Congress having authorized its establishment by an Act passed in 1842; but only since 1861, when Maury, faithless to his country, left his post of duty, has it been under his abler direction. It would have been better for the scientific reputation of the country had it continued in his hands. An earlier Observatory at Washington, fitted up mainly by him, had been the scene of his labors from 1838 to 1842, and in the volume containing the results—the *first* volume of American Astronomical Observations—Mr. Gillis expresses in his Preface, his pleasure that "the prosecution of these observations should have resulted in the foundation of a permanent Naval Observatory."

During the three years, 1849 to 1851, Capt. Gilliss was in Chili, in charge of the U. S. Expedition for determining the Solar Parallax, and if his observations failed of all that was expected of them, it was from the want of that coöperation in the northern hemisphere which was reasonably looked for by him. The National Intelligencer, (Washington) of the day before his death, (Feb. 8,) contains his last astronomical communication—one relating to the Planet Mars—dated Feb. 7.

Capt. Gilliss was an observer of great skill and accuracy, a man of noble personal character, and a patriot in the highest sense of the word. Three of his sons have been in the recent armies of his country, and the eldest—a Captain—reached home from the Libby prison, after four months imprisonment, only the day before his father died.

5. GEORGE P. BOND.—It is seldom that astronomical science has received a more severe blow than that occasioned by the death of George Philips Bond, of Harvard College, Philips Professor of Astronomy, and Director of the observatory connected with that institution. After a lingering illness of more than a year, during which his ardor in the study of the heavens led him oftentimes to exposures entirely incompatible

with the state of his health, he closed a useful and an unblemished life on the 17th of February,—eight days after his compeer, Captain Gilliss.

As an accurate and truthful observer of astronomical phenomena, he was, without question, unequalled by any one in this country, and among the first in the world. In his short career he contributed many valuable papers of original discoveries and calculations to various periodicals and institutions in this and other countries. His greatest work, and that which gave him honor the world over, is his account of the Donati comet which constitutes the third volume of the Annals of the Observatory.. To this, the palm of unrivalled excellence has been freely awarded by the best astronomical observers of Europe.[1] Well trained by his lamented and distinguished father, and taking advantage of the best telescope mounted in so high a southern latitude, he explored with searching scrutiny the great nebula of Orion, a work which he pursued with untiring zeal and anxiety in his latter days; and while we fear his waning strength may have left it incomplete in form, we are assured, and rejoice in the assurance, that abundant ability remains in the observatory to prepare it for publication.

We might dwell much longer on his astronomical history, but the necessary brevity of this notice requires that we should turn to his private life. It is rare indeed, that so many virtues are blended in any man. His innocent unpretending manners, the perfect absence of every air of vanity or pretension, crowned with an unwavering christian faith and deep sense of religious obligation, secured for him, not the mere respect, but the kindest regard of all that had the happiness of his acquaintance. W. M.

6. Dr. Hugh Falconer.—Dr. Falconer, Vice-President of the Royal Society, died at London, on the 31st of January.

7. *The Differential Calculus;* by John Spare, M.D. Boston, 1865. Bradley, Dayton & Co. 8vo. pp. xx, and 244.—The plan of this treatise is different from that of most works upon the same subject. The aim seems to have been to treat the calculus as peculiarly the continuation of Algebra. Hence most of the applications are to problems which resemble as much as possible the ordinary problems of Algebra. The author's style is somewhat involved, and his logic not entirely free from criticism. H. A. N.

8. *History of Delaware County, Pennsylvania, from the Discovery of the Territory included within its limits to the present time, with a notice of the Geology of the County, and Catalogues of its Minerals, Plants, Quadrupeds and Birds;* written under the direction and appointment of the Delaware Co. Institute of Science, by George Smith, M.D. 582 pp. 8vo.—This history is the work of great care and research. The chapter on the Geology of Delaware County is illustrated by a colored map showing the distribution of its rocks, and it closes with a catalogue of mineral localities. The work is illustrated by several plates, wood-cuts and maps.

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[1] We are informed that, a month since, Mr. Bond received word from President De La Rue, of the Royal Astronomical Society, that the Society, at its last annual meeting, in January, had voted him a gold medal for his work on this comet.

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JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XXVII.—*On Molecular Physics;* by Prof. W. A. NORTON.

[Continued from vol. xxxviii, page 223.]

IN a former part of this paper a succinct exposition has been given of a consistent general theory of Molecular Forces, and the Molecular Constitution of bodies, and special theories of the different states of aggregation of matter, and the processes of transformation from one state to another, as well as of the essential nature and modes of excitation and propagation of the two agencies of light and heat.[1] The conclusions arrived at were all deduced from two fundamental principles, viz:

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[1] In a paper on heat-vibrations, by Mr. James Croll, published in the Philosophical Magazine, May, 1864, it is maintained that the heat-vibration does not consist in a motion of an aggregate mass of molecules, but in a motion of the individual molecules; also that it does not consist in excursions of the molecule or atom across centres of equilibrium, but in alternate expansions and contractions of the atom itself. It will be seen that these ideas are in accordance with the conception of the constitution of a molecule adopted at the beginning of the present memoir (p. 62), and with the theory of heat-vibrations, or heat-pulses, deduced therefrom (p. 64). The author remarks that his conclusion that "the ultimate atom itself is essentially elastic, is opposed to the ordinary idea, that the atom is essentially solid and impenetrable, and favors the modern idea that matter consists of a force of resistance acting from a center." But in the present communication the ground is taken that it is the elastic ethereal atmosphere condensed upon the atom that invests it with its panoply of power. Thus armed it becomes an efficient molecule—when associated with a kindred molecule an epitome, in fact, of the universe. In the contractions and expansions that result from the action of the central atom upon its atmosphere is to be discerned the origin not only of heat-vibrations, but of all the molecular forces; and in the varied possible movements and changes of molecular atmospheres, dependent upon their elasticity and mutual action, are to be found the essential causes of physical phenomena.

(1.) That matter exists in the three different forms of *ordinary* or *gross matter*, an *electric ether*, and a more subtile *universal ether;* and that each of these is made up of spherical atoms.

(2.) That there are two primary forces: *attraction*, and *repulsion.*

The primary force of attraction is exerted between the atoms of ordinary matter and the two ethers, and between the atoms of the electric ether and those of the universal ether; while a mutual repulsion subsists between the constituent atoms of the two ethers.

From these two postulates the conclusion was derived that each atom of ordinary matter must be surrounded by two atmospheres, one consisting of electric ether, and another of the universal ether, pervading the former; and that the atoms of the electric ether must also be surrounded by atmospheres of the more subtile universal ether which pervades the space between them. Such being the condition of things, it was assumed that the attraction of the central atom of matter for the atoms of electric ether exterior to it was propagated by the intervening universal ether, and that the same was true of the mutual repulsion exerted between the individual atoms of the electric ether.

The primary force of heat, as one of the molecular forces, was deduced from these principles, and found to have its origin in the force of molecular attraction.

We propose now to show that the characteristic phenomena of electricity, comprised under the several heads of *frictional electricity, voltaic-electricity, thermo-electricity, magnetism, electro-magnetism, magneto-electricity, induced currents*, and *dia-magnetism,* may be derived as mechanical deductions from the same fundamental conceptions.

We have thus far had no occasion to introduce arbitrary hypotheses, but have in fact discarded those hitherto in vogue, as that of the permanent polarity of atoms, and various hypotheses as to the mode by which such polarity is constituted and maintained. It will be seen that in the wide field we are now entering, the same fundamental ideas will suffice, and that the same two forces, attraction and repulsion, operating on the same three forms of matter, are alone concerned in evolving the phenomena. In every different province that we enter we but recognize new results, achieved by the same agencies, working by the same essential processes. As transcendently wonderful as is the infinite variety of Nature, no less so is the all-comprehensive unity of its origin, and the grand simplicity of its evolution. From the point of view now taken, this truth, which has long been discerned with more or less distinctness, stands forth in its full proportions. In the discussion of specific properties of different substances, we have admitted, and shall continue to admit, only differences in degree, not in kind; and differences too, that fall within the scope of the general theory. No higher requirement than this can reasonably be exacted of any fundamental conception.[*]

Electricity.

Preparatory to the discussion of this topic it is important to inquire more minutely than has hitherto been done into the electric condition of molecules, both simple and compound. Each simple atom, as we have seen, is surrounded by an electric atmosphere; but has this atmosphere a definite limit? and if so what is the condition of the electric ether exterior to it, and in the interstices between molecules? The equilibrium of such an atmosphere is determined by the operation of two antagonistic forces, the attraction of the central atom for each atomette of the atmosphere, and the repulsion experienced by such atomettes from all the others of which the atmosphere is composed. If we regard these forces as in no degree intercepted in their propagation, the atmosphere will extend with decreasing density to that height at which the attraction exerted by the central atom upon an atomette is just neutralized by the repulsion exerted upon the same by the entire atmosphere. The centers of repulsion and attraction will also be coincident, and the law of variation of the two forces will be the same; hence beyond the limits of the atmosphere there will be no effective action exerted by the molecule upon any electric ether that may be posited there. But the notion that there is no interception of force does not accord with the fundamental idea of the propagation of force by the universal ether (p. 238). It is impossible that a propagated force of repulsion should take effect upon an atomette of electric ether, unless the universal ether, which is the medium of propagation, be in some degree condensed upon the surface upon which the wave-force falls. Such condensation must give rise to a reflex wave, and the dispersion of a certain amount of force into the surrounding ether. Upon the principle of the conservation of force the amount of force thus dispersed must be abstracted from the original wave-force. Now if the atomettes of the electric atmosphere intercept a certain portion of the repulsion propagated from other atomettes, it follows that the center of repulsion of the whole atmosphere cannot be coincident with the center of attraction, or center of the atom of matter. It would seem also that the repulsive pulses propagated from the atomettes should be more or less intercepted by the central atom of the molecule, which should tend to displace the center of repulsion still more. For each side of the molecule this center must lie somewhere between the center of the atom and the surface of the atmosphere, as at *r*, fig. 4. In this state of things the atmosphere will have a definite limit, as before; but beyond its limits, since its repulsion must decrease more rapidly than the attraction, an effective force of attraction will be exerted by the entire molecule. As a consequence, the external electric ether will be retained in contact with the atmosphere, and press upon its surface with a certain force. Its density will decrease outward and doubtless become insensible at sensible distances. If then we confine our attention to a single molecule, we perceive that it must consist of an atom of ordinary matter surrounded with two envelopes of electric ether; of which the outer presses upon the inner, and extends indefinitely, but becomes evanescent at a distance a certain number of times greater than the diameter of the inner envelope.[1] Hitherto in speaking of the electric atmosphere of a molecule, we have alluded only to the inner and principal envelope, and have regarded the diameter of a molecule the same as the diameter of this envelope; and we shall use these terms in the same sense hereafter, unless the exception is distinctly specified.

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[*] The author may seem to have adopted, in the scheme of molecular forces presented on pp. 63, 64, an arbitrary hypothesis, in assuming the existence of a force of molecular repulsion between the surfaces of contiguous electric atmospheres; as the grounds upon which the theoretical inference was drawn were not stated in the course of the discussion. There will soon be occasion to offer these in another connection.

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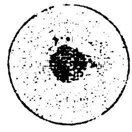

The outer electric envelopes of molecules serve to establish an electric communication between them, and play an important part in all electric phenomena. By their pressure upon the inner envelopes, or the atmospheres so-called, of the molecules, they develop a force of electric repulsion exerted outward at the surface of each atmosphere. This is the force already recognized as one of the molecular forces (p. 64). It has its immediate origin in the compression of the atmosphere at or near its surface, by the outer envelope, which increases the repulsive action of the upper portion of the atmosphere, and so brings into operation an effective repulsion at this surface, where otherwise the effective force would be zero. It is to be borne in mind that the attraction of the central atom is dynamical in its effects, and that attendant upon its exertion the outer envelopes will have an inward and outward movement; but such alternate movements will, when propagated outward, neutralize each others' effects, unless a secondary force is developed in the process. Now it is precisely such a force that is developed in the manner just explained, and this must be propagated by the electric ether of the outer envelope, or the interstitial electric ether of bodies, as a wave-force.[4]

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[1] It is possible that in some cases the outer atmospheric envelope may be made up of several spherical layers, separated by surfaces of no effective molecular action.

We have seen that the attraction exerted by the atom upon its atmosphere, proper, by forcing outward a portion of the universal ether near the surface of the atom, develops at the same time an effective molecular attractive force and what we have called the force of heat-repulsion. From the difference that obtains in the circumstances under which these forces and the above mentioned force of electric repulsion originate, it is highly probable that their constants (n and m, p. 68,) would be different.

To pass now to the case of a *compound molecule:* let a and b (fig. 5) be two similar molecules, held in equilibrium by their mutual actions, and let us enquire into the condition of the electric ether on the line crossing perpendicularly the line of their centers, at its middle point, m. At m the attractions of the two molecules for an atom of electric ether there will neutralize each other, but at all other points of this line, the resultant of the attractions will have a finite value and be directed inward toward the line of the centers. This resultant will increase in value as the distance from the line of the centers increases, to a certain point, n, beyond which it will decrease. The sum of all these resultants will act as a compressing force upon the electric ether about m, and determine its density at that point. At other points, as e, r, s, variously situated in the vicinity of the molecules, the joint attraction of the two will generally exceed the attraction of either molecule alone. It follows, therefore, that the two united molecules will be surrounded by an electric atmosphere of their own, spherical or spheroidal in form, and at the same time that the two individual atmospheres will be materially modified. This atmosphere will have an outer envelope, as in the case of a simple molecule.

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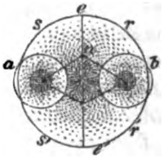

If we suppose several molecules thus united, the entire group will have its own proper atmosphere. The extent of this atmosphere will depend upon the normal attraction at the surface of the group; and this will also determine the density of the electric ether in the interstices between the molecules.[5]

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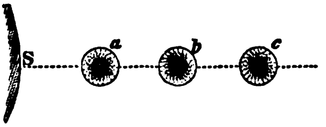
[4] The pulses of heat that are received from extraneous sources, and pass from molecule to molecule, augment this force of atmospheric repulsion. Accordingly, the value of the "constant," m, of this force (p. 67,) depends not only upon the attractive force, size, and perhaps other peculiarities of the central atom, but also upon the amount of heat received from all extraneous sources. External forces, of compression or extension, applied to a body, also tend to augment or diminish the value of m.

[5] In treating briefly of solidification (p. 209, &c.), our attention was confined chiefly to the union of simple molecules in one homogeneous mass, but there is good reason to believe that in most cases of solidification, with the exception, perhaps, of that of perfect crystallization, compound molecules are first formed, and that these combine, in various modes, to form the solid. In every such instance the formation

Let us next inquire into the true nature of the *electric polarization* of molecules.

A molecule becomes polarized whenever, from any cause, a portion of its electric atmosphere is urged around from one side to the opposite side; where there is an excess, the polarity being *positive*, and where there is a deficiency, *negative*. But it is especially important to observe that the act of polarization is distinct from the result. The one is dynamical, the other statical. The act of polarization, too, does not consist simply in a flow of a certain amount of electric ether from the one side to the other of the atom; in addition to this, a certain quantity of the ether is detached, or expelled from the atmosphere on the positive side, and a corresponding amount absorbed into it on the negative side. Accordingly, *when a molecule is being polarized, or when its polarization is increasing, there is a flow of electric ether toward its negative side, and away from its positive side. On the other hand, when a polarization once acquired is falling off, there is a flow, or an electric current, in the opposite direction.* To make this more evident, let S (fig. 6) be a surface receiving a charge of positive electricity, and *a*, *b*, *c*, &c., a series of particles of a dielectric medium. The repulsion of the positive charge on S will polarize the nearer side of *a* negatively, and the farther side positively; but the same force will urge the interstitial ether toward *a*, and the augmented attraction of the negative side of *a* will add a certain portion of it to the atmosphere of *a*. At the same time, the excess of electric ether accumulated on the farther side of *a* will act repulsively upon the upper portion of its atmosphere and expel a certain portion of it. It will also urge toward *b* the interstitial electric ether between *a* and *b*, and polarize *b*, just as S has polarized *a*. The chain of particles will therefore become polarized in succession; and at the same time there will be an electric movement, a dis-

6.



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of such groups of molecules should be attended with the evolution of heat. This will be the inevitable result of the compression of the electric ether between the combining molecules (fig. 5); whether the individual atmospheres be compressed or not.

The heat of combustion and of chemical union generally, has, in all probability, a similar origin; that is in a compression of the electric ether between the molecules, or in such a compression combined with a contraction of the individual atmospheres. This topic, and the probable cause of the different effects observed in different instances of combination, will be briefly considered under the head of chemical union.

The varied forms of crystallization assumed by different substances, under similar circumstances, are probably due, in a great degree, to the diversity that obtains in the number and grouping of the individual molecules of which the ultimate compound molecules consist.

charge from one to another. To take a more complete view of the matter, the molecular polarization resulting from the electricity received by any single point, S, will not be confined to the normal line *a*, *b*, *c*, but will extend, though with diminished intensity, along lines radiating outward from S. A similar remark may be made of the flow of electricity that accompanies the polarization.

If we take account of the entire spherical surface, S, and suppose it to be surrounded by a dielectric medium, as the air, extending to an indefinite distance, each new addition to its positive charge will develop a wave of increasing molecular polarization, which will be propagated in all directions outward through the surrounding medium. This wave, if it may be so termed, will be accompanied by a spherical electric "wave of translation" that will spread indefinitely through the same medium. This latter wave will consist of a series of discharges from one spherical layer of particles to the next; while the former will consist of propagated movements confined to the atmospheres of the particles;—these movements being produced by the repulsive action of the electric ether accumulating on the farther sides of the particles, transmitted primarily through the universal ether. The quantity of electricity that moves forward from one layer of particles to the next, should be equal to the quantity received by the surface, for the determining cause of the electric wave of translation lies in the fact that the force of tension of the propagated electricity urges this quantity between the surface and the first layer of the medium.

The tension of the electricity received results from the compression produced by the expansive action of the charge, which is ultimately in equilibrium with the force by which the disturbed atmospheres of the medium tend to return to their original condition. The tendency of the free electricity to escape between and around the polarized molecules of the medium is counteracted, as will soon be seen, by a resistance developed by the polarization.

This we conceive to be the true process of *induction*. It will be readily perceived that it is in entire conformity with the experimental results obtained by Faraday, and in accordance with his theoretical ideas of the general nature of the process.

We have supposed the electrised surface to be surrounded by a single dielectric medium, extending indefinitely; but the result would be essentially the same if the waves were propagated through several such media, as air, glass, &c. If we suppose the first medium to be replaced by another, while the entire quantity of electricity propagated by it, as the polarization of the medium goes on, should remain the same, as Faraday has shown that it actually does, it does not follow that the final degree of polarization received will be the same as before. In this respect substances may differ, or they may have different "specific inductive capacities," as maintained by Faraday. It is, in fact, just this difference of property that constitutes the difference between conduction and non-conduction, or between the different degrees of conduction or of non-conduction.

So long as the charge of electricity on the surface S, fig. 6, remains the same, the degree of polarization of the surrounding medium, or media, will continue the same; but if the charge be drawn off, waves of decreasing polarization will be propagated outward from S, which will be accompanied by electric waves of translation flowing inward toward S. The entire process that went on within the medium while the body was receiving its charge, will now be exactly reversed.

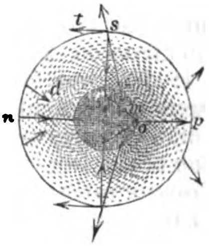
If we suppose a body to receive a charge of negative electricity, the process will also be reversed; as when a charge of positive electricity is withdrawn. The final result will be that the particles of the surrounding dielectric medium will be positively polarized on the side toward the electrised surface, and there will be a deficiency of electric ether between the surface and the first layer of the particles of the medium.

Conduction.—We have seen that the act of polarization comprises two different processes: a movement of a certain portion of the ether of the atmosphere of a molecule from one side to another, and cotemporaneously with this a discharge of ether from the one side, and an absorption of a corresponding quantity upon the other side. Now the electric constitution of two different bodies may be such that, with the one the same amount of electric movement, from particle to particle, may be accompanied by a feeble degree of polarization, and with the other by a much greater degree. The polarization is a change determined originally by the forces propagated by the universal ether, while the outward flow of the electricity is partly the result of the mutual repulsion between contiguous atoms of the electric ether.

Let us suppose two extreme cases, compatible with our general theory. (1.) Let there be an indefinite line of molecules; such that the density of the ether between them is the same as at the surface of the molecular atmospheres, and suppose a quantity of free electricity to arrive at one end of this line. It is plain that the repulsive energy of this electricity will urge before it a certain quantity of electricity throughout the whole length of the line, and that if the flow be unchecked it can exert little or no disturbing action upon the molecular atmospheres except very near their surface, and cannot polarize them. To do this the moving electricity must have a certain decided tension; that is, be present in a certain sufficient quantity at each point of the line. This would be the case of perfect, or nearly perfect conduction, and there would be little or no polarization of the molecules. (2.) Let us suppose that the density of the electric ether is very feeble between the molecules, there being now no continuous mass of ether to give way before the electricity received at the end, the latter will, as a first effect, exert its repulsive action through the universal ether upon the atmospheres of the nearer molecules and polarize them, and this will extend through the line, and the polarization will determine the discharge in the manner already explained. This would be the case of a non-conducting dielectric medium. In all actual instances of good conduction the case lies between these two extremes. The greater part of the electric movement is confined to the outer envelope of the atmosphere, and the outer portion of the inner envelope; and the polarization resulting from the disturbance of the lower envelope, or the atmosphere proper, is slight. With non-conductors, or poor conductors, the flow near the surface of the lower envelope, around the molecule, resulting from the direct repulsive action (or elastic tension) of the electric ether, is comparatively slight; and the polarization of the lower envelope, with the attendant discharge on the farther side and absorption on the nearer side, is much greater.[6]

To obtain a more distinct idea of the entire process of imperfect conduction, we must take account of a certain resistance which a polarized molecule offers to the flow of ether around it from the negative to the positive side.[7] Let *a*, fig. 7, be a polarized molecule. The center of repulsion of its electric atmosphere will fall at some point, *o*, on the plus side of the center. If we enquire into the effective action of the molecule upon a mass of electric ether posited at *s*, midway between *p* and *n*, if the atmosphere were not disturbed the center of

7.



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[6] In the comparison made on p. 216, between the propagation of heat and the propagation of electricity, it is intimated that each is promoted by a polarization of the molecules. This is true only in a limited sense. The circumstances most favorable in general to a conduction of either electricity or heat, are a uniformly dense condition of the interstitial electric ether, which may serve as a medium for the direct propagation of the waves, or pulses, and such a condition of the electric atmospheres of the molecules that the resistance developed by any polarization that may ensue, shall be comparatively feeble. (See p. 246). When, however, the direct conductive action, by the intervention of the interstitial electric ether, is feeble, a greater facility of polarization on the part of the individual molecules would promote the discharge from molecule to molecule.

[7] The terms negative and positive, as here used, have reference to the polarization simply, which is a change in the distribution of the electric ether in the lower portion of the atmosphere. It will be seen in the sequel that in the upper portions through which the free electricity moves, when a conductive discharge occurs, there will be an excess of electric ether, or a positive state, on the side of the molecule which is negatively polarized, and a deficiency or a negative state on the opposite side.

effective repulsion would fall at *r*; in its supposed polarized condition, therefore, it will lie at some point *m* intermediate between *r* and *o*. The two forces exerted upon *s* would therefore act in the direction shown in the figure, and their resultant would act from *s* toward *t*. It will be readily seen that at all points between *p* and *n*, the resultant will have a tangential component directed toward *n*, and a normal component acting outward on the positive side, and inward on the negative side of the molecule. The density of the electric ether at *n* should therefore be greater than at *p*; and while by its elastic tension it is urged around toward *p* it is resisted by the tangential force just spoken of. And so if the electric ether above *n* is urged by the repulsion of free electricity on that side of the molecule around toward *p*, it has to overcome this resistance. It is to be observed that the attractive or compressing action about *n*, is but the result of the repulsive energy of the free electricity which has determined the polarization.

The outer circle in the figure represents the surface of the undisturbed atmosphere (inner envelope). When the molecule becomes polarized the surface of its atmosphere takes, or tends to take, the spheroidal form, with the center of figure lying on the side negatively polarized. At points near this surface, about midway between *p* and *n*, and indefinitely beyond the surface, there will be a resultant force, as shown in the figure, tending to check the flow of free electricity around or past the molecule, in the direction of *n* toward *p*.

It does not follow because the lines of direction of the attraction and repulsion meet at *s* under a small angle that the resultant *s t* may not be equal to, or greater than the force of tension of the free electricity that may reach the molecule, since the absolute intensities of these forces must be very great. Besides, whatever may be the intensity of this resultant, it must be of a corresponding order of magnitude with the tension of free electricity, since it is precisely the resistance exerted by the polarized molecules of air contiguous to an electrised surface, that determines the tension of the statical electricity collected upon it.

When a discharge takes place through an imperfect conductor, as rarefied air, the contiguous particles of air are first polarized by the repulsion of the electrised surface, propagated by the universal ether. This determines the escaping electricity to fall upon their nearer negative sides; the molecular resistance considered above then comes into play, and if the elastic tension of the electricity is sufficient to overcome this resistance, a conducting discharge takes place; otherwise the electric movement can only be after the manner explained on p. 243, that is, as in the process of induction. According to these views, the greater resistance offered by dense than rarefied air, is to be explained by the smaller number of resisting particles encountered, together with the diminished resistance of each particle. For it will be evident, on refering to fig. 7, that if the molecular atmosphere expands, as it will if the air be rarefied, the resistance at *s* will be diminished. Thus air rarefied to a certain degree should become a conductor of electricity. For a similar reason the surfaces of solid, or liquid bodies, should conduct electricity better than the internal portions, since the molecular atmospheres are more expanded there.* It will also be observed, in view of the results obtained, that imperfect conduction and non-conduction depend upon certain effects resulting from the polarization of molecules, and that non-conductors may become imperfect conductors if the tension of the charge presented to them be sufficient. But it should not be overlooked that the amount of polarization induced, and therefore of the resistance resulting from it, must depend upon the condition of the interstitial electric ether (p. 243), and the distance between the molecules; since the interval of time in which the repulsive impulses propagated by the universal ether, may come into action before the electric ether impelled from the one molecule to the next shall begin to take decided effect, must depend upon these two particulars. The quality of conduction, or non-conduction of a substance, should therefore depend to a certain degree upon the mode of aggregation of the particles; more especially as different groups of particles, or compound molecules (p. 241), may offer different degrees of resistance.

If an insulated metallic ball, *a*, be placed in good conducting communication with the ground and afterward with the charged prime conductor of an electrical machine, the electricity will flow freely along the route thus opened, and the tension of the ether passing over *a* being very feeble there will be no perceptible outward movement from *a* through the surrounding air, and no sensible polarization of the particles of air; but this will no longer be the case if the charge be too large to be conducted off with facility. The effect of connecting the prime conductor with the ground when the machine is worked is to allow a free escape to the ether, which would otherwise pass toward the first layer of air particles, and displace an equal quantity from each layer in succession and transfer it to the next.

Electric Spark.—If we suppose the insulated metallic ball, *a*, still connected with the prime conductor, to be placed near another ball, *b*, which is in communication with the ground, on working the machine the ball *a*, in receiving its charge, will give rise to electric waves proceeding outward from it (p. 243), and at the same time polarize the particles of the surrounding air. As a consequence, a certain portion of the electric fluid on the nearer side of *b*, will be urged away and pass into the ground. This side will thus be electrised negatively by induction, and react upon the air particles between the two balls, increasing their polarization, and upon the nearer parts of *a*, drawing more electricity to them. All these changes will be attended with increasing wave movements, and increasing discharges from one particle of air to another along the line between the nearest points of the balls. The electric ether in transitu between the air particles on this line, may thus come to have sufficient density to establish a certain degree of conducting communication between the two bodies, and so to convey a sudden conductive discharge along this line. This result will be partially due to the lateral expansion, which the free electricity received by the air particles on the line will occasion in their electric atmospheres. There will be two causes in operation to produce this effect, the pressure of the stream of ether passing from one particle to the next, against the atmosphere upon which it falls, and the mutual repulsion of the particles that will thus become momentarily overcharged. We have seen (p. 247) that such lateral expansion would give rise to a diminution in the resisting force of the polarized molecules.

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* Surfaces of non-conductors resist the flow of free electricity after the same manner as the internal mass, that is, by their molecules becoming polarized.

As the positive electricity which thus passes over to *b* spreads over its surface, it partially neutralizes its negative state, and thus tends to check the flow and interrupt the passage of the spark. There is also a sudden diminution in the tension of the electricity received by *a* from the prime conductor, which is another cause of this interruption. Apparently another cause conspiring with these two is a reaction to the sudden lateral expansion above mentioned. If *a* were previously charged and insulated, and *b* brought continually nearer to it, the mutual inductive action of the two balls upon each other would initiate the electric movement above alluded to through the intervening air, which would finally result in the passage of a spark.

The light of the spark results in part from the vibratory movements in the atmospheres of the air molecules, attendant upon the discharge. (See p. 219.) Experiments by Riess, Masson, and other physicists, have conclusively established that the electric light is partially due to the passage of highly luminous metallic particles from the positive to the negative surface. The detachment of such particles, as one result of the discharge, may be explained by the discharges, or flow of electricity that must take place directly through the ball *a*, when the conductive discharge from *a* to *b* occurs. This follows from the fact that the electricity within the ball is no longer in equilibrium under the action of the electrical shell that surrounds it, and the impulses conveyed by the electric current should tend to detach the par ticles at the end of the metallic line through the ball. The luminosity of the detached particles is to be ascribed to the vibratory movements imparted by the discharge to the electric atmospheres of the particles.

Excitation of Electricity.—There are various special modes of exciting electricity, but they are all only so many different modes of polarizing contiguous molecules; or, more comprehensively, of effecting certain disturbances in the equilibrium of electric atmospheres. The different special causes of disturbance in these cases may all be traced to some action of the molecular forces, or of heat; and, as we have seen (p. 64), heat is also, in its primary origin, one of the molecular forces.

Frictional Electricity.—The surface of one body should not in its natural condition exercise any sensible direct electric action upon that of another, unless they are brought into close proximity; certainly much nearer than in any ordinary case of mere contact. (See p. 240 and Table I.) When, however, two dissimilar surfaces are *pressed* together, especially if they are brought within the limits at which a force of adhesion would come into operation, the unequal molecular forces of the dissimilar particles, might, as will be seen in another connection (p. 250), give rise to a polarization of the particles. The development of such polarization should establish a wave movement entirely through the two bodies (p. 242), and as the result, the surface toward which the flow occurred might have an excess of free electricity while it would be negatively polarized (p. 246); that is, the outer envelopes would, on the outer side of the molecules, have an excess of electricity, and on the inner a deficiency. The reverse would be true of the other surface. This is the probable explanation of the electrical excitement which may be obtained in a feeble degree, by simple *pressure*, as in the experiment by Haüy, of pressing the smooth surfaces of fragments of calcareous spar, quartz, &c., between the fingers. But the process of excitation by friction seems to be different. The most notable distinction to be perceived between the act of rubbing and that of pressure, is, that in the former alone the protuberent parts of the two surfaces are pressed against each other laterally. The probability, then, is that this lateral pressure of the surface particles is the immediate cause of the development of electricity by the rubbing of one surface over another. Now it is easy to see that this pressure should tend to compress the electric atmospheres of the particles laterally, and so to produce an accumulation of electric ether on their outer sides, and a discharge from thence toward the other surface. If the surfaces be dissimilar in nature, or condition, the effects of this sort of action should be unequal, and the result should be that upon the one surface there would be an excess of electricity, and upon the other a deficiency.

The particles of each body, if non-conductors, should also become polarized, after the same manner essentially as already explained (p. 242), and in this condition would serve to retain the surfaces in their disturbed state (p. 245–6). The explanation here given is sustained by the effect of heat when applied to one of the surfaces, which is to dispose that surface to take the negative state, and in fact the heat-pulses should expand the molecular atmospheres (p. 73) and tend to produce a discharge of electric ether from their outer sides. The like tendency of roughness of surface may be explained in a similar manner.

The tension of the electricity obtained should depend upon the degree of polarization which the surface molecules are enabled to retain, for upon this will depend the limit of the resistance which they are capable of offering to the free flow of the electricity from molecule to molecule (p. 246). It should therefore be independent of the velocity, the pressure, and the extent of the rubbing surfaces, as established by Peclet. The electrical state of the surface, whether positive or negative, must depend upon the mutual relations of the surfaces, and the comparative energy of the disturbing force in operation upon each (p. 249).

Voltaic Electricity.—In entering upon this topic, we must first endeavor to obtain, from our present point of view, an insight into the nature of *chemical union.* When two dissimilar molecules, *a* and *b*, are brought into close proximity, the effective action of *a* upon the atmosphere of *b* may be quite different in amount from that exerted by *b* upon the atmosphere of *a.*[*] As a consequence, the relative condition of these atmospheres may be disturbed; in other words, they may become polarized. As the act of polarization proceeds, an electric current, or series of waves (p. 243), will pass through the two molecules, and at the same time they will approach each other. As this approximation continues the density of the electric ether between them will increase (p. 241, fig. 5,) and a true electric or conducting union may thus be established between them. Under these circumstances, it would seem that finally the non-conducting resistance offered by the polarized molecules (p. 245) should be in a great degree overcome, a comparative equilibrium of tension be established between the outer envelopes of the two atmospheres, and, as a consequence, the polarization materially decrease if not ultimately disappear. In this event the final union would be due chiefly, or entirely, to the molecular attraction proper, as in the case of similar molecules. It is in this final condition, essentially, that we suppose a particle of water to exist. The two constituent molecules, oxygen and hydrogen, are not polarized, or but feebly so, and they are, to a certain degree, *in conducting communication with each other.*[10]

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[*] A disturbing action from the molecular attraction, may even come into operation when the molecules are beyond the range of effective attraction, that is beyond Oc, Fig. 1. For it is to be observed that in the action of each molecule upon the atmosphere of the next, the attractive impulses prevail over the repulsive, beyond Oc as well as between Oa and Oc.

In developing the theory of the voltaic current, let us confine our attention to the case of a single cell, consisting of water, or acidulated water, between a plate of zinc and a plate of copper. We must suppose that the first effect is a mutual polarizing action between a molecule of zinc and one of oxygen, the former being brought into the positive state and the latter into the negative state, upon their contiguous surfaces. The attraction thus developed will arrange the oxygen, with its associate hydrogen molecule, on a line normal to the surface of the zinc plate. The farther surface of the molecule of hydrogen will be brought to the same positively polarized state as the zinc plate, and will act in a similar manner upon the next particle of water; and so on from one particle to another until a complete chain of polarized molecules extends to the copper plate.

In this chain, as first established, we regard each particle of oxygen as in the negative state on the side nearest to the zinc, and each associate particle of hydrogen as in the positive state on its farther side; or at least that they are brought essentially into this condition, and that the true polarization of the contiguous sides is comparatively feeble, by reason of the conducting communication between them, resulting from the condensed state of the electric ether by which they are electrically connected (p. 241). Not only does the positive repulsion that originates at the zinc plate establish, by induction, a chain of polarized water particles, in which the farther, or hydrogen side, is in the same positive state as the zinc, but it also tends to increase the density of the electric ether posited between the oxygen and hydrogen of the individual water particles of this chain, and so to urge them asunder.[11] Before the closing of the circuit, while the mutual polarizing action between the zinc and oxygen is in continual operation, waves of positive electricity spread in an indefinite series, from the zinc plate through the cell and all the media on that side of the plate (p. 243). A corresponding series of negative waves spread in every direction through the zinc plate and the media lying without it, the electric movement in these being toward the zinc plate. When the two plates are provided with wires leading away from them, we have evidence of this wave movement, and of the polarization that has attended it, in the positive and negative states of the ends of the wires. Now if the ends of these two wires be brought together, the entire series of waves which pass through the copper and zinc plates are condensed, so to speak, upon the wires, and pass through the circuit. The entire quantity of electricity that would be dissipated from the copper plates is thus brought around to the zinc plate again. The arrival of this electricity intensifies the polarizing action going on at the zinc plate, and hastens the union of the zinc and oxygen molecule. It also determines, if not before established, the completion of the line of polarized water-particles traversing the cell. As soon as this takes place, the waves that before spread through the cell are converted into linear currents. At the same time, the electricity discharged from the zinc to the oxygen passes over by conduction to its associate hydrogen molecule, and by its impulsive and repulsive action urges the latter over to the next particle of oxygen in the chain. This particle of hydrogen, with its charge of positive electricity thus received, acts upon the second particle of oxygen in the same manner that the zinc acted upon the first, and so on throughout the chain. As the detached hydrogen particle is made, by the same force which detached it, to attract the next oxygen particle more energetically, there may be no material movement of the common center of gravity of any of the pairs of particles that are separating or uniting.

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[10] It is not absolutely essential to the explanation of the voltaic current that the two molecules, when combined, should be regarded as devoid of polarization.

According to the received theory of the constitution of a molecule of water, we must regard the molecule of hydrogen that combines with a molecule of oxygen as compound, and composed of two simple molecules (p. 241, fig. 5); but this in no degree effects the explanation to be given, for the compound molecule, as it is not decomposed, comports itself throughout essentially as a simple molecule would under like circumstances. It is to be observed that the process of polarization above considered does not occasion an excess of electric ether upon the entire molecule of the one substance, and a deficiency on the entire molecule of the other; since when a molecule becomes polarized it absorbs upon the one side the same amount of electric ether that it gives off from the other side (p. 242).

In all these remarks the term molecule is used in the same sense as heretofore.

[11] If the constituent molecules of each water particle were not in conducting communication, then the action transmitted along the line would serve to polarize these molecules and thus to bind them more closely rather than to separate them. This objection seems to hold against Schönbein's theory.

The explanation of the voltaic current that has now been given seems to accord with the established laws and phenomena of the current. The primary *electro-motive force* must consist in the energy of the natural polarizing, or chemical action,[12] exerted between the zinc and the oxygen molecule, diminished by any opposing action of the same nature that may be in operation at the copper plate. It follows from the principles of induction laid down on p. 243, that the quantity of electricity in circulation, or the *intensity* of the current, must be the same at all points of the circuit. The period of time which the zinc and oxygen particles occupy in combining should be proportional to the length of the entire circuit, supposing it to be of the same material and cross section throughout; actually should be proportional to the "*reduced*" length of the circuit. During this period all the electricity set in motion by the union of the two particles should pass through the circuit; or more strictly, be urged forward past each point of the circuit, in electric "waves of translation." The quantity of electricity that moves forward in a given time should then be *inversely proportional to the length of the circuit*, other things being the same. The reason that the quantity of electricity, or the intensity of the current, is *proportional to the area of the cross section of the wire* is, probably, that the number of points of the zinc plate which are contemporaneously in action, with the same degree of energy, would be proportional to this cross section. The *tension* of the electricity circulating in the current should be the greatest where the velocity of the individual particles of the ether is the least. Possible retardations result from the electric relations of contiguous molecules in the line being such that they become more or less polarized, and so offer a resistance to the free flow of the electricity (p. 245); besides that the process of polarization is attended with a retardation. The degree of polarization that exists at any point of the current serves as a measure of the "*resistance*" experienced by the current there.

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[12] This chemical action is intensified by the coöperative polarizing action of the sulphuric acid.

If an *electrolyte* be disposed between the ends of the wires, the theory of its electrolysis is similar to that of the decomposition of the water in the cell. The only difference is that the ends of the wires are brought by the electro-motive force into the same positive and negative states, which the natural chemical action in the cell determines upon the zinc and copper plates.

When two or more cells are employed, the natural polarizing action at each zinc plate should be enhanced, and the tension of the free electricity at the ends of the wires of the broken circuit should be augmented. Hence there should be a more energetic force to polarize and decompose an electrolyte interposed between the ends of the wires. But it does not follow that when only good conducting wires are employed to complete the circuit the intensity of the current will be augmented by increasing the number of cells; since the principal retardation of the flow occurs in the cells, and this increases in the same proportion with the number of cells. (See Pouillet, *Elements de Physique*, ii, 732.)

The *heat* developed in the voltaic current is to be ascribed to the impulsive action of the electric ether moving in it upon the universal ether. Currents, or waves of translation, are thus developed in this ether, which fall upon the central atoms of the material molecules in the circuit, or the dense ether surrounding these atoms. The impulses thus received are given off, or pass into the molecular atmospheres, one after another, and are finally radiated off as heat-pulses. The explanation is the same as that of the molecular absorption, and subsequent radiation of the ethereal pulses of radiant heat, already given (p. 215). When the condition of things is such that the particles in the circuit become polarized, a greater amount of heat should be developed, because a part of the electric movement within the molecular atmospheres, which was before confined to their upper portions, now occurs at greater depths, where the universal ether is more dense. Thus, when the resistance to the passage of the current becomes greater, more heat is developed. Heat may also be evolved, under special circumstances, as a consequence of a compression of the molecular atmospheres, produced by the current.

We shall see, in the remaining portion of this memoir, that it is to these same *impulses of the moving electric upon the universal ether* that are to be ascribed all the external actions of the current; as attracting or repelling wires conveying currents in the same direction with the given current, or in the opposite direction,—giving motion to the magnetic needle,—developing magnetic, or diamagnetic currents, in the compound molecules of adjacent masses,—and inducing currents in wires, or metallic bodies, in the vicinity.

[To be continued.]

ART. XXVIII.—*On the combination which takes place when Light of different tints is presented to the right and left eye;* by Prof. OGDEN N. ROOD, of Columbia College.

IN 1806, de Haldat stated that when differently colored glasses were held before the two eyes, a combination of the two tints took place in the brain, and that the resultant impression was the same that would have been produced by mixing the two tints together, and presenting the compound color to a single eye. These experiments were repeated by many good observers without success. In 1841, with the aid of a stereoscope and polariscope, Dove confirmed the general correctness of de Haldat's conclusion. In 1846, Seebeck, and, in 1849, Foucault and Regnault arrived at the same result.

The testimony of these observers has then proved that a combination of the two sensations does take place in the brain, and that the resultant impression is, in the cases they examined, generally similar to that which would be produced by the presentation of the two tints to a single retina: but with what exactitude the resultant tint obtained by the binocular method agrees with that produced in the ordinary way by rapid rotation, or by

Helmholtz's method, or how far it is in point of fact possible to predict, by the binocular method, what the result of the true mixture of different tints willl be, is a question which, so far as I know, has never been studied.

The following set of experiments was undertaken for the examination of this matter.

The first method pursued was as follows: Light of different tints, complementary or not, was presented to the right and left eye, and the resultant tint gained by binocular vision was noted: afterward, by the method of rapid rotation the two tints were mixed and presented to a single retina. In many cases the results obtained by the two methods were nearly identical, but in others there was found to be a considerable variation, so that often it was impossible to predict by the binocular method what the exact resultant by rotation would be; it sometimes being of a tint which was not at all present to the eye or mind during the binocular union.

An open lenticular stereoscope was provided with a white card board, in which were cut two square apertures one half an inch in diameter, they being placed at such a distance apart that their binocular union readily took place. The ground glass was removed, and the stereoscope held horizontally in front of a rotation apparatus to which was attached a circular card-board disc, with its two halves painted in different colors. The disc remaining at rest, the dividing line of the colors being vertical, light of different colors entered the two apertures in the card-board, and after their binocular union had been effected, the resultant was noted: the disc was then set in rotation, the stereoscope remaining in its position, and a true mixture of the two tints was obtained and compared with the first result.

Helmholtz has shown that vermilion represents the red of the solar spectrum up to the line C, that red lead answers to a portion between C and D, but not reaching up to D; also, that the pure yellow portion of the spectrum is imitated by the chromate of lead, the less refrangible blue by Prussian blue, and the more refrangible blue by ultramarine. Accordingly discs were prepared with these substances, and, in addition, some others were used: viz.: a disc colored with a bright and pure yellow, a little more refrangible than chrome yellow, but still not at all greenish yellow—its tint was about that of 107·7, in Kirchhoff's chart: for the production of a greenish yellow disc, gamboge, with a minute portion of Prussian blue, was used—its tint was that of 113·2; more of the latter color, with gamboge, gave a good greenish-blue disc. Red discs, slightly purplish, were used; also discs colored with emerald green, 170· in the chart. In addition discs of a golden yellow, being some of the same paper I had previously used in the stereoscopic imitation of the luster of gold—its tint was 102·7; and finally purple discs were made by the mixture of ultramarine and crimson lake.

Comparison of the results obtained by the binocular and actual combination of different tints.

1. When one-half of the disc was covered with vermilion, the other with emerald green, the binocular combination was rather difficult, and the result was sometimes a red gray, sometimes a green gray; but when the disc was set in rotation, its true resultant tint was found to be a decided green, which could not have been predicted from the binocular union. That red and green should give a decided green by rotation may seem singular, but it must be remembered that I was not able to vary the *proportion* of these two tints, *each half* of the disc necessarily being of the same tint. In this case then, it merely happened that the intensity of the green was such that after a portion of it had been neutralized by the red, enough remained to give a strong green coloration to the disc. Similar results, given below, will be passed over without notice: they really furnished a delicate test for deciding whether the binocular resultant agreed with that produced by rotation, *preconceived notions* of complementary colors being thus avoided, as it was impossible to know beforehand what the result would be, when the rotation experiment was tried with *equal* surfaces of the colored papers. In several cases the tints on equal portions of some of the discs nearly exactly balanced each other, so that the resultant tint by rotation approximated to a white or neutral gray.

2. Vermilion and ultramarine gave with some ease the *same* tint by the two methods of combination; viz., a red purple: the tint was however rather more red by rotation than would have been expected from the binocular combination.

3. Vermilion and yellow (107·7) gave by rotation a tint containing much more yellow than would have been expected from the binocular examination.

4. Vermilion and purple gave the same tint in both cases, viz., a red purple.

5. Vermilion and black gave approximately the same result in the two cases; the disc was however less red by binocular vision than by rotation.

6. Vermilion and white gave the same result in both cases.

7. Orange, (red lead,) and purple gave a good approximation to the same result in the two cases; the tint was more red by rotation than was expected.

8. Orange and black gave a more neutral tint by binocular vision than by rotation.

9. Orange and white gave the same tint.

10. Chrome yellow and emerald green (170·) gave the same result, viz., a yellowish green.

11. Chrome yellow when combined with black by rotation gave a more decided yellow than would have been expected from the stereoscopic union of the two components.

12. With white the tint was the same in each case.

13. Emerald green with white gave the same tint in both cases; with black the tint was not nearly green enough in the binocular union.

14. Ultramarine with white or black gave about the same result in each case.

15. Purple and yellow (107·7) by rotation gave a decided yellow: the resultant impression from binocular vision was much more neutral.

It will be noticed that in all these experiments the colors of the discs were such either in intensity or nature that by rotation a neutralization was not to be expected; I now pass to those cases where the tints were complementary or approximately so.

1. Golden yellow and Prussian blue gave about the same tint in the two cases, viz., a nearly pure gray.

2. Vermilion and greenish blue gave approximately the same tint in both cases, viz., a gray slightly purplish: the tint by binocular vision was a little more neutral.

3. Greenish yellow and purple gave sometimes the same tint in both cases, viz., a gray with a tinge of green.

4. Emerald green and purplish red gave by rotation a good neutral gray, but I could not, by binocular vision, get exactly the same result, the tint of the resultant being either too red or too green. Both of the original colors used were very pure and bright.

Lastly, I give two combinations on which many experiments were made, viz., first, chrome yellow and ultramarine. By the stereoscopic union of the components a pure gray was obtained, or at least a gray without any tinge of red, but by rotation the color was of reddish copper hue, which could not have been anticipated in using the other method of combination. Also, yellow (107·7) and ultramarine often gave in the binocular method a neutral gray, while the true union of the colors gave a flesh tint.

It will be seen from these experiments, that while a combination does in reality take place, yet that the resultants in the two cases may very considerably differ, and farther, that there is a tendency in the binocular method to consider the resultant impression a neutral one, when the method by rotation shows that this is not truly the case. This is illustrated in a striking manner by an experiment previously published by me, which I have lately repeated a number of times with the same result. A yellow glass is held before one of the eyes, a blue glass before the other, when, as Brücke rightly states, a landscape thus viewed, seems of a neutral gray if the attention be equally directed to the two impressions. When, however, the two glasses are attached to opposite openings in a blackened card disc, and set in rapid rotation so that the tints are truly mixed and a true resultant obtained, this is found (with the glasses I employed), to be a strong purple, though nothing of this kind is seen or suggested in the binocular use of the glasses. It is probably this tendency to consider the resultant neutral, that makes it so easy to combine, in binocular vision, tints produced by polarized light, and to perceive that the resultant is white.

It may be remarked in passing, that when the stereoscope is held as described before the two halves of the colored disc, and the latter is caused to revolve slowly, that the aperture in the card board appears extraordinarily lustrous, so much so that it would be possible by a little artistic arrangement of the accessories to deceive persons with the belief that they were looking at the surface of a polished metallic mirror.

Binocular union of complementary tints produced by polarized light.

For the purpose of experimenting on this point, I arranged a binocular polariscope, consisting of a large plate of polished glass resting on a black cloth, two Nicols' prisms being fastened before the two eyes in the right position, the principle section of one of them making an angle of 90° with that of the other. As objects, plates of selenite were used, fastened behind apertures in black card board. Films of common mica cannot well be employed on account of the rapid change of the tint by the least inclination. As the colors were exactly complementary, the resultant by binocular vision should always be pure white.

Results with polarized light.

1st, Red with greenish blue of 2nd order combined with some readiness to make a white. The same was true of these tints in the 3rd order; here the resultant white was *very easily* obtained. The same tints in the 4th order gave an excellent result; the components were of course pale, but the resultant white was proportionally steady. So also with the same tints in the 5th order.

2nd, Golden yellow with its complementary blue of the 2nd order, gave a good result. The same tints of the 3d order also readily neutralized each other.

3rd, Yellow and indigo blue of 1st order gave easily a very good white. The same tints in the 2nd order united with more difficulty. Union was about the same in 3rd order.

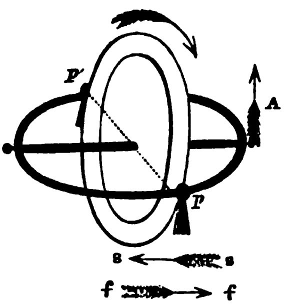
4th, Greenish yellow and violet of 2nd order combine to a white with some difficulty. The same is true of these tints belonging to the 3rd order.

5th, Green and purplish red of the 2nd order, united with moderate ease. The same tints in the 3d order united with great ease, and the resultant white could be retained very steadily.

From the above it will be seen that the union most easily takes place when the complementary tints have a low intensity. The same result can also be produced with the colored discs by shading them, when the two impressions much more readily fuse into a neutral single one.

Art. XXIX.—*On an experiment with the Gyroscope;* by Prof. O. N. Rood, of Columbia College.

If a gyroscope be suspended in equilibrium by pivots at *p* and *p′*, and the disc be set in rotation in the direction indicated, and an attempt be made to cause it to rotate about a new axis *p′ p*, in the direction shown at A, the disc will tend to turn in the direction *s s*, and if the pivots *p p′* are immovable, pressure against them will be generated in the direction *s s*, which will continue till the disc has been rotated through an arc of 90°. If the rotation around the new axis *p p′*, be still continued, the pressure on the pivots *p p′* becomes reversed, and the disc tends to move in the direction *f f*, and retains this tendency till it has passed over an arc of 180°, when this tendency is again reversed and becomes the same as in the first case, its sign not being changed till the disc has passed through an arc of 180°.



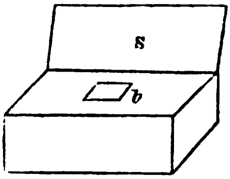
Hence, during the entire revolution around the new axis *p p′*, a varying pressure is exerted on the fixed pivots, alternately to the right and left hand.

It occurred to me that by causing a gyroscope, suspended in this way, to rotate *rapidly* about a new axis as *p p′*, its original motion might thus be destroyed in this indirect manner. A gyroscope was selected, which, when set in rotation by a certain force and allowed to remain undisturbed, continued in motion for 16 minutes. It was connected with a set of multiplying wheels, and the instant after it had been started in rapid rotation, it was forcibly caused to revolve about the new axis *p p′*, at a rate of forty revolutions per second. The resistance experienced was considerable, such as in two cases to eject the disc from its mounting, and the motion of the disc was found to be arrested in about twenty seconds, or in the $\frac{1}{48}$th portion of the time it otherwise would have occupied.

ART. XXX.—*Description of a simple apparatus for producing lustre without the use of Lustrous surfaces or of the Stereoscope;* by Prof. OGDEN N. ROOD, of Columbia College.

IN attentively looking at plane polished surfaces of aventurine glass, I have often been for a moment unable to determine by binocular vision exactly where some of the imbedded crystals were placed, they seeming for an instant to be suspended in the air above the glass. This uncertainty depends on the fact that it frequently happens minute crystals will reflect bright beams of light to one eye but none to the other, so that one of the binocular components is wanting. The apparatus described below was contrived so as to produce the same effect even to a much greater degree, as well as to show the production of simple lustre.

A box was constructed three inches long, one and a half inches broad and deep; this was blackened, and provided with a black screen at S; the side directly under S was left entirely open, and allowed the light from a window or candle to enter. At *b* was an aperture $\frac{1}{2}$ inch square, which was covered by a black card-board or piece of blackened brass foil, in which a hundred or more pin-holes, or better, minute triangular apertures, were punched. Directly below this on the blackened surface of the floor of the box, were sprinkled about a hundred small pieces of white, red, and green paper, each being about $\frac{1}{15}$th of an inch square. When thus arranged and properly illuminated, many of the apertures appeared lustrous, like small brilliant grains of sand, from the reason that, owing to the construction, light of different intensity or color was presented to the two eyes.



If now a second blackened diaphragm of brass foil, perforated with many minute triangular apertures, carefully prepared so as not to have an indented surface, be placed above the first set of apertures, at a distance of $\frac{1}{2}$ or $\frac{1}{4}$ of an inch, brilliant points of light are seen either by one eye or the other, light from the same opening rarely reaching both eyes. It now becomes impossible to decide on the location of these points, and they often seem suspended in space, somewhere in the interior of the box, but exactly where the observer cannot determine.

ART. XXXI.—*Remarks on the Beatriceæ, a new Division of Mollusca;* by ALPHEUS HYATT, Jr.

DURING an expedition to the island of Anticosti in the summer of eighteen hundred and sixty-one, undertaken by Mr. A. E. Verrill, N. S. Shaler and myself, and prosecuted under the auspices of the Museum of Comparative Zoology, in Cambridge, numerous specimens of Beatriceæ were collected at English Head, West Point, near Junction Cliff, on the shores of Ellis Bay, and at several points between Salmon River and Fox Bay.

I have satisfied myself by a careful inspection of this large collection during the past two years, that these remarkable remains are not tree-like fossils, as originally described by Mr. Billings of the Canada Geological Survey, but a new and interesting order among Mollusca closely allied to the Orthoceratites.

I have in course of preparation a monograph upon this new order, but, as unforseen difficulties will probably delay its completion, I hope that even so slight a sketch as is here given of the most prominent characters may not prove uninteresting.[1]

The Beatriceæ are long cone-like bodies, composed of three distinct parts, or layers, in the following order: (1) A central chain of small hollow chambers; (2) a succession of concentric coniform layers; (3) an external or sub-epidermal layer.

(1.) The central chambers are imperforate, generally deeply concave, and set one upon another like a pile of Chinese teacups. The tops, as also the bases of the coniform layers, are turned toward the larger end of the fossil, and completely closed by the bottoms of the succeeding cavities. They invariably occupy the axis of the encircling coniform layers, and have thin and sharply defined walls, resembling the fossilized nacreous septa of an Ammonite or Nautilus, and are not continuous with each other or with the adjoining coniform layers.

These cavities present many variations of form in the same individual, especially in *B. undulata*, sometimes in this species even becoming reversed, convex instead of concave, or broken up into several minor chambers. Such irregularities, however, are merely local, arising from mutilation or other disturbing causes, not affecting any individual as a whole. In *B. nodulosa* they remain, with but few exceptions, constantly concave, and have regular forms.

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[1] Although any adequate acknowledgment of the kindness shown to the members of the expedition by Prof. Louis Agassiz, Sir Edmund Head, then Governor-General of Canada, and Mr. George Ticknor of Boston, all of whom did their utmost to aid us in accomplishing our designs, would be more appropriate in the monograph alluded to above, I cannot refrain from availing myself of the present opportunity to express our sincerest thanks for the sympathy and assistance we received from them. My personal thanks are also due to my father, Mr. Alpheus Hyatt, Sr., of Baltimore, Md., to whose generosity I owe the privilege of attending the expedition.

(2.) The coniform layers are separated in fully grown specimens by intervening spaces near the central line of cup-like cavities, which become gradually shallower upward and outward, and finally disappear between the closely appressed bases of the overlapping layers.

In consequence of the great length of the cones or layers, the upper or narrower portions of the intervening spaces in *B. nodulosa* are parallel with the external surface, and appear as narrow streaks of blue limestone. In the young of *B. undulata* and *B. nodulosa* the intervening spaces are not present, but the layers are easily distinguished and have the same abrupt cone-like terminations as in the adult. The cones are formed of numerous thin laminæ, whose surfaces being but partially in contact, form oblong, angular, interstitial spaces or cells, with two concave, and one convex side, the latter always turned outward. They are largest near the center, giving a porous, spongy aspect to that part of the cone immediately around the axial cavities, but become gradually smaller outward until they are not distinguishable by the naked eye between the closely packed laminæ.

(3.) The sub-epidermal or outer layer is of a dark brown color, and in well preserved specimens is ornamented and continuous without annular marks or furrows.

It was probably the structure of the overlapping partitions or cones, and the parallelism of their upper parts, which in a transverse section closely resemble annular rings of growth, that led Mr. Billings to class Beatriceæ with plants. The cellular structure is, however, not due to the presence of true cells pervading the substance of the cones, but to the regularly formed interstices, described above, between the thin laminæ of which they are composed; and the annular appearance is consequent to the overlapping of the cone-like concentric partitions, which were deposited from within, and cannot, therefore, be considered plant-like exogenous layers.

We were fortunate enough to obtain a specimen of *B. undulata* at English Head which demonstrates that the cone-like partitions are the walls of chambers that were successively occupied by a part, if not by the whole body of an animal.

It is a fragment of the larger end, broken across the terminal chamber, exhibiting the outline of the animal parts, contained within the cavity at the time of the burial of the Beatricean in its Silurian grave. The centre is filled with a core of carbonate of lime from which a variable number of lobe-like longitudinal folds impinge against the sides of the vesicular cone, corresponding to angular external ridges projecting from the outer surface. The spaces between the folds are acutely angular, and answer to an equal number of broad shallow channels between the external ridges. The correspondence of this core with the exterior renders the supposition of its being the fossilized form of those parts of the animal that secreted the shell extremely probable. The differences, also, which exist between the lobe-like folds and the corresponding, angular, external ridges between the vacant, deeply cut, angulated interspaces, and the shallow, external channels, show that the inclosed body was surrounded by a mantle. If the shell had not been secreted by an enveloping mantle, the outline of the exterior would have corresponded more closely to the internal irregularities of the surface of the body, and there would have resulted a shell with prominent lobe-like folds, instead of depressed, angular ridges, and deeply cut angulated interspaces, instead of broad, shallow channels.

In attempting to refer the Beatriceæ to their proper place in the animal kingdom, we were at first much impressed by their great likeness to Cystophyllum; Mr. Verrill, however, after having made extensive comparisons between them and all the seemingly allied forms of Radiata in the Museum of Comparative Zoology in Cambridge, became convinced that their apparent affinity to the Radiate branch was nothing more than an analogical resemblance; extending, however, in a most extraordinary manner throughout the internal parts.

Mr. Verrill separates them from the Radiata principally because of the absence of any radiated structure, and the presence of the external, subepidermal, ornamented layer; features which he considers as irreconcilable with the structure of that branch.

An additional objection of considerable weight, may be founded upon the internal organization of the specimen, from English Head, above described.

This, with the fossilized remains of its former occupant still preserved within the chamber, the last of a series of other cone-like chambers, when considered in connection with the continuity of the sub-epidermal layer, and the absence of all annular furrows, shows conclusively that the shell was secreted by a Mollusk.

The Beatriceæ are very like the Hippuritidæ, both in general form and the arrangement of the component parts of the shell; but here again it may be demonstrated that the resemblance is not so close as it at first appears to be.

The geological horizon in which they occur, without other evidence, would alone be sufficient to render their affinity with the Hippuritidæ exceedingly doubtful; but, besides this, the structure, evidently, is not so closely allied to that of the Hippurite as to the Cephalopod. The shell of the Hippurite is composed of three parts: first, the inner septa, second, the outer layers, which frequently form a porous mass,[2] and third, an external sub-epidermal layer.[3]

The inner septa, which supported the principal part of the body of the Mollusk, form large cavities, while the second part is made up of laminæ laid on by the mantle margin, or at least that part corresponding to the mantle margin of the Lamellibranchiates.

In Beatriceæ, on the contrary, the inner septa did not contain the body of the animal, and there are no marks whatever of a mantle margin. This objection could not be urged against their affinity with Caprinella and the like, in which the central cavities are small; but from these they may be separated by the absence of all ligamental or muscular impressions and the mode of forming annular, cellular partitions, composed of numerous laminæ, instead of a continuous series of porous or tubular laminæ.

The Hippurites, Caprina, and the like, were, with few exceptions, attached to the surfaces upon which they lived or to each other, and had short, thick, cone-like forms, affording broad bases of attachment, whereas the Beatriceæ were long, thin, almost tubular bodies, resembling the Orthoceratites, and entirely unfitted to support themselves in fixed positions.

We saw hundreds of *B. nodulosa* and *B. undulata* "in situ," but nowhere any indications of attachment, either to the rocks or to each other.

After close comparison with all the types to which these singular fossils appeared to have any resemblance, I have at length considered myself warranted in considering them as Cephalopods more closely allied to the genus Endoceras, than to any other group of that class.

They differ greatly from all the Tetrabranchiates, in the open structure of the partitions or septa between the chambers, and this character, together with the absence of a siphon, and the cone-like form of the septa, demands that they should be separated as a distinct order, for which I propose the name of Geriolites.[4] Although distinct as an order because of the differences in the form and structure of the septa, arising from their great length, and the loose way in which the laminæ are arranged, the parts may be compared point for point with similar parts of Endoceras.

We may imagine the cone-like septa of a Beatricean to be spread apart, until their surfaces should be parallel throughout the shell; they would then be entirely separated by hollow chambers, as the septa are in Endoceras, and if, at the same time, the central cup-like cavities were supposed to be prolonged into cones, we should, without violence to the typical idea of the organization, have transformed the Beatricea into a shell separable from the Endoceras by only one character, the vesicularity of the septa.

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[2] Woodward. Hippuritidæ. Quar. Jour. Geol. Soc.; London. v. 15, part 1, page 41.

[3] Woodward. Quar. Jour. Geol. Soc., p. 46.

[4] κηριον, a honeycomb, λιθος, a stone.

The analogies which the Beatriceæ have with plants in their general aspect, with Radiates in their internal vesicular structure, and with Hippurites in the arrangement of the parts, are so close as to entirely bury, as it were, their true affinity with Cephalopoda, which only becomes obvious after diligent comparisons.

This complex nature establishes the Ceriolite as one of those strange types that not only combines, as does the "*Phoronis hippocrepia*,[5] the Annelidan homomorph of the Hippocrepian Polyzoa," the characteristic features of other groups belonging to different branches of the animal kingdom, but also of groups in other classes within its own sub-kingdom.

All these analogical relations are, so to speak, retrospective; they refer to lower ranks of life than the one in which Beatricea itself is found.

There still remains a question which I have not been able to solve in a satisfactory manner with the specimens at my command. Are the Beatriceæ internal or external shells? This problem, so difficult to settle conclusively with regard to the Orthoceratites, is equally puzzling in the structure of Beatriceæ. Their extreme length, cellular structure, and the irregularity of the ornamentation, would be almost determinative in favor of their being internal shells, were it not for the aspect of the cast of the animal in the terminal chamber of the specimen from English Head, which proves that a large portion, if not the whole of the body, was contained within the shell.

If, however, upon further examination, they should prove to be internal, which I consider doubtful, the cycle of their analogical characteristics would be completed, and they would refer not only to lower types, but have certain features in common with the more highly organized dibranchiate Cephalopods.

There are but two known species of the order Ceriolites, both occurring in the Silurian strata of Anticosti.[6]

Mr. Billings, in describing the species, states that they differ only in the external ornaments or markings.[7]

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[5] Allman's fresh-water Polyzoa. Ray Soc., 1856, p. 55.

[6] Since writing the above I have been indebted to Prof. J. D. Dana for the loan of some fragments of a Beatricea, resembling *B. undulata*, from Kentucky. The remoteness of this locality affords ground for hoping that the remains of other species may be brought to light in some of the intermediate Silurian basins.

[7] Billings's Report, Canada Geol. Survey. 1853-56. p. 343.

He, however, probably did not possess so fine a series of natural sections as it was our good fortune to collect, or he would have discovered the very considerable differences which exist in their internal structure, by which they may be as readily distinguished as by the external characters. I think, therefore, it may not be amiss to redescribe the species, giving such peculiarities as have not already been mentioned.

ORDER CERIOLITES, Hyatt.

FAMILY CERIOLIDÆ, Hyatt.

Genus BEATRICEA, Billings.

Beatricea nodulosa, Billings, is a long cone, tapering very gradually, the central chambers occupying from a fourth to a third of the transverse diameter in adults; in the young they are larger proportionally, varying from one-half to two-thirds of the breadth of the whole shell. The coniform layers are nearly parallel; the inclination at the lower part, as they trend outward to the circumference, being decreased very slowly. The external shell is closely set with tubercles and covered with granular points. The size, as nearly as could be inferred from fragments, is not over four feet long, by from three to five inches in diameter at the larger end.

B. undulata is a much larger species, one fragment found by the expedition being thirteen and a half feet long, by eight and a half inches in diameter at the larger end, and judging by the inclination of the sides, the length of the entire shell, when living, was certainly not less than twenty feet. The chambers are very small, frequently in adults not occupying more than one-tenth of the transverse diameter. The coniform layers are more widely separated in the adults than in the young; their inclination as they trend outward to the periphery is more decided, and they nowhere assume the parallel appearance of the same parts in Beatricea nodulosa.

The exterior is granulated and ornamented by ten or more prominent longitudinal ridges and intervening broad, shallow channels.

Cambridge, Mass., Dec. 6, 1864. (Residence, Baltimore, Md.)

ART. XXXII.—*The Albert Coal, or Albertite, of New Brunswick;* by CHARLES H. HITCHCOCK.

THE nature of the Albert coal and the mode of its occurrence in the strata have been vexed questions in geology. Its beautiful appearance attracts the eye, while its pecuniary value gave rise to the litigation which occasioned the delivery of the diverse opinions. In this as in so many other difficult subjects time has developed much truth, and shown us that we must not insist too strongly upon seemingly well-established theories.

In this communication I propose to describe briefly the geological features of the Albert coal vein and the accompanying rocks. I shall, whenever necessary, refer to the facts observed by others in this locality, but rely chiefly upon my own observations made in 1861 and 1864, as well as upon hints derived from persons of intelligence living in the vicinity. For a knowledge of many facts relating to the distribution of the different strata, I am indebted to Mr. James Blight, of Hillsborough, N. B. It is necessary to be dependent upon others for some knowledge of the internal structure of the Albert Mine, because the Company will not allow any scientists to examine their property below the surface. I use the word *coal* as a matter of convenience, not necessarily in strict propriety.

There are four different mining properties in Hillsborough, situated upon veins of Albertite:—the Albert, (the only one worked extensively and thoroughly proved), the East Albert, the Prince of Wales, and the Princess Alexandra. The second lies east of the first, and the others north and south of the same. Hillsborough is situated upon the west bank of the Petitcodiac river, near its confluence with the Bay of Fundy.

The rocks are of Lower Carboniferous age, and belong to the Acadian coal series. Several species of *Palæoniscus*, *Lipidodendron*, *Lepidostrobus*, *Spheraedra, and Stigmaria* occur in the shales and sandstones. Two or three miles southwest from the Albert shaft there appear older crystalline rocks, such as syenite and metamorphic slates; constituting the easternmost point of the extension of these rocks from the vicinity of St. John. The lowest rock in the Carboniferous series is the Albert shale; but I cannot state whether it crops out near the syenite. It probably does not appear anywhere on the edges of the coal basin. Nothing similar to it occurs in the Joggins' section. It should be expected to occur near and below the numerous deposits of gypsum in the eastern provinces.

This shale contains a large amount of hydro-carbonaceous matter. Certain layers of it at the "Caledonia Oil Works," by a rude process, have yielded thirty gallons of refined illuminating oil to the ton. The greater portion of the shale will sustain a fire without the aid of other fuel. Other layers are more bony, and others still highly ferruginous. It contains immense numbers of fossil fish, almost enough to make one imagine they gave the shale its inflammable character. The surfaces of many layers are glazed. The rock is very weak and abounds in small contortions of the strata. It appears in three localities. The largest has the Albert shaft in its centre; being exposed a mile or so in length, and showing best in the low ground. Small patches of shale may be seen on Peck's and Stinking Creeks, besides an unknown amount farther west at the oil-works. This series cannot be less than one thousand feet thick, as it has not yet been cut through by the shaft, and the general inclination of the bedding is very small.

The second group of strata is a conglomerate, separated from the first by a narrow bed of sandstone. Bits of Albert coal and shale constitute component parts of certain coarse sedimentary strata of this group, and render them oleaginous. The thickness is unknown, probably from 100 to 200 feet. Between the Albert shaft and the Petitcodiac, from three to four miles, this group prevails at the surface, except it be a small area of shale in a deep valley.

The next layers in ascending order are red marl and a bluish gray siliceous limestone. Above these are immense deposits of gypsum, from which most excellent plaster-of-paris is made for exportation. The highest rock in the series in this vicinity is another great mass of reddish conglomerate and sandstone.

There appears to be an anticlinal axis passing through this region, trending nearly ten degrees north of east. I have traced it from the Albert shaft to the Petitcodiac river, and have reason to believe it extends much farther, both west and east. The anticlinal structure is shown in three ways. *First*, the testimony is unanimous that there is an anticlinal in the Albert Mine. *Second*, the conglomerate dips in opposite directions in seven equi-distant localities examined over this area, but the dip is small. *Third*, the rocks succeed one another on both sides of the supposed anticlinal line in the ascending order mentioned above. On the north, we find above the shale, conglomerate, red marl, limestone, gypsum and sandstone. On the south, the order is the same with an opposite dip.

There appears to be a fault along or near this axis, displaying the usual phenomena of anticlinal fissures, and its location may easily be accounted for. The great plicating force acting from the direction of the ocean crowded the Hillsborough rocks northwesterly from the Bay of Fundy, crushing them up into a fold. The shale was not strong enough to sustain the bending; hence its layers were much twisted and fissured along a central line.

It is not likely that any of the shale rose above the surface at the time of flexure; and now that a portion of it has been laid bare by the removal of the upper rocks, the fissure and contortions show more plainly than in the overlying tough conglomerate to the east, which being narrower will naturally contain less of any foreign matter that has subsequently been injected into it.[1]

Some relics of this great force are now perceptible at the Albert mine, showing that the pressure is still exerted, perhaps as strongly as at any time of its manifestation. This phenomenon is much more noticeable than the "swellings of the walls," so common in deep mines. As soon as the coal is removed, strong timbers are put in to keep open the drifts, but in a short time these cross-pieces are split and crushed by the powerful force pushing the walls together. And when the timber is destroyed, the walls shut in, closing with a great noise as loud as thunder for hours, but not so near the workmen as to interfere with their progress. Not merely do the walls close, but frequently large fissures are produced behind the vein, so that the miners can clamber up and down new crevices. Large masses of rock are sometimes detached from either wall, in consequence, filling up the drift. We might explain the falling of fragments by gravity, but not so easily the crushing of the timbers.

The coal shows the effects of the crushing process no less plainly. It is much broken, even to grains, and needs no pick for its removal from the vein. It will flow as easily as heaps of corn, and therefore pains are taken to tap the vein in the right place, and at the proper time. If by oversight the main shaft is not walled up very tight, the coal will stream through the crevices between the beams, to the great inconvenience of the workmen.

The first outcrop of Albert coal was discovered by John Duffy, fifteen or sixteen years ago, in a deep ravine on Frederick's brook. The vein was about four feet wide, but by working upwards twenty-five feet into the bank, it thinned out to two or three inches. Duffy drifted about 300 feet on the course of the vein above the water-level, and sunk a shaft sixty feet, where the coal is said to have attained a width of ten feet. He then disposed of the property to Cairns, Allison & Co., who held it at the time of the litigation, but have now mostly transferred their shares to other parties, holding them under the same charter.

To describe the numerous variations in the course, thickness and shifts in the Albert workings, so far as known, would be unnecessarily tedious. Percival describes them for the first 200 or 300 feet of the descent, occupying more than three pages of the size of this Journal in a little more than catalogue style of enumeration. I am assured by the manager, Capt. Byers, that, from that depth to the bottom of the shaft, 950 feet, the character of the irregularities has not changed. In brief, the peculiarities of the mine are the following.

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[1] The fact of the existence of the coal in a vein occupying an anticlinal dislocation was maintained by Messrs. Robb and R. C. Taylor in their Joint Report upon the Albert Mine in 1851. See *Proc. Am. Phil. Soc.*, vol. v, p. 242. Their report was accompanied by chemical analyses by Dr. C. M. Wetherill, who made the material a variety of asphaltum and named it *melan-asphalt*.

The general course of the vein is N. 65° E., but the coal is repeatedly heaved southward by small faults. Its inclination is northwestward from 75° to 80°, often vertical. The body of the vein is extremely irregular, constantly expanding and contracting, both laterally and vertically. What is too narrow to be worked in one level enlarges to six and twelve feet, a hundred feet lower, or the reverse; but in general the width increases in following down the vein. At the time of Percival's examination, the vein was not considered workable 170 feet west from the old shaft. At lower levels the yield is renumerative 700 feet west and 2300 feet east of the new shaft, which lies several rods west of the first. In consequence of the uncertainties in the character of tho vein, it is found necessary to accumulate a large supply of the coal during the suspension of navigation, so that there will always be enough stock on hand in the warmer months to load the vessels without delay. Whenever a displacement is met with the vein is not lost, because a film of the coal remains in the slip to indicate the location of the heaved portion. The widest part of the vein is said to be twenty-eight feet.

The narrow portions of the coal are invariably contained in a harder rock; where the rock is softer the vein is larger. "Horses" are common. In such cases the cavity above, out of which the horse fell, is found to be filled with coal; so that the width of the coal at that level is equal to the usual width *plus* the width of the horse. Numerous small branches run off into the shales from the main vein. These are short and might be described as irregular and branching spines from a main stem. Many of the fragments of rock taken from the mine show these small injections. The most striking proof of the proper character of the mass is afforded by the edges of the strata in contact with the coal; they are coated with Albertite, while the surfaces are covered only when enclosing one of the small lateral branches, a few inches long.

With the facts now presenting themselves to the explorer, I think no one would call the Albertite mass a bed. It occupies an irregular fissure along an anticlinal line, and the deep workings have failed to develop the lower anticlinal branching of the coal anticipated by the advocates of the bed theory. The numerous branches are unlike any phenomena connected with beds. There is no fire-clay to form the floor of a bed; and, in addition, the common adherence of the coal to the edges of the strata, rather than slickensides—I do not mean those in the horizontal slips—seems to complete the evidence that the coal does not occur between stratified planes. To disclaim a bedded character casts no reflections upon the observations of the distinguished geologists who have decided otherwise; because they started with erroneous premises. To them the idea of coal in a vein was preposterous. It appeared as great an anomaly as it would be now to find Niagara fossils in the Potsdam group.

The vein-character of the deposit is seen more distinctly in the smaller openings. On the East Albert property two shafts have been commenced near the anticlinal line in the conglomerate over the shale. These reveal, at the depth of thirty feet, nearly six inches width of a richer and more beautiful coal than the Albert, gradually thinning out to the width of coarse paper at the surface, and most unequivocally cutting vertically across nearly horizontal layers of sandstone. As before, we have here the phenomena of shifts constantly working the vein southward, and a slight leaning in the same direction. Following the line to the Petitcodiac, there are seen other openings upon the vein of less extent.

The two veins crossing the anticlinal are very interesting. Upon parallel lines about a mile apart, their course is N.E. and S.W. One appears to intersect the principal vein very near the Albert shaft. The intersection of the other is concealed by a great depth of alluvium. It cannot be said that the coal is likely to prove more abundant at these intersections, as is the case at the union of metallic lodes, yet the similarity of the two classes of veins is such as to warrant the exploration.

These two side veins cut the strata nearly at right angles to the dip. The following is in general their nature, as observed in half a dozen openings. The conglomerate with a gentle dip is traversed by vertical joints, two of which parallel to each other, and from two to seven feet apart, are filled with threads of Albertite, occasionally enlarging to bunches an inch thick. Between them are branching threads of the same material, joining the lateral seams at various angles. I think there are no branches upon the outside walls. The whole field reminds one of an area of tin veins. Like the others, this vein-field leans slightly southward. It will be interesting to watch the development of these veins to see whether they will develop like the Albert. Their persistency and ability to cut through the strata render them worthy of attention. But no one ought to expect to discover a large vein till the threads have been followed down to the subjacent shale. The surface at the Albert mine is more than a hundred feet lower geologically than the bottom of the East Albert shaft where the coal is nearly six inches wide.

I think the following conclusions may be drawn legitimately from the foregoing and kindred facts.

1. *The Albert coal occurs in true cutting veins, not in sedimentary beds like ordinary coal.*

2. *The Albert coal was originally in a liquid state, was injected into vertical fissures, and subsequently hardened into a substance resembling jet.* The liquid may have been derived from vegetable accumulations, or possibly in part from the abundant ichthyic remains in the shales. Whether the shales were originally oily, as now, and the fissures subsequently filled with a viscid fluid derived from them, or whether the charging of the fissures imparted an inflammable character to the rock, I will not conjecture, though it is easy to satisfy one's own mind. The cavities of the Albert coal occasionally hold liquid petroleum, and those in the adjacent shales more often. A few quarts of petroleum have been brought up from borings along the line of both the Albertite veins on the east side of the Petitcodiac. With the hardening, the hydro-carbonaceous liquid received oxygen into its composition.

3. *The Albert coal must be compared with the asphaltic and bituminous veins found in the Quebec group in Canada.* It there "fills veins and fissures in the limestones, shales, sandstones, and even in the trap rocks which traverse these." "In other cases, it fills fissures several inches in diameter, so that it has been mistaken for coal, and attempts have been made to work it at Quebec and elsewhere. The mineral is never, however, in true beds like coal, but is always confined to veins and fissures which cut the strata." "The matter is of a shining black color, very brittle, breaking into irregular fragments with a conchoidal fracture." (*Geol. Canada.*) The Quebec coal is like the Albert in the small amount of the ashes, but contains more carbon.

4. *These carbonaceous veins are analogous to veins of petroleum.* The borings for petroleum in Ohio and Western Virginia are most successful along lines of fracture, particularly an anticlinal axis. The description of the chasm filled with oil would undoubtedly be given in words similar to those used respecting the Albert vein, if we could sink shafts and drive on the course. The views of Prof. Andrews in this Journal, ([2] xxxii, 85,) respecting the location of petroleum, are very just, and show that it often occurs along anticlinal faults. The immense yield of many oil-wells certainly suggests the presence of more than the "horse-cavities" filled with the liquid.[1]

5. The carbonaceous veins, such as the Albert coal, Canadian asphalt and liquid petroleum, while possessing many characteristics of metallic lodes, will be found to differ from them in some

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[1] A valuable paper, by T. S. Hunt, in this Journal, [2] xxxv, 157, 1863, upon Bitumens, etc., presents the general conclusions stated above. His data were derived both from analysis of mineral combustibles and explorations in petroleum districts.

Sir William Logan mentions the occurrence of petroleum springs for twenty miles along a fold in the stratification in Gaspé in 1844. J. P. Lesley has described a vertical vein of asphaltic coal, precisely like the Albert, in Ritchie Co., W. Va.

respects. These particulars will be ascertained fully by the immense enterprise now manifested in sinking for petroleum. We can anticipate differences in respect to the limited depth, little variations of thickness at intersections, irregular yield, and origin of the carbonaceous veins. A proper knowledge of them may lead to some modification of terms in our definitions.

37 Park Row, New York, Jan. 23, 1865.

ART. XXXIII.—*Detection of the adulteration of Essential Oils with Oil of Turpentine by the Saccharimeter*; by Dr. JULIUS MAIER, Assistant in the School of Mines, Columbia Coll., N. Y.

THE essential oils, especially the expensive ones, are mostly adulterated with oil of turpentine. It is often difficult to detect this adulteration, especially when the adulterated oil gives similar reactions with oil of turpentine.

With the saccharimeter it is possible not only to detect the adulteration but even to find out the quantity of oil of turpentine mixed with the other essential oils. A large number of oils, particularly those belonging to the camphenes, the carbon of which is in the proportion of 5 to 8 to their hydrogen, have an action on the polarized light, deviating the light either to the right or the left hand side. These optical researches have been made by Biot, Soubeiran, Capitaine, Gladstone and Berthelot, in order to establish the constitution of the camphenes. I made some researches to detect the adulteration of the essential oil with the oil of turpentine.

For that purpose, a chemically pure oil of lemon which I had prepared myself, was tested in a saccharimeter, the tube of which was 200^{mm} long.

The deviation was +137°·296 for the middle yellow ray. The oil of turpentine, used for the research, prepared by myself, had a specific gravity of 0·865 and gave a deviation of −73°·135. A mixture of equal volumes of both these oils showed a deviation of +30°·65. The calculation gives a deviation of +32°·081 in the following manner:

$$\begin{array}{ll} \frac{1}{2} \text{ vol. oil of lemon} & = +68{\cdot}648 \\ \frac{1}{2} \text{ vol. oil of turpentine} & = -\ 36{\cdot}567 \\ \hline 1 \text{ vol. mixture} & = +32^{\circ}{\cdot}081 \end{array}$$

A mixture of 2 vol. oil of lemon with one vol. oil of turpentine gave a deviation of +65°·34; from the calculation results a deviation of +67°·152, as follows:

$$\begin{array}{ll} \frac{2}{3} \text{ vol. oil of lemon} & = +91^{\circ}{\cdot}531 \\ \frac{1}{3} \text{ vol. oil of turpentine} & = -\ 24^{\circ}{\cdot}379 \\ \hline 1 \text{ vol. mixture} & = +67^{\circ}{\cdot}152 \end{array}$$

I made the same researches with pure oil of juniper, which I had prepared myself, and arrived at the following results:

The oil used for the experiment had a specific gravity of 0·858 and showed a deviation of −5°·970. The oil of turpentine employed was the same as in the above mentioned experiment. A mixture of equal volumes of oil of juniper and oil of turpentine showed a deviation of −40°·84; the calculation gives a deviation of −39°·553 as follows:

$$\begin{array}{ll} \tfrac{1}{2}\text{ vol. oil of juniper} & = -2^\circ{\cdot}985 \\ \tfrac{1}{2}\text{ vol. oil of turpentine} & = -\underline{36^\circ{\cdot}568} \\ 1\text{ vol. mixture} & = -39^\circ{\cdot}553 \end{array}$$

From this it is proved that the quantity of the adulterating oil of turpentine can be detected through the medium of the saccharimeter. But if the essential oil is adulterated not only with oil of turpentine but also with another optically active oil, the saccharimeter test is of no value. In order to find out the quantity of the adulterating oil of turpentine from the deviation showed by the oil of turpentine, by the adulterated oil, and by the mixture, the numbers of deviation have to be brought in reference to a common distinct starting point. This starting point generally is the power of rotation, that is the deviation of the respective oil as calculated for a tube of 100^{mm} length, and a specific gravity of 1. This power of rotation, the worth of which is generally expressed by $[\alpha]$, is the following for:

$$\begin{array}{ll} \text{Oil of turpentine} & [\alpha] = -42^\circ{\cdot}275. \\ \text{Oil of lemon} & [\alpha] = +80^\circ{\cdot}578. \\ \text{Oil of juniper} & [\alpha] = -3^\circ{\cdot}479. \end{array}$$

The quantity of oil of turpentine employed for the adulteration, is calculated as follows:

a the power of rotation of the pure oil.
b " " " " " " oil of turpentine.
c " " " " " " mixture.
m " quantity " " "
x " " " " adulterating oil of turpentine.

The quantity of the pure oil as contained in the mixture is $=m-a$, and the power of rotation of this quantity is $=(m-x)a$; the power of rotation of the oil of turpentine $=-bx$, and the power of rotation of the whole quantity of mixture $=mc$; hence results the following equation:

$$\begin{aligned} (m-x)a-bx &= mc \\ ma-ax-bx &= mc \\ ma-mc &= ax+bx \\ \frac{m(a-c)}{a+b} &= x \end{aligned}$$

To show this calculation by an example, the power of rotation is supposed to be—

of the pure oil of lemon $= +80^{\circ}{\cdot}573$
" " " " turpentine $= -40^{\circ}{\cdot}275$
" " mixture $= +18^{\circ}{\cdot}70$
the quantity of the mixture $= 20$ c. cm.

$$(20-x)\,80{\cdot}573-40{\cdot}275x = 20\times 18{\cdot}70$$
$$7611{\cdot}46-80{\cdot}573x-70{\cdot}275x = 374$$
$$1237{\cdot}46 = 122{\cdot}848x$$
$$10{\cdot}0 = x.$$

The mixture contains equal parts of the pure oil and the adulterating oil of turpentine.

Optical behavior of several essential oils.

Tested oil.	Specific gravity.	Power of rotation.	Observer.
Oil of absinth,	0·973	+ 20°·67	Soubeiran & Capitaine.
Oil of orange blossoms, first product,	0·835	+127°.43	
Oil of orange blossoms, second product,	0·837	+125°·59	
Oil of bergamot,	0·850(?)	+ 29°·28	Biot.
Oil of bergamot, first product,	0·850	+ 49°·396	Soubeiran & Capitaine.
Oil of bergamot, last product,	0·877	− 6°·573	
Oil of caraway seed,	0·897	− 11°·7	Maier.
Oil of lemon,	0·848	+ 80°·484	Biot.
Oil of lemon,	0·852	+ 80°·573	Maier.
Oil of lemon, (Grasse,) first product,	0·844	+ 79°·749	Soubeiran and Capitaine.
Oil of lemon, (Grasse,) last product,	0·853	+ 78°·156	
Oil of lemon, rectified,	0·854	+ 80°·916	
Oil of copaiva balsam,	0·881	− 34°·18	
Oil of copaiva balsam, (Para,)	0·898	− 28°·553	
Oil of cubebs,	0·929	− 40°·159	
Oil of cubebs, free from water,	0·914	− 39°·40	
Oil of elemi,	0·852	− 90°·30	Deville.
Oil of juniper,	0·855	− 3°·521	Soubeiran & Capitaine.
Oil of juniper,	0·858	− 3°·479	Maier.
Oil of turpentine,	0·8722(?)	− 39°·950	Biot.
Oil of turpentine,	0·860	− 43°·38	Soubeiran & Capitaine.
Oil of turpentine,	0·865	− 42°·25	Maier.

Gladstone (Chem. Soc. Jour., [2], ii, 1,) has given a more complete table on the power of rotation of most of the essential oils. My researches were restricted to the few above mentioned.

ART. XXXIV.—*Introduction to the Mathematical Principles of the Nebular Theory, or Planetology;* by GUSTAVUS HINRICHS, Professor of Physics and Chemistry, Iowa State University.

(Concluded from p. 150.)

§ 14. *The Lunar Distances.*

As Kepler's third law was deduced from the planetary orbits alone, so was the law of Titius. But it was shown to be a consequence of the law of universal gravitation, and therefore itself universal and applicable to any system—hence, also to the lunar systems. Now the law of Titius, as modified above, has been found to be identical with the equality of the intervals of time in the history of any system. Therefore, also, this law (38) must apply to the lunar systems. This we now will show.

A. *The Lunar System of Jupiter.*

The *Jovial World* is the youngest of those great lunar systems that adorn the exterior planets. (This Journal, xxxvii, p. 45.) Therefore, it is the most regular yet of any, and our law (38) must very closely harmonize with the actual distances of Jupiter's moons. It is easily found that $\gamma=2$, again, as for the planetary distances; and that $\alpha=4$ and $\beta=3$ radii of Jupiter. Thus (38) is for the Jovial World,

$$a_t=4+3\times 2^t. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (64)$$

	t.	Distance. Calculated.	 Observed.	Fall.
Moon I.	0	7	6·049	·951
" II.	1	10	9·623	·377
" III.	2	16	15·350	·650
" IV.	3	28	26·998	1·002

The "fall" of a moon is the distance it has fallen toward the planet in virtue of the resisting ether. That this fall corresponds to the age, mass and density of the different moons has been shown in our previous article. (This Journal, xxxvii, 45.)

The calculation of *t* from the observed distances gives for the 2d, 3d and 4th, respectively, ·907, 1·92, and 2·94, which only deviate by ·09, ·08 and ·06 from the theoretical values 1, 2 and 3; and all values being *too small* shows that these moons are correspondingly nearer the primary, having approached so much on account of the etherial resistance.

B. *The lunar world of Saturn*

is next in age, hence not quite so regular as that of Jupiter. We find that (38) represents the distances of the eight moons if the constants are

$$a_t=4+0{\cdot}35\times 2^t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (65)$$

as will be seen from the following table:

	Distance.		
Moon.	Calculated.	Observed.	Difference.
I. Mimas, . .	4·35	3·4	+·95
II. Enceladus, . .	4·70	4·3	+·40
III. Tethys, . .	5·4	5·4	·0
IV. Dione, . .	6·8	6·8	·0
V. Rhea, . . .	9·6	9·6	·0
VI. Titan, . .	15·2	22.2	−7·0
VII. Hyperion, . .	26·4	28·0 ?	−1·6 ?
VIII. Japetus, . .	48·8	64·0	−15·2

Excepting for a moment the 6th and 8th moon, we see but small differences; Mimas and Enceladus being too near Saturn, appear to have but very small mass, which conclusion is strengthened by the fact that it required Herschel's great telescope to discover them (1789). The next three almost exactly harmonize with this law; they are, therefore, not only larger than the first two, but also much alike. They were discovered by Cassini, first the fifth (Rhea) in 1672, and later (1684) Tethys and Dione. As the latter were discovered by the same observer, the difference in date is, perhaps, alone due to the greater nearness to the disk of the primary. Hyperion is even lower than any, and, therefore, smaller than even the interior ones. This is confirmed by its discovery, which was not made till 1848, by Bond and Lassell. But the sixth, Titan, and the eighth, Japetus, are much farther distant than (65) gives; thus proving them to have much more considerable mass (or rather *v* (37) is less, which in general will be the case if the mass is greater). This is fully confirmed by the date of discovery: Titan being the first discovered of all, (by Huyghens, 1655), and Japetus the second, (by Cassini, 1671). These estimates of the masses are further corroborated by Humboldt,[1] who calls Titan "the largest of all known secondary planets." Compare another theoretical estimate, (this Journal, xxxvii, 46), leading to the same results.

C. *The lunar system of Uranus*

is exceedingly important on account of the plane and direction of its motions. We have tried to show that this very position affords one of the most conclusive confirmations of the nebular theory. (This Journal, xxxvii, 50.) Here we will consider the arrangement of the individual members of the system.

We know it to be the oldest, because it is the most distant system of which we have definite knowledge. The original distances and the original harmony of these distances is therefore here most deranged. We cannot even with any degree of certainty consider the moons to be now in the same order of succession as at first. At the same time, observation has as yet hardly determined the number, much less the exact distance of the different moons. Therefore, we give the following more for the sake of completeness than with the view of adding any important confirmations of our law.

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[1] *Cosmos*, i, Harper's edit., p. 95.

We have seen that the nearest luminaries may be equi-distant, and that the farthest may succeed at distances that form a geometrical progression--see (61) and (63). If the distances, as given by Herschel, and the times of revolution, as given by Lassell,[2] are exact, we may represent the distance of the first six moons by

$$a_t = 7{\cdot}5 + 3t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (66)$$

corresponding to (62), and the distance of the sixth, seventh and eighth by

$$a_t = a_6 . 2^{t-6}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (67)$$

corresponding to (61).

	Distance.		
	Calculated.	Observed.	Difference.
Moon I=	7·5+0× 3 = 7·5	7·5	·0
II=	7·5+1× 3 =10·5	10·5	·0
III=	7·5+2× 3 =13·5	13·1	+ ·4
IV=	7·5+3× 3 =16·5	17·0	− ·5
V=	7·5+4× 3 =19·5	19·8	− ·3
VI=	7·5+5× 3 =22·5	22·7	− ·2
VII=	2×22·5=45·0	45·5	− ·5
VIII=	4×22·5=90·0	91·0	−1·0

If these observed distances really are correct, then this remarkable discontinuity will enable us to determine the lunar masses long before observation can ascertain them.

D. *Conclusion.*

The lunar system of the *Earth*, consisting of but *one* moon and that of *Neptune*, which comprehends one or two, cannot, or do not afford any chance to test our law. But we have seen that the systems of Jupiter and Saturn fully confirm our law (38), if due regard is had to the individual mass and volume—or density and radius—of the several moons. Even the system of Uranus, as far as known, does not deviate from it except in so far as it offers the two extreme limits of the law, probably on account of the high age and a close similarity between the masses of the first six moons.

Therefore we may say that as far as observation on the lunar systems goes it is embodied in our law (38), or *in every lunar system the consecutive moons were formed at equal intervals of time.*

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[2] See Schweigger in Astronomische Nachrichten, No. 832, Beilage.

§ 15. *The incommensurability of the periodic times.*

By the third law of Kepler we have, if T_ι and $T_{\iota'}$ are the periodic times of two planets,

$$\frac{T_{\iota'}}{T_\iota}=\left(\frac{a_\iota'}{a_\iota}\right)^{\frac{3}{2}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (68)$$

or by (38),

$$\frac{T_{\iota'}}{T_\iota}=\left[\frac{\alpha+\beta\gamma^{\iota'}}{\alpha+\beta\gamma^{\iota}}\right]^{\frac{3}{2}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (69)$$

which expression will not generally make $T_{\iota'}$ and T_ι commensurable. Thus we see that our law accounts for another important condition of stability of the system, (see § 3, 1).

But as the distances are continually decreasing, and at different rates, (this Journal, xxxvii, 41, gives the numerical values of these rates), we perceive that *in time such commensurability may take place* between any two planets.[3] Such is actually the case between Jupiter and Saturn, as discovered by Laplace.

The distances were (see § 13) for Jupiter $a_5 = 520$, for Saturn $a_6 = 1000$, giving for (68) the continued fraction $2(1, 1, 2 \, . \, . \,)$ having the approximations,

$$\frac{2}{1}, \ \frac{3}{1}, \ \frac{5}{2}, \ \frac{13}{5} \quad . \quad . \quad .$$

or $T_6 : T_5$ approached originally to 5 : 2; now it is very nearly so.

For Venus and the Earth the original distances 70 and 100 give the approximations,

$$\frac{1}{1}, \ \frac{2}{1}, \ \frac{5}{2}, \ \frac{12}{7}, \ \frac{29}{17} \quad . \quad . \quad .$$

whilst Airy has found the commensurability 13 : 8 or nearly our 29 : 17 [13 : 8=29 : 17·8].

In the *lunar systems* such commensurability is common; and it is for the satellites of Jupiter that Laplace demonstrated[4] the great proposition, *if such commensurability exists but approximatively it will become exact in time.*

Having seen that the change in distance produced by resistance will make the ratio approach commensurability, it therefore, as we stated before, will become rigorously so.

From (68) we find easily that the ratio will be 2 if the distances are in the ratio of $2^{\frac{2}{3}} : 1$, or (by continued fractions) as the approximative fractions,

$$\frac{1}{1}, \ \frac{2}{1}, \ \frac{3}{2}, \ \frac{8}{5}, \ \frac{19}{12}, \ \frac{27}{17}, \quad . \quad . \quad .$$

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[3] Grant (*History of Physical Astronomy*, London, 1852, p. 93,) states that the libration of the jovial moons is "independent of the effects of a resisting medium," meaning that it will be preserved notwithstanding such medium. This is probably a mistake, for it would depend upon the relative magnitude of the resistance and the perturbation.

[4] Méc. Cél., vol. viii, Ch. vi, § 15. We express the proposition in more general terms.

For Jupiter's satellites we have $a_2=16$, $a_1=10$, or $a_2 : a_1=8:5$; and $a_1 : a_0=10:7=8:5{\cdot}6$, therefore we find the periodic time of the second moon twice that of the first, $T_1=2T_0$, and the periodic time of the third twice that of the second, $T_2=2T_1$; hence Laplace's famous relation between the mean motions,

$$n_0+2n_2=3n_1.$$

In the system of Saturn similar relations obtain. At first we had (see § 14, B) for the distance of Tethys (III) and Mimas (1) the ratio 54 : 44=8 : 6·6, while the duplication of the periodic times requires the ratio 8 : 5. But Mimas has approached Saturn the most, and thus this proportion (now 5·4 : 3·4=8 : 5·04) has been brought about.

For the fourth and second we had originally, Dione : Enceladus=6·8 : 4·7=8 : 5·5, or likewise sufficiently near 8 : 5 that the duplication of the periodic time should become almost rigorous.[*]

The lunar world of Uranus is particularly noted for such duplications, from the fact that Schweigger, as early as 1814, on such grounds predicted the existence and gave the orbits of the two innermost moons of Uranus, which were discovered by Lassell in 1851. The coincidence is very remarkable, as will be seen from the following:[*]

	Schweigger, 1814.	Lassell, 1851.
Uranus, I moon, - - -	2·1767 days,	2·5117 days.
II - -	4·3534 "	4·1445 "

and the IV (or II of Herschel) having a period of 8·7068 days approximates to the further duplication of the periodic times. Also the period of III is about half the IV period, the former being 5·8926 days, the latter 10·9611.

Taking only the first two decimals we find by means of continued fractions the following approximations:

$$\text{II to I or } \frac{441}{251}=\frac{1}{1},\ \frac{2}{1},\ \frac{3}{2},\ \frac{5}{3},\ \frac{28}{17},\ \frac{33}{20},\ \text{etc.}$$

$$\text{IV `` II ``} \quad \frac{871}{441}=\frac{2}{1},\ \frac{19}{9},\ \frac{21}{10},\ \frac{40}{19},\ \frac{61}{29},\ \text{etc.}$$

$$\text{V `` III ``} \quad \frac{1096}{589}=\frac{1}{1},\ \frac{2}{1},\ \frac{13}{7},\ \frac{93}{50},\ \frac{199}{107},\ \text{etc.}$$

thus proving that only the fourth (Herschel II) and second (Lassell II) have periodic times nearly in the ratio of 2 to 1.

The other instances adduced by Schweigger, and especially the first, do not seem to have any claim to be considered as real duplications. Still it is evident that the configuration of the Uranian-system is such as approaches to simple ratios between the periodic times; and if the perturbating force arising here-from is greater than the effect of resistance, these ratios and the corresponding configuration would become permanent. It is not improbable that an analysis of the lunar system of Herschel's planet will throw much light on the future configuration of the solar world by ascertaining the exact relation between perturbation in commensurable revolutions brought about by resistance and the continued influence of the latter force on such commensurable motions.

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[*] Herschel, *Outlines of Astronomy*, § 550.

[*] Schweigger; Ueber die Auffindung der ersten Uranustrabanten durch Lassell. Astronomische Nachrichten. 1852, No. 832.

Though this latter question cannot at present be fully answered, we have proved in this paragraph that not only the general incommensurability of the periodic times ensuring the stability of the system, but also the deviations therefrom are accounted for by our law (38).

§ 16. *History of the Solar System.*

Believing that we have, in the preceding pages, brought forth some further arguments in favor of the nebular hypothesis, we may be permitted in a very few words to sketch the grand history of the material universe as it is seen in the light of this theory. The philosophers of old called Man a Microcosmos—we compare the Universe, the Macrocosmos, to man, thereby intimating that as Man has a parentage, growth and decay, i. e., a *history*, so has the Macrocosmos.

The history of the *material* world may be divided into four periods or ages, corresponding to those given in a note to §6. (Compare Guyot's views in Dana's Geology—chapter on "Cosmogony").

In the beginning *God created the heavens and the earth.* And the earth was *without form and void,* and *darkness* was upon the face of the deep. And the spirit of God moved upon the face of the waters. (*Genesis*, I, 1, 2).

The material universe was created *not* in its present form, but without form; it was *void* and *dark*; but the spirit of God pervaded it, and planned it such that his All-Foresight, or Providence, might also be manifest in the material world. *This* is really *the Creation*—it is merely stated, not described, for it is inconceivable to mortal understanding. It is too awful, our mind is lost in reflecting thereon; hence the divine writer merely mentions it at the beginning, and, to give fullness to his picture and adapt himself to our understanding, describes the first three great ages as real creative acts, though mere consequences of the unfathomable word given in the *first* verse of Genesis. We believe that the first five verses of Genesis have never before been fully understood in their deepest sense. We shall in the sequel keep constantly before our eyes both this, the *revealed History of the Cosmos*, and *science*, deduced from the revelation we have in the present form of nature.

Since Genesis merely states that the universe (i. e. heaven and earth) was *formless*, *void* of any organized being and *dark*, it is science alone that can give us any idea of the constitution of the universe as it came from the hands of the Creator. But as science is progressive, our ideas of the primeval condition of the Cosmos must progress correspondingly, or rather with advancing science our eye pierces farther and farther back into the dark past, approaches more and more to the mysterious and almighty "Fiat." As these approaching steps represent greater and greater series of ages, we infer that the *Fiat* lies infinitely far behind us, and can never be reached by human thought. We experience in regard to the age of Cosmos by penetrating farther and farther into the dark past with our spiritual eye, the same that we feel in piercing, by means of more and more powerful telescopes, farther and farther into the world-filled abyss of space. Here, if looking through a giant telescope we find ourselves surrounded by a boundless space filled with the wonders of the Creator; and if ardently searching in the existing documents of nature for records of her past, we behold infinity also here, the infinity of time, *eternity*, teeming with wonders no less astounding. The beautiful poem of Schiller, "*die Grösse der Welt*," is true both as to the extension and the duration of the World.

The ancients most frequently thought that the world left the hands of the Creator in the shape it now is. Even Newton himself was unable to see farther back. To him the Creator was but a tinker, forming his wheeling globes and wheeling them around their axis, putting them one by one and one by one to their very place in his clockwork—to him an unorganized machine to run on and on forever in the same shape. But Huyghens, and Newton himself, by discovering the generic cause of the figure of the earth aimed the first blow at this base idea, which nevertheless has found its advocates even to the present hour, especially among theologians. The corner-stone being broken out of the system it has been crumbling down. Geology has restored the lost history of the earth, and the nebular theory has traced this earth to the sun as her mother. Thus creation was now identical with the productions of the rotating mass of matter, i. e., of the chemical elements.

We have attempted to show that both rotation and the elements come from the forces wherewith the ONE matter (Urstoff) was endowed (see § 6). It is highly interesting to see how the first verse of Genesis has been understood by scientific men. It will at the same time more clearly set forth what we implied above when saying that science is approaching to the true original condition of Cosmos by making steps representing longer and longer periods of time.

"In the beginning God created the heaven and the earth" means according to

Newton, 1686: a direct, immediate creation of every globe as it is now.

Huyghens, Hutton, and modern Geologists: a direct creation of the heavenly globes as *fiery masses*, circulating in the system as they do now.

Kant, 1755, Laplace (later): a direct creation of a rotating mass of chemical elements; giving rise to the planetary system.

We, in 1854, conceived this rotating mass of elements to be the product of a created nebula consisting of but *one single element*.

We will now contemplate the different ages manifest in the development of this Urstoff.

First Day or Age.—The atoms of "Urstoff" combine—light (and heat) and the *chemical elements* result. The mere production of light would not entitle it to be considered one of the days of creation; but light is by the divine writer taken as a type to represent itself, and the less obvious, though much more important, chemical elements. It was not so much the light as the formation of the elements, the basis of modern physical science, which characterized the first day. We think that a rotation was also produced hereby. (This Journal, 1864, vol. xxxvii, p. 52.)

Second Day or Age.—Formation of the planetary orbs with their satellites.—The nebula developed itself into a great number of similar planetary nebulæ, which again gave birth to similar lunar nebulæ. Thus we see here the simplest kind of "life," reproduction by division, as exhibited by many plants, and even animals, which to distinguish them as such from inanimate matter, have another mode of reproduction *besides*. The planets represent the children, the moons the grandchildren of the sun.

Third Day or Age—The fiery balls resulting from this subdivision cool down and are shaped, as Geology has ascertained in relation to our own earth.

The Fourth Age of the *inorganic era* is the present. We have shown that the further characteristic of life, namely, death, is not restricted to the organic but is participated in by inorganic nature (this Journal, [2], xxxvii, 56). As every breath of our lungs is a differential of decay—so every rotation of the earth giving us the enjoyments of another day, and every revolution charming us with the succession of the seasons, brings our own mother earth nearer to her grave.[1]

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[1] We beg the scientific reader's pardon for these paragraphs, which do not belong to this place. But we felt it urgent to say at least this much, as some, even to-day, are apt to base the cry of "heretic," "infidel," etc., on any such deviation from the beaten path in their dogmas. The nebular hypothesis has richly participated in the abuse heaped in its day on the Copernican system, and on some leading doctrines of geology. Even yesterday, I found, in one of the leading religious quarterlies, Laplace called an "atheistic dreamer"! We wrote this paragraph as a protest against such imputations.

§ 17. *Conclusion.*

The principal results arrived at in this paper are

1st, A simple mechanical *theory of spiral nebulæ.*

2d, A more accurate determination of the orbits; and above all,

3d, *The discovery of the true law of the planetary and lunar distances.*

4th, The determination of the periodic times as a function of the distances—or borrowing this third Law of Kepler from the theory of gravitation, we have therein almost a theoretical demonstration of *the equality of the intervals.*

As (38) what we have repeatedly called "our law" is very much like Bode's, or rather Titius's, law, we apprehend that the propriety of thus naming (38) will be doubted. To set this point in clear light we refer to a similar, though undoubtedly grander, case in the history of science.

The law of Titius was exclusively derived from observation. It is empirical, as is the third law of Kepler. It is, moreover, not exact, neither in its general form nor in its numerical results. But neither is the famous law of Kepler exact, though, on account of the different circumstances connected herewith, this latter law agrees better with the numerical data of observation than Titius's law.

Newton discovered the true form of Kepler's law by deducing it from a higher law, that of universal gravitation. Instead of Kepler's form, C being the same constant for all planets,

$$\frac{T_2}{a^3} = C, \quad . \quad . \quad . \quad . \quad . \qquad (70)$$

Newton found that the true law is

$$\frac{T^2}{a^3} = \frac{\mu}{4\pi^2}(M+m), \quad . \quad . \quad . \qquad (71)$$

μ being the constant of gravitation, hence the same for all planets; hence,

$$C \propto (M+m). \quad . \quad . \quad . \quad . \qquad (72)$$

That is, Kepler's constant C is proportional to the sum of the mass M of the sun and the mass m of the planet. By farther analysis it is found that C even is dependent on *all* the masses and distances in the system.

So also in our case. We have given the true expression of Titius's law by extending it to Mercury and have accounted for the deviations of nature from the law, by demonstrating that it is a necessary consequence of the higher law, viz: *the intervals between the abandonment of the different orbs of the same system are equal* (see § 13). *Now this is what we claim as our law.* As Newton deduced and corrected Kepler's law by his law of *equal gravitation*, so we have deduced and corrected the law of Titius by our law of *equal intervals.*

We referred to (38) as our "law" because it is a *consequence* of our law, and certainly our formula; we did not intend to obliterate the merit of Titius, as will be seen wherever we have mentioned his name.

There is yet another circumstance which makes our demonstration of the law of planetary distances so important. It is the touchstone of the nebular theory; for as this ascribes the formation of the planets to the slow descent of cosmical matter to its center, it has to be proved that such descent will give exactly the actual system. Already Plato held that[7] "the motion of the planets is such as if they had been all created by God in some region very remote from our system, and let fall from thence toward the sun, their falling motion being turned aside into a transverse one whenever they arrived at their several orbits." Galileo was the first who subjected this "concetto platonico" as he calls it to a numerical calculation based upon the laws of falling bodies as discovered by him. He finds an admirable harmony between his calculations and the actual velocities and distances as they were known at his time.[8] Next after him, Newton took the matter in hand, and in his third letter to Dr. Bentley he gives as his result, that it is impossible to account for the configuration of the system in the manner of Plato and Galileo. This result is based upon his assumption of a vacuum. By taking the influence of a resisting medium into account, we have proved that the Platonic idea as embodied in the nebular hypothesis *does* lead to the present configuration of the solar world. We make these remarks to show that the idea we advocate is old and venerable; we hope, at some other time, to give the highly interesting history of the law of planetary distances, including the application of the Phyllotaxis, (Pierce, Agassiz,) the radius of gyration, (Kirkwood,) the regular polyhedra, (Kepler, Plato,) etc.

How grand and beautiful is the harmony of the planetary world! What an admirable *unity of plan* is manifested therein! As *now* the planets are slowly sinking to the sun, so they have *always* been sinking since the moment of their creation as a nebulous mass; the same motion that now brings them nearer to their death has caused their formation, has brought them to life! And how sublime is the plan of creation! To call forth the harmonious system of the solar world with all its multiform aspects and dependencies fit to support life throughout almost endless ages—nothing but a collection of matter endowed with its molecular forces was placed in a little spot of the house that contains many mansions besides. This matter slowly collected together. In thus following the force of attraction planted in it by eternal love, the whole great life of the solar world was awaked; and as the pulsation of the heart in man indicates the fleeting moments of his life, so the pulsations of that great whole, succeeding each other at equal intervals, gave each one birth to a new world to mark the historic epochs of the Universe by its position and to roll on for ages, a revelation of the Great Author, until, always following the same attractive force, it in death finds rest at the bosom of the planet-mother, the sun. And then—this grand system remains as a mere lump, a *Cosmic Fossil*, suspended in space, where perchance some higher being may meet with it, touch it, investigate it, and construct its whole past history, as the geologist in our days studies the history of a fossil shell!

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[7] Brewster, *Life of Newton*, Ch. 16.

[8] *Dialogo intorno ai due massimi Sistemi del Mondo, Tolemeico e Copernicano.* Gjornata I, (ed. *Opere*, Firenze, 1842. Vol. i, p. 34–35.) He finds: le grandezze dei cerchj, e le velocità dei moti s'accostano tanto prossimamente a quel che ne danno i computi, che è cosa maravigliosa.

Iowa City, Iowa, July—November, 1864.

ART. XXXV.—*The determination of the height of Auroral Arches from observations at one place*; by H. A. NEWTON.

IN the displays of the Aurora Borealis the luminous cloud often takes the form of an arch. Sometimes the lower boundary of the auroral light is arch-shaped. Beneath is a dark segment, while perhaps streamers run upward from the mass of light. Again, there is sometimes a bank of light in the north, resting apparently on the horizon. The upper boundary of this bank forms a more or less regular arch. Again, there is sometimes a narrow band or bow of light, spanning the heavens, coming down to within two or three degrees of the horizon at each extremity, having one or both of its edges sharply defined, and being often only two or three degrees in breadth.

The arch in each of these three cases may be incomplete, or broken, or otherwise irregular. But there is a manifest tendency to form a regular curve. This curve, that is, the boundary line of the arch, or the axis of the bow, is rarely, if ever, an arc of a great circle. It cuts the horizon at points notably less than 180° from each other. It has apparently the same law of formation in each of the three cases. Its peculiar shape is therefore probably due to a single cause.

There is no reason to believe that each observer sees a different arch, just as each sees his own rainbow. There is no center of light beneath the arch, and moreover a decided parallax is very frequently found. The curve of the auroral arch has then a definite locus in the atmosphere.

This curve is not caused by mists in the atmosphere obscuring and revealing parts of an indefinite cloud. For the arch has little or no relation to the horizon, and cuts it at all angles.

It is not a straight line, for the arch does not cut the horizon at points 180° from each other.

The arch resembles the projection of a portion of a circle, or a sphero-conic. The venerable Hansteen has in two instances seen at Christiana nearly the whole ellipse. Prof. Twining has observed, at Middlebury, Vt., in one instance at least, an arch in which the extremities of the major axis of the ellipse were visible above the horizon.

It is reasonable to infer that, in general, the locus of the light is parallel to the earth's surface. For the arch has the same general form at all places, as will be seen in the diagrams of Mairan and others.

This leads naturally to the hypothesis of Hansteen,[1] that the auroral arch is a real ring, which in its normal form is parallel to the earth's surface, and is symmetrically placed about the magnetic pole. The dark segment is seen when we look beneath the ring into space beyond. The bank of auroral light is a similar broader or more distant ring.

The results of Prof. Loomis's investigations respecting the geographical distribution of the aurora[2] confirm and modify this conclusion. He shows that there is a narrow belt of an elliptical form surrounding the magnetic and astronomical poles of the earth, and at a considerable distance from them, which is the region of the greatest and most frequent displays of the aurora. It is reasonable to infer that an aurora of considerable intensity would naturally take a form symmetrical with this narrow belt of the earth's surface. The portion of the curve which we see at any instant should be regarded as part of a circle whose center is the center of curvature of the nearest portion of this belt.

To obtain the parallax of the auroral cloud, observations at two distant stations have been necessary. These have to be made upon a moving object, the time of whose appearance cannot be predicted. It is only by a happy chance that good observations can be secured. If the height can be computed from measures made at a single station, a great advantage is gained. A second observer is not essential, if the position and shape of the auroral cloud is assumed to be as described above.

The distance on the earth's surface from the observer to the center of curvature of the nearest portion of the belt of frequent auroral displays can be measured. Represent this by d. Let the apparent altitude of the auroral arch be h, and its amplitude on the horizon be $2a$. Let x be the height of the auroral cloud from the earth, R the earth's radius, b the distance from the observer to the point on the earth directly under that part of the cloud which forms the vertex of the arch, and c the distance in like manner from the observer to the point underneath that part of the arch seen in the horizon. We have then these equations,

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[1] Mèmoires de l'Académie Royale de Bruxelles, tome xx, p. 118. See also B. V. Marsh, this Journal [2], xxxi, 311.

[2] This Journal [2], xxx, 89.

$$\mathrm{R}+x=\mathrm{R}\sec c=\mathrm{R}\cos h\sec(b+h)\,; \quad . \;\; . \;\; . \;\; . \quad (1)$$

and since d, $d-b$, and c, are the three sides of a spherical triangle, and a the angle,

$$\cos(d-b)=\cos d\,\cos c+\sin d\,\sin c\,\cos a. \quad . \;\; . \;\; . \;\; . \;\; . \quad (2)$$

From these equations b and c may be eliminated, and x found in terms of a, d, h, and R.

From (1) $\cos(b+h)=\cos h\,\cos c$, and hence

$$\begin{aligned}\cos(d-b)&=\cos\{d+h-(b+h)\}\\ &=\cos(d+h)\cos(b+h)+\sin(d+h)\sin(b+h)\\ &=\cos(d+h)\cos h\,\cos c+\sin(d+h)(1-\cos^2 h\cos^2 c)^{\frac{1}{2}}. \quad (3)\end{aligned}$$

Equating (2) and (3) and dividing by $\cos c$,

$$\cos d+\sin d\,\tan c\,\cos a=\cos(d+h)\cos h+\sin(d+h)(\sec^2 c-\cos^2 h)^{\frac{1}{2}}. \ (4)$$

But $\cos d=\cos(d+h)\cos h+\sin(d+h)\sin h$. Substituting, dividing by $\sin(d+h)$ and placing $\sin\varphi$ for $\sin d\cos a\,\mathrm{cosec}\,(d+h)$ we have

$$\sin h+\tan c\,\sin\varphi=(\sec^2 c-\cos^2 h)^{\frac{1}{2}},$$

Reducing, $\tan c=2\sin h\,\sin\varphi\,\sec^2\varphi.$

Hence, to compute the altitude of the auroral arch above the earth's surface, we have the three equations,

$$\sin\varphi=\sin d\,\cos a\,\mathrm{cosec}\,(d+h),$$

$$\tan c=2\sin h\,\sin\varphi\,\sec^2\varphi,$$

and $x=\mathrm{R}(\sec c-1).$

To apply these observations to particular cases I have selected 25 or 30 auroral arches, observed by Pres. Stiles, Prof. Olmsted, Mr. Herrick, and Mr. Bradley. They were all observed at New Haven, except those in the year 1860, which were seen by Mr. Bradley at Chicago. Most of these observations are from the Auroral Registers of Mr. Herrick and Mr. Bradley. These Registers will form part of a volume of memoirs about to be published by the Conn. Acad. of Arts and Sciences. The value of d is assumed to be 32°, which is also very nearly the distance from New Haven to the magnetic pole of the earth. In the following table are given the dates of the auroras, the observed apparent altitudes and amplitudes of the arches, and the computed values of x in miles, and in kilometers.

In selecting from the Registers the arches for this table, I have omitted those which were low in the north, as the horizontal extent is then concealed by the mists. I have also omitted those of which the observations were indefinite, or seemed imperfect.

Table of the observed altitudes and amplitudes of auroral arches, with the computed heights above the earth's surface.

Date.	Altitude.	Amplitude.	Height in miles.	Height in kilometers.	Observer.
March 27, 1781	66½°	165°	83	53	Stiles.
Sept. 21, 1840	10–12	120	52–68	84–109	Herrick.
March 6, 1843	5–8	100	42–83	67–133	"
April 13, 1845	8	100	83	133	"
April 27, "	10	90±	155	250	"
Oct. 9, "	8	100	83	133	Bradley.
Oct. 19, 1846	8–10	80±	165–214	266–345	"
Dec. 9, "	5	50	281	452	Herrick.
May 15, 1847	7–8	80	142–165	228–266	"
June 12, "	5	50	281	452	"
Aug. 4, "	4	60	154	248	Bradley.
" " "	7	90	98	158	"
Sept. 29, "	10	70–80	290–214	467–345	Herrick.
Nov. 25, "	10–15	100	111–183	179–295	"
May 18, 1848	7–9	75±	168–222	270–358	"
Oct. 23, "	5–6	70	134–165	215–266	Bradley.
March 18, 1849	10–15	100	111–183	179–295	Herrick.
April 6, 1850	10–12	90	155–193	250–310	Bradley.
Feb. 18, 1851	23	120–130	154–104	248–168	"
March 18, "	12	120	68	109	Olmsted.
Sept. 29, "	8	90	118	190	"
Sept. 29, 1852	10–15	100	111–183	179–295	Herrick.
March 26, 1860	6±	90±	80	129	Bradley.
March 27, "	10	90–100	155–111	250–179	"
July 4, "	10–12	100+	111–140	179–226	"
Aug. 12, "	5	90±	62	100	"
" " "	7	90±	98	158	"
Aug. 17, "	8	100±	83	133	"

The average height as indicated by the table is 134 miles, or 215 kilometers. The observations were not taken with reference to the use here made of them, and the results can therefore be regarded as only approximately correct. Mr. Bradley informs me that his method of determining the amplitude of the auroral arch was to place himself with his face directly towards one end, and then compare the amplitude with the arc of 90°, which was easily estimated. He was accustomed to consider the curve of the arch as continued down until it cut the horizon.

This method of determining the height of the auroral arch is imperfect in that it supposes for it a given regular form. In fact the auroral cloud is usually more or less irregular. Yet in view of the very great difficulties in securing good observations for parallax at two stations, the method is believed to possess very decided value, both independently, and as a check upon other measurements. It furnishes moreover the means of determining not only the magnitude of the auroral cloud, but also the breadth and height of the streamers which often rise from the arch-shaped mass of light.

Yale College, March 18th, 1865.

Art. XXXVI.—*On the Iron Ores of Marquette, Michigan;* by J. P. Kimball, Ph.D.

It is proposed in the present article to record a few observations made, in the month of May last, in the Iron region of Marquette county, Michigan, with the view of elucidating certain obscure points in the geology of the immense beds of earthy red hematite, specular and magnetic iron ores, with which this district abounds to an unparalleled extent, constituting it beyond any doubt the most opulent source of iron in the world yet discovered. It was not my privilege carefully to explore this region beyond a limited district near the middle. I am therefore unable to add any information as to the geographical extent of the iron deposits to the published results of Foster & Whitney's survey. Indeed, the following contribution to the knowledge of this region is chiefly by way of observation of the instructive rock-cuttings in the iron mines, or rather quarries, and in one particular cutting of the Peninsula Railroad, all of which have been presented to geological study since the close of the government survey; and, inasmuch as neither population nor industry has advanced beyond these quarries, no other advantage to the geologist at present exists over the means of exploration employed by the United States geologists at that time. It is appropriate to remark in this connection that it must certainly be a cause of great regret to all friends of natural science and mineral industry in this country, that the federal government has omitted to publish the special report upon the Marquette Iron Region by Mr. J. D. Whitney, the results of a second geological exploration.

In their survey of that portion of the large area of the Northwest exclusively occupied by schistose, metamorphic and crystalline rocks, which is included in the northern peninsula of Michigan, Messrs. Foster & Whitney in 1851 marked out the outlines of two distinct systems of crystalline rocks, one of which was defined as metamorphic, their Azoic proper; while the other, distinguished as a great development of granitic rocks outstretching in separated expanses, was described under the name of granite belts or ranges. The relation to each other of these two systems was hardly traced to conclusiveness owing to the concealment of their conditions of contact; but the latter was assigned to an origin later than the Azoic series upon the testimony of disturbances of the upper metamorphic strata by intrusive masses of the granite, and earlier than the Silurian, as the lower beds of the Potsdam sandstone are seen to rest undisturbed around them.[1] The distribution of the Azoic and granite rocks, as laid down by the federal geologists, is presented in their map of the district between Keweenaw Bay and Chocolate River, to which reference can readily be had without encumbering these pages with a verbal recital.

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[1] Report on the Geol. of the Lake Superior Land District, Part II, 1851, 44–48.

The metamorphic or Azoic rocks, consisting of gneiss, hornblendic, talcose and chloritic slates, and beds of argillite, novaculite, quartzite, conglomerate, saccharoidal marble and crystalline limestone, were described as existing under conditions of great displacement, and displaying evidences of metamorphism, particularly in vicinity to the line of contact with the granite or igneous outburst. The occurrence of granite within the area of their distribution was recognized in the form of intrusive masses, while the greenstones and some of the schists, both following the general stratification, were regarded as trappean overflows, the former as an igneous product, the latter as pulverulent greenstone in the form of volcanic mud. The iron-ores of this series of rocks were described as alternating with the trappean ridges in the form of intrusive masses, and the remaining phenomena of their dissemination in many rocks of the series, were ascribed to processes of elimination and mechanical admixture prevalent during the accumulation of the original sediments. This explanation was believed to meet the mode of occurrence of the larger masses, which, at the time it was proposed, had not been extensively uncovered, and manifestly, as appears in the concluding paragraph of the chapter on iron-ores,[2] was not meant to be extended to beds of specular and magnetic oxyds of iron included within metamorphic strata, with a conformable range and dip, which indeed the federal geologists explicitly state their disposition to regard as the result of aqueous deposition.

The granite belts were defined in general terms to occupy an area of more than 2000 square miles, and, within the boundaries of Michigan, to be developed in two distinct ranges or spurs, of which the northern, including the Huron Mountains, forms the coast between Presqu'isle and Granite Point, and expands westward to a width of 25 miles. Along its southern intersection with the metamorphic series—its longest direction—it is laid down as continuous for sixty miles. It is separated by a zone of metamorphic rocks of some fifteen miles in width, which embraces the present iron industry of the region, from the southern belt, which is of very irregular outline, though nearly parallel for a distance of 36 miles along its northern intersection with the metamorphic belt. The large expanse of crystalline rocks occupying the undeveloped tract of the northern part of Wisconsin, stretching across the head waters of the Mississippi to the north and west of Lake Superior, was considered by the federal geologists to be the continuation of these granite belts. The granite was described as forming numerous parallel ridges bearing east and west, and in both belts to indicate leading axes of elevation. Gneiss was observed to flank the granite both on the north and south with intercalated beds of quartzite, potash, feldspar and hornblendic schists, the whole dipping uniformly from the axes of elevation. Hornblende sometimes was seen to replace mica, forming a light grey syenite. The granite south of the metamorphic belt is generally made up of feldspar and quartz, mica being almost always wanting, or very sparingly present. Feldspar is the most predominating mineral, large bodies of flesh-red orthoclase being of common occurrence. Two systems of dykes, and extensive intrusive masses of greenstone —the larger of a bearing uniform with the axes of elevation, the smaller being N.E. and S.W.—were indicated as occurring throughout the distribution of both belts. These are intersected by veins of quartz.[3]

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[2] Foster & Whitney, ibid, 68.

A most engaging, and, at the same time, a very wide field of inquiry, is presented to geologists in addressing their attention to the ancient crystalline rocks of the Northwest, with the object of resolving the distinctive distribution of the Laurentian and Huronian series.

Although no detailed description of the crystalline rocks of the northern peninsula of Michigan has been given, so far as I am aware, since the close of the survey of the Lake Superior Land District, a noticeable disposition prevails among geologists to assign to the metamorphic strata, distinguished by Messrs. Foster & Whitney as Azoic, an equivalency with the Huronian series of Canada.[4] Such a tendency appears to be due to the investigations upon the south shore of Lake Superior of Mr. Alexander Murray, of the Canada Geological Survey,[5] the results of which have been employed by the provincial geologists in their comparative study of the crystalline rocks of Canada and its bordering districts.

The lines of demarcation between the Azoic strata and the granite, as laid down by the Land District Survey, appear to have been determined by the succession of beds rather than upon any evidence of unconformability. Orthoclasic gneiss, succeeded by alternating beds of quartzite and hornblendic slates, flanks the granite as it falls away from its axes of elevation, and the points at which these have been observed to be succeeded by argillaceous, chlorite, and talcose schists, and beds of conglomerate, afford the only clue to the boundaries of the

[3] Foster & Whitney; ibid, chap. iii.

[4] Geol. of Canada. 66, 596; Dana's Manual of Geology, 143.

[5] This Journal, [2], xxxi, 394.

two systems of rock, thus far presented. It has not been claimed for the information upon the geological structure of the region in question, that the relation of the Azoic series with the granite ranges, has been established by sufficient or definite data. This must continue to remain an open question until it is decided by patient and careful observations, facilitated by the development of the district, which, up to within a short period, was almost unvisited by white men, and still presents formidable physical obstructions to the work of geological exploration, scarcely less in degree to those experienced by the Land District Survey, and explained in the report of Messrs. Foster & Whitney. The recent direction of attention to the Silver-lead region, so-called, or the Huron Mountains, has served somewhat to open this obscure region during the past summer, by the laying out of roads, and the commencement of mining operations. Although the district has been pretty thoroughly explored during the past season in a spirit of mining adventure, it has engaged no particular study with a view to scientific results. It is proper to state, however, that upon the evidence of explorers and collections, obtained at Marquette, the metamorphic character of the Northern range is demonstrated by numerous instances brought to my notice, of the extensive development in that region of orthoclasic gneiss—often granitoid, quartzite, massive feldspars, talcose, hornblende and chlorite slates, seams of graphite and magnetic iron ore. My own observations in the Iron region impressing me with the indigenous character of the larger masses of diorite and granite represented within the defined area of the metamorphic strata, and their entire distinctness from intrusive dykes or erupted masses, and concurring in the recognition of these strata by Mr. Murray as Huronian, I am disposed to regard the entire region as of metamorphic character, all of whose larger masses of crystalline rocks are indigenous, and to be divisible into the two formations, Laurentian and Huronian: the former formation probably forming the surface of the areas known as the granite ranges, while the latter probably occupies, with minor deviations, the limits laid down for the crystalline schists comprehended under the name of Azoic.

This view is fortified by the fact that the Huronian or Azoic series is interposed between the granite or Laurentian rocks and the Potsdam sandstone, thus bringing the Laurentian in its proper place below the Huronian; while the only argument militating against it is the observation by Mr. S. W. Hill, of intrusive granite at one point cutting the slates near their southern line of intersection with the main granite, whence is derived the inference that the slates antedate the granite. The indigenous character of the greater part of the granite, however, throughout its general distribution, connected with the fact of its coëxtensive association with metamorphic sedimentary strata, tends in this instance to invest the intrusive granite with characters distinct from those of its greater development, and to impart to it the special phenomena of an independent dyke. Apart from the evidence of superposition, the lithological features of the Granite Belts, so far as they have been indicated, bear a marked analogy with the standard characters of the Laurentian series as determined in Canada and upon the north shore of Lake Superior.

In a private letter from Mr. T. Sterry Hunt, acquainting me with some of the unpublished leading results of Mr. Murray's examinations upon the south shore, I am informed that this geologist, whose opportunities for comparison cannot be too highly appreciated, describes the islands and some points on the main land north and northwest of Presqu'isle as of Laurentian gneiss with hornblende, epidote, and pyroxene, arriving at this conclusion through the observation to the south of these points of the greenish slates, greenstone, dolomitic limestones and conglomerates, associated with the beds of iron ore, and their relations to the gneiss. This latter series he judges to be the equivalent of the Lower slate conglomerate, or chloritic slates, at the base of the Huronian system, regarding them here as retaining nearly the same relative conditions as upon Lake Huron where the dolomite marks the division between the Upper and Lower slate conglomerates.

The gneiss which marks the boundary of the granite belts, and accordingly characterizes the top of the Laurentian series in this region, the same as it is elsewhere represented, is succeeded by dark colored hornblendic schists, which consequently represent the base of the Azoic or Huronian series. These schists are followed by a series of augitic rocks and schists, interstratified with magnesian hydrous rocks and slates, the two kinds of rocks being represented on the one hand by hypersthene, pyroxene, and bedded diorite passing into dioritic slates, and on the other by talcose and chloritic schists. The former character of rocks prevails to such an extent as to impart to the lower members of the Huronian series a distinctive augitic aspect. The several rocks composing this augitic zone are commonly of a greenish color, and vary in this respect chiefly as to shade, resembling in this particular the Lower slate conglomerate which marks the base of the series in Canada, and from which they seem to differ only in the absence of pebbles and boulders from the subjacent Laurentian rocks, which there form a distinguishing feature. Messrs. Foster & Whitney, however, describe as occurring upon Little Presqu'isle a pure white feldspar containing acicular crystals of hornblende, passing into separate beds of each of these component minerals, and enclosing angular fragments of hornblende and chlorite slates, jasper and a green magnesian mineral.[6] It seems not unlikely that this rock in its further distribution may possess more decidedly the characters of a conglomerate, and accordingly the more closely bear out the lithological analogy between the lower members of the Huronian series near Marquette, and their well ascertained characters north of the great lakes.

The hornblendic beds are succeeded by thick beds of pure quartzite displaying ripple marks, passing into quartziferous conglomerate on the one hand and siliceous slates on the other. The more massive beds of this description form a prominent feature of the country through their outcrop in an east and west ridge traversing the middle of the Azoic or Huronian area. They dip at high angles to the south, though numerous less elevated beds dip to the north. They are associated with greenish hornblendic slates and more or less crystalline diorite, and at their base with bands of dolomite somewhat siliceous and highly altered. Overlying this quartz zone, the probable equivalent of the Upper slate conglomerate of the North Shore, occur talcose, argillaceous and siliceous schists interstratified with specular iron ore, earthy red hematite and specular conglomerates, the latter resting immediately upon thick masses of hornblende rock blending into bedded diorite and hyperite on the one hand, and on the other into chlorite schists, and siliceous serpentines. At some points a dark colored quartzite resembling greisen except in the presence of hornblende instead of mica, and clearly of detrital origin, next underlies the ore beds.

Possessing the same stratigraphical conditions as the schistose rocks, while many varieties of them are represented in the schists by their exact counterpart as to composition, the crystalline Huronian rocks must be regarded as essentially metamorphic, while in a comprehensive view of the whole series it is seen that together with variable conditions of deposit, it indicates variable degrees of local metamorphism.[7] Plentiful evidence exists of the blending of a rock of one character into that of the other, or the continuity between crystallized and schistose beds. The diorites and hyperites of this region are never porphyritic, but on the contrary are so fine-grained as rarely to admit of the mechanical separation, or even the identification of their ingredients.

Besides the indigenous crystalline rocks distributed throughout the Huronian series, exotic or intrusive crystalline rocks are met with, but only in the form of dykes, and limited to a narrow distribution. The numerous elevated protuberances and ridges of granitoid gneiss, rising above the uppermost Huronian schists which rest upon their flanks apparently unaffected except by the regular convolutions of the entire series, are undoubtedly of Laurentian age. Two systems of greenstone dykes, according to Mr. Hill, as cited by Messrs. Foster & Whitney, traverse the gneiss—the one bearing N. from 70° to 80° W.—the other N. 10° E., and the same authorities have noticed many instances of the intersection of the schists by intrusive granite, especially near the borders of the granite ranges.[*] Not having enjoyed the opportunity of following Mr. Hill's notes, I am unable to confirm these observations; but it may not be inappropriate to suggest the probability that the larger and more persistent bodies of greenstone bearing approximately east and west—that is conformably with the axes of the folds which constitute a regular system of flexures coëxtensive with the distribution of the Huronian series in the vicinity of Marquette, are in reality indigenous greenstone, and a portion of the development of the diorite upon which repose the upper members of the series, and which, as will hereafter be shown, is uncovered along most of the ridges of the region. No instances of trappean overflows in this region have been brought to notice. Quartz veins, sometimes carrying the sulphurets of copper, lead, and iron, are of frequent occurrence, occupying fissures evidently of an extensive vertical range. Workable veins of copper pyrites have recently been discovered. This is another point of agreement between the features of the Huronian series as developed in this district, and upon the north shore of Lake Huron, where copper-bearing veins are frequent, of which those of the Bruce, Wellington and Wallace mines are well known examples.

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[6] Foster & Whitney's Report, ii, p. 18.

[7] Compare Foster & Whitney's Report, ii, 16.

It is urged by Prof. Rivot of Paris, that the crystalline and schistose rocks south of Keweenaw Bay entirely correspond with the rocks of Keweenaw Point; for he asserts the syenite of the Bohemian Mts. to be equivalent to the Granite Ranges, or to the granite of the Huron Mts.; and the metamorphic schists of the Marquette district to be the stratigrapical equivalents of the intercalated greenstones, amygdaloids, sandstones and conglomerates, known as the Upper copper-bearing series, or the Quebec Group. Mr. Rivot thus distinguishes between the general characters of the rocks composing the Bohemian Mts. and the overlying series of beds, and believes to have discovered in the former, which he regards as erupted, the source of the elevation and displacement of the latter, which he regards as more or less completely metamorphosed sediments.[*]

Not to discuss at present the peculiar notions of Mr. Rivot, it will be well to advert very briefly to certain remarkable misconceptions upon which they are based. First: There seems to be no evidence in support of Mr. Rivot's assumption that the greenstones associated with the hornblendic schists of the Marquette district are, like the copper-bearing rocks of Keweenaw Point, "traversed by veins containing copper and native silver." On the contrary, the vein phenomena of the crystalline schists forming the Azoic belt of Marquette county, are decidedly characteristic of the Huronian series, the lodes of which carry the sulphurets of copper unaccompanied by silver, and are altogether without the distinctive properties of the native copper lodes of Keweenaw Point. Second: The Paleozoic strata among which the greenstones of Keweenaw Point are intercalated conformably, and within the sectional development of which they are included, rest unconformably upon the crystalline schists of the Marquette district, and hence the stratigraphical identity between the rocks of these two districts, inferred by Mr. Rivot, is clearly out of the question. Third: The difference between the rocks composing the Bohemian Mts. and the superincumbent series is one merely of special lithological characters, and not one of general structure as assumed by Mr. Rivot. The syenite of Mt. Houghton has recently been found by Mr. W. H. Stevens, of Copper Harbor, to be intercalated upon the Reliance location with several massive beds of sandstone, conglomerate and amygdaloid, thus establishing uniform conditions of composition and structure to prevail throughout the entire series of the north-dipping copper-bearing rocks of the South shore.

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[*] Foster & Whitney's Report, ii, 44.

[*] Rivot, Notice sur le lac Supérieur—Extrait des Annales des Mines, x, p. 50.

Mr. Rivot was one of the first to assert the metamorphic character of the greenstones of the Marquette district, and, upon grounds of structural analogy alone, opposed the generally received opinion that the bedded syenites, diorites, dolerites, chloritic, zeolitic and epidotic amygdaloids, and so-called traps of the copper-bearing series, are erupted masses, having originated from successive overflows during the accumulation of the Paleozoic sediments in a horizontal position. Notwithstanding the stratigraphical arrangement of this series upon the South shore entirely bears out the theory of their metamorphic origin, certain observations of the Geological Survey of Canada upon the North shore tend to strengthen the view advanced by Messrs. Foster & Whitney.[10] If the explanation of trappean overflows on Thunder Bay, resting upon the authority of Sir William Logan, be confirmed by future investigation, the North shore must be regarded as the geological type of the Lake Superior country, and the absence of similar phenomena upon the South shore attributed to effects of denudation. If, on the other hand, the trappean overflows of Thunder Bay be found disconnected with the lower traps of the Upper copper-bearing series, but be ascertained really to belong to the system of dykes which traverse

[10] Geol. Survey of Canada, p. 70.

this series on the North shore, and which have no counterparts in Michigan, it will remain for special lithological investigation to complete the evidence of the metamorphic origin of the Upper copper-bearing rocks of Lake Superior on the grounds of analogy with the conditions of their development in Canada. That the question has been left open by the Canadian geologists, is apparent from the fact that the stratified greenstones of this series, in a chapter by Mr. T. Sterry Hunt, are described as the stratigraphical and lithological equivalents of the diorites of the Quebec Group examined by him in Eastern Canada, which he asserts to be altered sediments, and to pass into chloritic, epidotic, and hornblendic slates.[11]

Allusion has already been made to the regularity of the flexures of the Huronian strata in the Marquette district, and to the general parallelism, and approximate east and west bearings of their axes. This regularity prevails throughout the Iron region proper, of which the hills and valleys conform to the resulting system of undulations—thus corresponding to anticlinal and synclinal arches and basins. The distance from one axis to the next of the same character varies from one half of a mile to one mile. Departures from the uniformity of direction occur at different points, but they may generally be directly traced to local causes of displacement. The plications in the strata which thus impart to the district its topographical configuration, have been subjected to powerful agencies of denudation, and, consequently, by the unequal operation of these agencies, are more or less imperfectly preserved. Hence their regularity is less superficial than stratigraphical; for while through the general denudation of the district, in common with the larger portion of the continent, by glacial action, the original surface of the rocks has been removed in a comparatively even manner, local agencies of erosion and excavation have served to break the continuity of ridges, and entirely to obliterate prominent outcrops over considerable areas.

Of the various indications of drift agencies with which this region abounds, the rounded and polished rocks or "*roches moutonnées*," and the glacial furrows which these surfaces retain, are the most frequent and instructive. It is almost impossible to meet with an outcropping rock whose character is such as to resist weathering influences, the surface of which does not exhibit one or the other, or both of these appearances. The course of the glacial grooves is invariably N. from 50° to 70° E., quartering with the direction of the valleys. Two, and not seldom three, systems of furrows can be traced, varying however but few degrees and quite alike as to distinctness. The rounded, polished and grooved surfaces are seen upon the flanks of the ridges, as well as upon their summits, and are invariably revealed by the removal from the surface of a rock of the alluvium and glacial drift by which it has been effectually protected. It is only in the outcrop of fissile schists that these appearances are wanting. The dense vegetation and the undisturbed covering of drift, aided by the deep snows of winter, almost entirely prevent the weathering of the rocks, so that the phenomena of glacial action are exhibited in this region in uncommon perfection. Fragments of rock occurring near by *in situ*, and boulders of specular iron are scattered throughout the drift, which here generally in fact appears to be made up of material derived from the immediate region, as seen especially in the circumstance that it is exceedingly ferruginous, while the source of the iron as a local detritus is evidenced by its exclusive presence as anhydrous sesquioxyd. Foreign boulders of gneiss occur however, considerable in size and number within limited areas of distribution.

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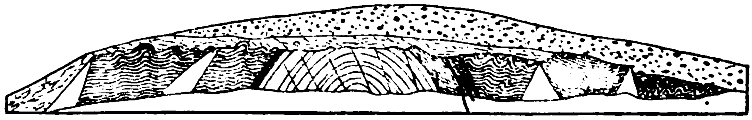
[11] Geol. Survey of Canada, pp. 612, 614.

The position of the beds of specular iron ore has already been stated to be at the top of the Huronian series as developed in the Marquette region, and it has also been remarked that they are interstratified with talcose and argillaceous schists. Sharing the plications of the entire series, these specular schists, as they may properly be called, are accordingly folded into synclinal basins and anticlinal crests—of which the axes in the case of the former lie below drainage, in the bottom of the valleys, and in the case of the latter are commonly obliterated through the erosion of elevated outcrops. Hence their outliers have been chiefly preserved along the flanks of the ridges where they have been the least exposed to the agencies of denudation, protected as they were against a drift current coursing not with the valleys but obliquely across them, by the elevated outcrops of their underlying rocks from the summits of which however they have been abraded. The denudation has been most extensive upon the purer specular schists and the earthy red hematites, while the only exceptions to these conditions of erosion and preservation as already given, occur with specular schists which acquire from their composition a refractory structure, or from their mode of deposit, the property of resisting the effects of a sweeping denudation.

The bosses of specular-iron—the iron-knobs or mountains, as they are called—are the most striking examples of exception to the general effects of denudation already noticed. They are instances of the preservation of the anticlinal crest, and owe their permanence to the fact that they are made up, not of pure and soft specular schists, but of specular iron ore closely interlaminated with quartz or jasper—a structure capable of resisting denuding action to a far greater degree than the homogeneous schists. The laminæ of jasper alternating with pure specular iron ore, vary in thickness from a hair's breadth to an inch, producing a banded appearance. The fact that the entire mass is highly contorted into minute as well as sizeable folds, furnishes an explanation of a power of resistance to vicissitudes of erosion, superadded to the same property which it derives from the mere presence of intermingled jaspery bands. The conglomeratic beds of specular iron ore and jasper, are generally preserved in elevated outliers, acquiring a refractory character from their coarse and tenacious structure, but I have met with no instance of the preservation of the anticlinal crest in beds of this description.

In Section 26, T. 47 N., R. 27 W., an example of a preserved anticlinal crest occurs in a ridge of half a mile in length and 170 feet in height. The specular iron-ore at the surface displays a laminated and contorted structure into which enter thin laminæ of quartz, and although the mass in itself is not so substantial as the exterior of the Cleveland Knob, its attending conditions of deposit impart to it similar refractory properties. In this region, as elsewhere, denudation may be noticed to have had less effect upon those stratified rocks which present to its agencies their planes of stratification, than upon those which are highly inclined. Hence it wiil be observed that the more extensive the sweep of the undulations the more complete is the preservation of the specular iron-ore which in the form of schists enters into them.

The most conclusive indications of the stratigraphical conditions which prevail among the hematite schists in Marquette county up to this time exhibited, are disclosed by the cutting of the Peninsula Railroad in the N.E. quarter of Section 8, T. 47 N., R. 26 W., to which allusion was made in the first part of this article. The grade of the railroad has been laid in an excavation 600 feet long and sunk 25 feet below the highest point upon the surface of a hill which derives its configuration from a boss of hematite schists intercalated with argillite. Thus the cutting presents an anticlinal section, as seen in the accompanying woodcut. The lower strata penetrated are preserved in an unimpaired anticlinal crest, while the arch of the upper, sharing the general denudation of the district, has been abraded with the preservation only of the deflected dips of these strata. The schists are alternations of earthy red hematite and chloritic argillite, or the peculiar greenish slates, already described, both becoming gradually reduced in an ascending order from comparatively heavy beds of two feet to thin laminæ of a quarter of an inch. The



power of resistance in the arch is strikingly illustrated; for while the crest itself remains perfect, the same strata have been much disrupted and displaced as they pass out of the arch and change their curvature. The upper laminated portions are exceedingly wrinkled, and the heavy beds broken and doubled. Under these circumstances of displacement but one or two of the heavier beds of argillite are exhibited above the grade of the railroad; and while these, through scales or plates retain marks of stratification so as to expose their true character, they have been sharply folded and collapsed, and by their nearly vertical position have the appearance of intrusive masses. The central portion of the cutting—that is, the undisrupted strata—is traversed by a system of parallel joints which the disrupted portions are without. Several segregated veins of quartz with limonite occupy the divisional planes of stratification.

Other sectional cuttings in the several mines or quarries display more or less completely the stratification and undulations of the hematites in common with the Huronian schists of this region, and show conclusively, if further testimony were requisite, the existence of the iron ore under the same conditions of deposit and secondary modification as the schists with which they are associated. In the Jackson mine, for instance, occur two bosses or short anticlinal folds, while in the Lake Superior mine a perfect synclinal flexure is exhibited.

Beds of specular conglomerate are of frequent occurrence throughout the Iron region of Northern Michigan, consisting of a paste of specular peroxyd of iron, through which are disseminated fragments of jasper, and rounded pebbles of specular iron ore which usually differ from the paste in texture, a difference very perceptible among ores of any one class even within narrow limits of distribution. These conglomerates not unfrequently resemble breccia in the angularity of the jasper fragments which they contain; but the pebbles of specular peroxyd, although sometimes obscure in a matrix of the same material, commonly serve to indicate the detrital origin of these beds. That they are derived from local detritus is evident from the fact that the jasper fragments are not rounded, while the particles of softer specular iron ore are worn but slightly. They seem to be of littoral formation and to have been derived from dismembered and crumbled deposits of successive laminæ of jasper and iron ore—similar to those deposits distinguishable in the bosses of the region. The specular conglomerate invariably exists under circumstances of true bedding, and is traversed by parallel joints splitting the imbedded pebbles. It occurs interstratified with talcose and argillaceous schists quite as regularly as the homogeneous ores. As would naturally be expected, the specular conglomerates, owing to their enduring composition, have resisted erosion more effectually than the purer schists, and are preserved in elevated outliers, as seen particularly at the New England mine, and in the N.W. quarter of Section 21, T. 47 N., R. 27 W. A specular conglomerate uncontaminated with any considerable portion of jasper, forms the bulk of the schists at present wrought by the Lake Superior mine. These are jointed obliquely, and are cleavable at right angles, to the stratification.

Specular schist often occurs charged with detrital quartz, or sand, thus differing from the conglomerates only in its external properties, while it is analogous to them in attitude and actual composition. Schists of this description are intercalated with the conglomerates in Section 21 above noticed, and in Section 26 of the same township are found underlying the laminated beds which crown the anticlinal arch.

An estimation of the thickness of the ferriferous series is attended with no little difficulty at present owing to the absence of any entire sectional cutting. The Jackson mine presents the best data for an opinion upon this point, although its workings expose neither the base, nor uppermost members of the series. It is upon the northern dip of a steep anticlinal, and the ground of the quarry is some 500 feet wide, the excavation having been entirely within the series. At this mine the range of thickness exceeds one thousand feet. Individual thicknesses of specular schist without the intervention of other schists, are upwards of 150 feet. Some idea of the massiveness of a homogeneous bed of specular schist may be had from the fact that in the month of September last 3500 tons of workable ore were thrown down at this mine in one blast, for which 11 kegs of powder were used.

It will be observed that while the smaller plications furnish the most available and complete evidences of the stratigraphical conditions of the ferriferous schists, every exposure of them in quarries or natural outcrops, conveys the same character of evidence, but upon a scale far more extended, and generally requiring allowance for superficial vicissitudes, and a large degree of denudation. Even if space permitted, I conceive it to be unnecessary to multiply instances of this evidence. It has been shown that the iron ores of the Huronian series in Michigan are essentially schists and heavy-bedded strata in which none of the phenomena of aqueous deposits formed by precipitation from water on the one hand, or by detrital accumulation on the other, are wanting. They exhibit not only stratification, anticlinal and synclinal folds, but are invariably traversed by systems of joints, and at many points exhibit a perfect slaty cleavage.

The intimate connection between the greenstones, hornblende rocks, and aluminous and magnesian silicated schists of the ferriferous series, has already been indicated in general terms, these rocks not only alternating with, but passing into, each other—a conjunction commonly existing wherever bedded greenstones, and isolated silicates occur together as sedimentary products of the decomposition of compound silicated rocks. The peculiar green slates which have a large development in this series, are intermediate in composition between clay slate and hornblende slate, and together with the talcose and chloritic slates, with which they are interstratified, are probably products of such a decomposition in the wet way of the same crystalline sediments which entirely or less undecomposed have gone to form those greenstones which constitute members of the same series. Effervescence with acids of some of the green slates in common with many of the greenstones of the series, together with the presence of a large amount of lime and magnesia, was pointed out by Messrs. Foster & Whitney, as indicating the characters of a pulverulent greenstone—the schalstein of German geologists.[12] The presence of carbonate of lime in many of the schistose rocks of this region must be regarded as generally due to the decomposition of silicate of lime in the silicates of the primary crystalline sediments whence they are derived, although in a few special instances it may have been derived by the subsequent conversion of greenstones into schalstones, or by infiltration from superincumbent calcareous strata. Chemical reactions in crystalline sediments resulting from the disintegration of crystalline silicated rocks, and operated upon by carbonated waters, are amply capable to have produced the lithological conditions of augitic rocks, clay-slates, schalstone, and other schists, together with the oxydized ores of iron intercalated with greenstone among the ancient crystalline rocks of North America as well as Europe—as shown by Bischof[13] in pronouncing the Neptunian characters of these rocks. From a stratigraphical point of view, while evidence is elsewhere often obscure, the Huronian greenstone, schists and iron ores of Northern Michigan, in the absence of close attention to their special chemical conditions, exhibit sedimentary and metamorphic phenomena adequate to render quite untenable, it is believed, the theory of the exotic character of any portion of them.

New York, Dec. 19th, 1864.

[12] Report of the Geology of the Lake Superior Land District, ii, 17, 93.

[13] Bischof, Elements of Chemical and Physical Geology, London, 1854–59, vol. iii, chap. 55. Volger, Studien zur Entwicklungsgeschichte der Mineralien. Zurich, 1854.

Art. XXXVII.—*Astronomical Photograyhy;* by Lewis M. Rutherfurd.

My present observatory is a circular brick building of twenty feet internal diameter, with a light revolving roof supported on twelve wheels which are fixed to the stone coping of the walls. The opening, two feet wide, extends from side to side with simple shutters, which, when elevated on the weather side, serve to prevent the wind from blowing into the observatory and shaking the telescope. Opening from the west side of the Equatorial dome is a small transit apartment with computing room attached. This observatory is in the garden of the house where I reside. The transit is 189 feet N.W. from the Second Avenue, and 76·3 feet N.E. from Eleventh street. It was erected in the summer and autumn of the year 1856. The equatorial, by Fitz, is a very substantial instrument, having circles divided on silver 18 and 20 inches in diameter.

The objective is of 11¼ inches aperture, and fourteen feet focal length, and was corrected for figure by myself after the methods and directions of Mr. Fitz. It is a fine glass, capable of showing any object which should be seen by a well corrected objective of those dimensions.

The observatory is low and therefore cannot reach any object near the horizon, but I prefer losing such observations to the tremors and expense of a high structure.

The transit room has been used on several occasions by the U. S. Coast Survey in their telegraphic operations for longitude. It is $0^h. 12^m. 15{\cdot}47^s$ E. of Washington, and in latitude 40°, 43′, 48″·53; the latitude being the result of observations with the zenith telescope upon twenty-four pairs of stars by the observers of the Coast Survey.

During the winter of 1857–58, Messrs. Alvan Clark & Sons constructed, and in the spring attached to the equatorial, a driving clock of the highest merit. It has a remontoir escapement similar to that of Bond's spring governor.

Having seen with great interest the photographic experiments conducted at the observatory of Harvard College, I determined, as soon as the clock should be in working order, to prosecute the subject of celestial photography. After many experiments it was ascertained that the best photographic focus of the objective was about $\frac{7}{10}$ of an inch outside the visual focus. I continued making photographs of the moon and such stars as could be obtained, and although when compared with what had been done by others the results gave reason for satisfaction, yet in view of what was desirable and apparently attainable, astronomical photography with me was a failure. By reducing the aperture of the telescope to five inches for the full moon, I was enabled to produce negatives which would bear an enlargement to five inches or fifty diameters. An impression of a sixth magnitude star was never obtained, γ Virginis, then 3″ distant, was the closest pair the duplicity of which could be measured on the collodion plate. The ring of Saturn and the belts of Jupiter were plainly visible, but entirely unsatisfactory. An image of Jupiter could be obtained in from 5 to 10 seconds exposure, but the satellites failed to impress the plate in any length of time. This was due to the uncorrected condition of the objective which diffused the violet rays over a large space, so that in the case of the planet each point of the picture was influenced not only by the ray due to that point, but by the stray beams from adjoining portions of the object, and thus nearly the whole actinic force of the objective was gathered within the dimensions of the image. In the case of the satellite the lost rays were not replaced by the wanderers from any adjacent point.

During the summer of 1858 I combined my first stereograph of the moon, producing quite a satisfactory result with the low power of the stereoscope. I do not know when this was first done in England by Mr. De La Rue, but with me the idea was an original one.

My greatest success with an uncorrected objective was in the pictures of the sun taken with about one-fiftieth of a second exposure, with the aperture reduced to one inch. The negatives were four inches in diameter and exhibited the spots with reasonable sharpness, the manifest difference in light between the center and the edge, and under favorable circumstances the faculæ. Some of the negatives verify the observation of M. Dawes, that the faculæ are elevations.

In June, 1860, the sun's disk was remarkably rich in spots, and I combined the pictures of two days to produce a stereograph, but the result was a failure and did not give the impression of a sphere, but presented the appearance of a flat uniform disk spanned by a spherical net-work which seemed entirely detached from the disk. This is attributable to a want of sufficient detail on the surface of the sun.

During the year 1859 and for a long time I worked with combinations of lenses to be inserted in the tube between the objective and the plate with the view of correcting the photographic ray. This attempt succeeded well so far as the center of the field was concerned, but it was impossible to produce a good correction over a space equal to the area of the image of the moon, without using a corrector of inconvenient size.

In 1860 I prepared a telescope with camera and instantaneous apparatus mounted equatorially to send by the U. S. Coast Survey Expedition to Labrador for the observation of the eclipse.

The objective in this case was a fine one, by Alvan Clark of 4¼ inches aperture. A ring was placed between the crown and flint lenses of such a width that the best visual and photographic foci were united. For this purpose it was necessary to shorten the combined focus about one-twentieth of its former value.

The pictures of the sun taken with this instrument were better than those made by my large telescope, in which no attempt had been made to correct the photographic rays.

Being unable to accompany the expedition, I made a series of pictures of the eclipse at home, upon which are seen the nuclei and penumbræ of the spots, the gradation of light of the sun's disk, and the serrated edge of the moon projected upon the sun. They show, however, none of the fogging of the moon's surface, commented upon by other observers, nor a greater intensity of light at the points of contact between the sun and the moon, both these results are, when they occur, due, in my opinion, to photographic or optical causes, and not to any true astronomical phenomena of that nature.

On examining the first negative of the eclipse I was struck by the difference of sharpness between the edge of the sun and that of the moon projected upon its disk. At first I was inclined to think that it was caused by a falling off in definition near the edge of the eye-piece used. In the next picture the edge of the sun was placed near the center of the field and the moon removed to a remote part of the plate, yet still the result was the same; the sun's edge was soft and indefinite while that of the moon was hard and sharp, showing that the light from the two objects comes to us under different conditions; in one case traversing the sun's atmosphere, in the other unaffected by this disturbing cause.

In the autumn of 1861 I began to experiment with a reflecting telescope with silvered mirror, which recommended itself both by the simplicity and ease of its construction and the entire freedom from dispersion. One was mounted of thirteen inches aperture and eight feet focus, of the Cassegranean form. It was ground and approximately figured by Mr. Fitz, and in its frame, as strapped to my large tube and carried by the equatorial clock, weighed less than fifteen pounds. Many modes were tried of silvering but the best results were obtained by Liebig's process, wherein the silver is deposited from an ammonia nitrate solution by sugar of milk. After three months trial I abandoned this instrument as unfit for use in my observatory. First, the tremors of the city, quite imperceptible in the achromatic, were, by the double reflection, increased about 36 times, an insurmountable obstacle to good work. Secondly, the silver deposit is so easily attacked, both by moisture and the gases which abound in the city, as to make it necessary to re-silver the speculum at least every ten days, a labor not to be contemplated with equanimity. Dr. Draper has found the silver surface very much more durable in the dry, pure air of the country. I regard the Cassegranean form as the best adapted to lunar photography, since the dimensions of the image can be varied at will, as circumstances dictate, by simply changing the small mirror, a number of which might be kept at hand.

Having thus failed in astronomical photography with an ordinary achromatic, with a correcting lens and with a reflector, I began, in the autumn of 1863, the construction of an objective, to be corrected solely with reference to the photographic rays.

In a former communication to this Journal, Jan. 1863, I drew attention to the peculiar adaptation of the spectroscope as a means of examining the achromatic condition of an objective, and since it was principally by the aid of this instrument that I have been enabled to procure a fine photographic correction, I may be pardoned for touching again upon this application.

The image of a star at the focus of a perfectly corrected objective would be a point, the apex of all conceivable cones having the object glass, or parts of it, as the bases. This point falling upon a prism would be converted in a line red at one end and violet at the other with the intermediate colors in their proper places. If, however, the different colored rays are not all brought to the same focus, the spectrum will no longer be a line, but in the uncorrected colors will be expanded to a brush the width of which will be the diameter of the cone where intercepted by the prism. It will thus be seen that a simple glance at a star spectrum will indicate at once what parts of the spectrum are bounded by parallel lines and consequently converged to one focal point, and what parts do not conform to this condition, and also the amount of divergence.

On applying this test I found that an objective of flint and crown in which the visual was united with the photographic focus, (in other words, where the instrument could be focalized on a plate of ground glass by the eye, as in ordinary cameras, and in the heliographs constructed by Dalmayer for the Kew observatory and for the Russian government), is a mere compromise to convenience in which both the visual and actinic qualities are sacrificed.

In order to bring the actinic portion of the spectrum between parallel borders, i. e. to one focus, it is necessary that a given crown lens should be combined with a flint which will produce a combined focal length about one-tenth shorter than would be required to satisfy the conditions of achromatism for the eye, and in this condition the objective is entirely worthless for vision.

Having obtained the achromatic correction, I had a most delecate task to produce the correction for figure, since the judgment of the eye was useless unless entirely protected from the influence of all but the actinic rays. A cell of glass inclosing a sufficient thickness of the cupro-sulphate of ammonia, held between the eye and the eye-piece, enabled me to work for coarse corrections upon α Lyræ and Sirius, but so darkened the expanded disk of a star in and out of focus that all the final corrections were made upon tests by photography, which gave permanent record of all the irregularities of surface to be combated. Still, however the process was long and tedious, dependent upon but three stars as tests, and they too often obscured by bad weather. My mode of correction was almost entirely of a local nature, such as practiced by the late Mr. Fitz and Mr. Clark for many years.

This objective was completed about the first of December last; it has the same aperture, 11¼ inches, as the achromatic, with a few inches shorter focal length, and can be substituted for it in the tube with great ease. The corrections of this objective are such that I think it capable of picturing any object as seen, provided there be sufficient light and no atmospheric obstacles.

As respects the light, I have obtained images of stars designated by Smythe as of the 8½ magnitude, and other stars on the same plate of full a magnitude lower. In the cluster Prœsepe, within the space of one degree square twenty-three stars are taken, many of which are of the ninth magnitude, with an exposure of three minutes. An exposure of one second gives a strong impression of Castor, and the smaller star is quite visible with half a second. With the achromatic objective it was necessary to expose Castor ten seconds to obtain a satisfactory result.

The great obstacle which prevents the results of photography from realizing the achievements of vision is atmospheric disturbance. In looking at an object the impression is formed from the revelations of the best moments, and it is often the case that the eye can clearly detect the duplicity of a star, although the whole object is dancing and oscillating over a space greater than its distance. The photograph possesses no such power of accommodation, and the image is a mean of all the conditions during exposure. It is, therefore, only on rare nights in our climate that the picture will approach the revelations of the eye.

Since the completion of the photographic objective, but one night has occurred, (the 6th of March), with a fine atmosphere, and on that occasion the instrument was occupied with the moon; so that as yet I have not tested its powers upon the close double stars, 2″ being the nearest pair it has been tried upon. This distance is quite manageable provided the stars are of nearly equal magnitude. The power to obtain images of the 9th magnitude stars with so moderate an aperture promises to develop and increase the application of photography to the mapping of the sidereal heavens, and in some measure to realize the hopes which have so long been deferred and disappointed.

It would not be difficult to arrange a camera box capable of exposing a surface sufficient to obtain a map of two degrees square, and with instruments of large aperture we may hope to reach much smaller stars than I have yet taken. There is also every probability that the chemistry of photography will be very much improved, and more sensitive methods devised.

On the 6th of March, the negatives of the moon were remarkably fine, being superior in sharpness to any I have yet seen. The exposure for that phase, three days after the first quarter, is from two to three seconds, and for the full moon about one-quarter of a second.

The success of this telescopic objective has encouraged me to hope that an almost equal improvement may be made for photography in the microscope, which instrument is more favorably situated for definition than the telescope, since it is independent of atmospheric conditions. Its achromatic status is easily examined by the spectroscope, using as a star the solar image reflected from a minute globule of mercury. Mr. Wales is now constructing for me a one-tenth objective, which, upon his new plan, is to be provided with a tube so arranged as to admit of the removal of the rear combination, and, in place of the one ordinarily used, one is to be substituted at will which shall bring to one focus the actinic rays.

Art. XXXVIII.—*Notes on Coal and Iron Ore in the State of Guerero, Mexico;* by the late N. S. Manross. (From an unpublished Report dated May, 1857).

The region here described is situated near the Pacific coast, just west of the meridian of the city of Mexico, extending 170 miles west of this line, and about 75 miles from the coast.

We have not succeeded in proving that true coal exists in this part of Mexico. Of the reported localities which we examined, the *first*, that of Chilpaucingo, proved to be a bed of soft and peaty brown coal, which might answer perhaps for local purposes, but would not bear transportation. The *second*, near Huetamo, was an irregular vein of asphaltum resembling the "Albert Coal" of Nova Scotia. The slates here bore a strong resemblance to those of the Coal-measures. Other rumored deposits in Tempantitlan and Los Nuevos turned out to be in one case pitchstone, and in the other black tourmaline. The people of the country have so little idea of what coal really is, that they are liable to mistake any black mineral for it. Their information is therefore very unreliable, and it is always best to conduct any examinations for coal by sections without regard to their reports. I am still of the opinion that true coal may be found in this country. The Carboniferous or Mountain limestone is largely developed here; and we have seen in several places shales and sandstones which in their lithological character strongly resemble those of the Coal-measures in other countries. What is still lacking is the evidence of fossils, to prove that a coal vegetation has once existed in these latitudes.

Our examinations for the metals have been highly successful. Iron ores of the very best quality, and inexhaustible in quantity, have been found in six different localities. The *first* is situated at a distance of four leagues from the town of Mescala. It is a vein 25 feet wide, and nearly vertical, and consists of solid magnetic ore. We have specimens from other similar veins in the vicinity, which we had not time to visit. There is no doubt of an abundant supply of ore in this region, and it could be readily transported to the river. It derives importance from the fact that it is upon the present direct road from Mexico to Acapulco.

The *second* great deposit is the Cerro Yman, or Magnet mountain, of Las Anonas Grandes, near Coyuca. This is also about four leagues from the river. It is a mountain of iron ore, fully equal to the celebrated ore of Missouri. It is about 300 feet high, half a mile long, and fully one-third of its bulk is pure magnetic ore. It also contains a bed of limestone in its summit. It is owned by a company who have some small furnaces near it, and are producing small quantities both of iron and steel from it.

Two other deposits, those of San Francisco and Singungao [Singangeo?], situated near each other, may be classed together as a *third* great locality. They are about two miles apart and about the same distance from the Mescala river. That of San Francisco is a conical hill about 150 feet high, consisting principally of pure magnetic ore. Its surface is covered with loose blocks of the same, from one to five feet in diameter. That of Singungao covers several acres near the summit of a mountain six or eight hundred feet above the river. Besides paving the ground over this space with large blocks of ore, the vein throws up several piles of ore in masses of ten or twelve feet diameter. Taken together or singly, these deposits are capable of yielding an unlimited supply of the very best ore. They are not worked at all at present. A *fourth* great deposit was met with near Villadero, within a day's ride of Zacatula. Here a bed of ore four hundred yards long and one hundred wide occupies the side and crosses the summit of a low hill. The surface is made up of blocks of pure ore from three to five feet in diameter.

The country around is well wooded and nearly level from this place to the Pacific.

The *fifth*, and, all things considered, the finest deposit we have seen, is situated near Chutla, on the road from Zacatula to Acapulco. It is not more than three miles in a direct line from the sea, and not more than six from a bay which affords a good and safe anchorage and landing. This deposit extends along the side of a mountain for more than a quarter of a mile, is several hundred feet wide and affords masses of ore of all sizes from ledges down to pebbles. The highest point of the bed is about 150 feet above the adjoining plain. On the upper side the deposit is bounded by a thick bed of soft limestone, affording an easy supply of flux. The ore is pure magnetic, and is the finest we have seen. The country around for many leagues has a fertile soil and is covered with dense forests of hard woods, which would produce an unfailing supply of the best charcoal. This location has special advantages in the quality and quantity of the ores, the convenience of fluxing materials and fuel, and the nearness of a good landing, with the whole of the Pacific coast for a market. I see no reason why the best qualities of charcoal iron could not be produced here as cheaply as elsewhere, with the advantage of freight and duties over any foreign competition in the supply of the Pacific coast of Mexico, and to a considerable extent also of the interior.

One other large deposit of iron was passed over, on the shore between Petatlan and Coyuguilla. It is a hill of large blocks of ore, situated immediately upon the shore, and beaten by a heavy surf. The ore was not seen in place, but the quantity was sufficient to show that a very large bed of it exists in the hill. The beach for several miles was blackened by sand derived from the disintegration of the ores.

We have information of another large deposit of iron ore, near the Hacienda of General Alvarez, which we may perhaps see on our way to Mexico.

The occurrence of so many and so rich deposits of iron, so widely distributed over the country, is interesting, from the assurance it gives that, whenever coal shall be discovered, there will be no want of iron ore within accessible distance of it.

Next to iron, copper appears to be the most abundant metal in this portion of Mexico. I shall defer to another occasion a description of the many localities of this metal which we have seen. One mine, that of Inguaran, has been worked since the time of the conquest, and is still producing well, though worked without the aid of machinery. Others in the same neighborhood, said to be equally rich, are entirely idle. The specimens and information which we have collected are sufficient to convince me that this portion of Mexico is one of the richest copper districts in the world. Silver and lead ores occur in abundance, but I will not undertake to describe here the various localities of them which we have seen, or the specimens which we have collected.

I am fully convinced that this portion of Mexico, when properly opened by the application of intelligent enterprise, will prove one of the richest mining regions of America.

ART. XXXIX.—*Numerical Relations of Gravity and Magnetism;* by PLINY EARLE CHASE, M.A., S.P.A.S.[1]

IN the fifth century before the Christian era, Leucippus and his disciple Democritus taught that heat is the soul of the world, the principle of life and intelligence, and that space is an infinite plenum, pervaded by material atoms too minute to be perceptible to the senses, which, by their constant motions, unions, and separations, form the beginnings and ends of things. In this theory, which is said to have been borrowed from the priests of Isis and Osiris, we may trace the origin of the modern belief in a universal kinetic æther, and of the attempts to resolve all forces into "modes of motion," which were practically inaugurated by our own countryman, Benjamin Thompson, Count Rumford, and which have been so successfully prosecuted by Carnot, Seguin, Mayer, Colding, Joule, Grove, and their collaborators.

The mutual convertibility of Light, Heat, Electricity, Magnetism, Chemical Affinity, and Vital Energy, may be now regarded as one of the most probable physical hypotheses. Faraday has endeavored also to connect gravitation and magnetism or electric action by experimental results, but in vain. Still, the conviction of such a connection is almost irresistible, and various physicists have given us incidental pointings in that direction. Ampère discovered the magnetic effect of electric currents circulating around iron bars; Arago, whose experiments were repeated and extended by Babbage, Herschel, Barlow, Christie, and others, showed that simple rotation produces magnetic disturbances which are governed by fixed laws; the distribution of induced magnetism in masses of iron, as determined by Barlow and Lecount, is the same as would follow from the relative centrifugal motions of different portions of the earth, provided the magnetic axis corresponded with the axis of rotation;[2] Hansteen suspected, and Sabine practically demonstrated, the influence of the sun upon terrestrial magnetism; Secchi ascertained that "the diurnal excursion of the needle is the sum of two distinct excursions, of which the first depends solely on a horary angle, and the second depends, besides, on the sun's declination,"[3] and that "all the phenomena hitherto known of the diurnal magnetic variations may be explained by supposing that the sun acts upon the earth as a very powerful magnet at a great distance."[4]

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[1] From the Proceedings of the American Philosophical Society, Dec. 16, 1864. The Magellanic Gold Medal was awarded the author for this memoir.

[2] This fact was first announced by me, at the Society's meeting, April 15, 1864. See Proc. A. P. S., ix, 367.

This hypothesis has been objected to, on the ground that it is difficult to understand how any conceivable intensity of solar magnetism, by its simple induction, could produce so great a disturbance as is daily observed. Therefore it will probably follow the fate of the earlier ones, which attributed terrestrial magnetism to one or more powerful magnets lying nearly in the line of the earth's axis, while Barlow's idea that the magnetism is superficial, and in some manner induced,[5] will still remain in the ascendant. Secchi's conclusions are, however, none the less interesting, and from the fact that magnetism is, like gravity, a central force, varying inversely as the square of the distance, they lend encouragement to those who are endeavoring to find new evidences of the unity of force.

My own experiments and researches have led me to the belief that all magnetism is a simple reaction against a force which disturbs molecular equilibrium, that the numerical equivalent of the magnetic force is therefore equal and opposite to that of the disturbing force, ($\pm M = \mp D$), and that all the phenomena of terrestrial magnetism result from tidal and thermal changes in terrestrial gravitation.

Sullivan[6] and Reinsch[7] have pointed out the effect of musical vibrations upon the magnetic needle, and I have shown the controlling influence of a purely mechanical polarity.[8] A careful examination of the polarizing thermal and rotation currents,[9] will show that the spirals, which they have a tendency to produce, are quasi horizontal cyclones, one set flowing in a nearly constant direction along the magnetic meridian, and the other toward the momentarily shifting solar meridian. From an investigation of these currents and a comparison of various observations, I have deduced the following theses:

I. The daily magnetic variations, though subject to great disturbances, at different hours, show an average approximation to the differences of the gravitation-tidal currents.

Hours from Mean, - - - - -	1h.	2h.	3h.
Means of Theoretical Ratios, - - - -	·500	·866	1
" " Observed " - - - -	·563	·865	1

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[3] Phil. Mag. [4], viii, 396.

[4] Ibid. ix, 452.

[5] Phil. Trans., 1831.

[6] See De la Rive's Electricity, ii, 635.

[7] Phil. Mag. [4], xiii, 222.

[8] Proc. A. P. S., ix, 359.

[9] Ibid., p. 367 seq.

II. Marked indications of an accelerating force are discoverable in the magnetic fluctuations, especially during the hours when the sun is above the horizon.

Hours from Mean, - - - - -	1h.	2h.	3h.
Mean Ratios of Hourly Tidal Differences, -	100	73	27
" " " Squares of Hourly Magnetic Differences, -	100	74	26

See also Thesis V.

III. There are lunar-monthly barometric and magnetic tides, which may be explained by differences of weight or momentum,[19] occasioned by the combined influences of solar and lunar attraction, and terrestrial rotation.

IV. The solar diurnal variations of magnetism between noon and midnight are nearly identical in amount with the variations of weight produced by solar attraction at the same hours.

The ratio of the solar to the terrestrial attraction for any particle at the earth's surface, being directly as the mass, and inversely as the square of the distance ($M \div R^2 = 354{,}936 \div 23{,}000^2$), is ·00067. The weight of any particle is therefore increased by this proportionate amount at midnight, and diminished in the same proportion at noon, making a total half-daily variation of ·00134 in the atmospheric weight, and consequently, according to my theory, in the terrestrial magnetism.

Theoretical variation, ·00134. Observed variation, ·00138.

V. The magnetic variations at intermediate hours, between noon and midnight, indicate the influences of an accelerating force, like that of gravity, modified by fluctuations of temperature, and by atmospheric or ætherial currents.

Every particle of air may be regarded as a planet revolving about the sun, in an orbit that is disturbed by terrestrial attraction and other causes. In consequence of these disturbances, there is an alternate half-daily fall toward the sun, and rise from the sun. By the laws of uniformly accelerated and retarded motions, the mean fall and the consequent mean magnetic disturbances should occur at $12^h \div \sqrt{2} = 8^h\, 29'$ from midnight.

Theoretical mean, $8^h\, 29'$. Observed mean, $8^h\, 31'$.

VI. Some of the magnetic influences appear to be transmitted instantaneously, through the rapid pulsations of the kinetic æther; others gradually, through the comparatively sluggish vibrations of the air.

VII. The comparative barometric disturbances of the sun and moon exhibit an approximate mean proportionality between their comparative differential-tidal and magnetic disturbances.

Let the solar differential-tidal force be represented by A′, and the lunar by A″, the respective barometric disturbances by B′ and B″, and the magnetic disturbances by M′ and M″. If M′ and B″ are required, we have

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[19] I believe there can be no weight without some degree of momentum. See Proc. A. P. S., ix, 357.

	A′÷A″	B′	B″	M′	M″
Theoretical values,			·00012	·00144	
Observed "	2·55	·00057	·00013	·00140	·0000255

VIII. The theoretical gravitation-variation of magnetism (Prop. IV) is slightly less, while the theoretical barometric variation (Prop. VII) is slightly greater, than the corresponding observed variation. The excess in one case exactly counterbalances the deficiency in the other, the sum of the theoretical being precisely equal to the sum of the observed variations.

IX. The total daily magnetic variations, like the barometric, can be resolved into a variety of special tides, which may be severally explained by well-known constant or variable current-producing and weight-disturbing forces.

Hours from Midnight.	A Theoretical Gravitation Tide.	B Theoretical Differential Solar Tide.	A+B Theoretical Mean Tide.	Observed Mean Tide.
0	−·00067	+·00024	−·00043	−·00043
6	·00000	−·00024	−·00024	−·00023½
12	+·00067	+·00024	+·00091	+·00095

The hours are counted from midnight, in each half-day.

Column A contains the hourly differences from mean weight, attributable to solar gravitation, with changed signs; diminution of weight being accompanied by increase of magnetism, and *vice versa.*

The form of the tide in column B is evidently such as should be determined by solar action. The magnitude of the tide is estimated by comparing the relative amounts of motion down the diagonal and down the arc of a quadrant $(\cdot 00067 \times [1-(\frac{\pi}{4}-\frac{1}{2})] = \cdot 00048)$. The mean-tidal difference $[(\cdot 00067 - \cdot 00048) \div 2]$ is very nearly equivalent to the average theoretical inertia-disturbance of weight. The atmospheric inertia at St. Helena, (regarding the fluctuations as uniform between successive hourly observations,) produces retardations of 59′, 85′, 26′, and 31′, at 0^h, 6^h, 12^h, and 18^h, respectively. The mean retardation is 50′, or $\frac{5}{72}$ of a half-day. The theoretical daily gravity-variation being ·00134, the average variation in $\frac{5}{72}$ of a half-day is $\cdot 00009\frac{11}{36}$, the mean tidal difference being $\cdot 00009\frac{1}{2}$.

The consideration of the moon's disturbance of the atmospheric gravitation is complicated by the magnitude of its differential attraction, the position of the center of gravity of the terrestrial system, the varying centrifugal force, and other circumstances involved in the lunar theory. Still there are indications, in the following synopsis, of the influence of gravity, sufficiently striking to encourage a hope that our knowledge of the moon's perturbations may be improved by a thorough comparative study of the lunar astronomical, atmospheric, and magnetic tables.

Lunar-daily disturbances of Magnetic Force at St. Helena, in millionths of the total force.

Hours.	0	1	2	3	4	5	6	7	8	9	10	11	12
Before Lunar M.	+5	−1	+4	−2	−5	−5	−6	−3	−2	− 1	+14	+15	+16
After " "	+5	−1	−5	−6	−7	−6	+1	+1	−2	+18	+25	+22	+16
Mean,	+5	−1	−0·5	−4	−6	−5·5	−2·5	−1	−2	+ 8·5	+19·5	+18·5	+16
Rotation-Tide.	0	0	±4·5	±2	±1	±5	∓3·5	∓2	0	∓ 9·5	∓ 5·5	∓ 3·5	0

The above table shows, that

1. The moon's attractive force ($M \div R^2 = \cdot016 \div 60^2 = \cdot000004$) multiplied by the coëfficient of its differential attraction (2·55) gives ·0000113, which is nearly the same as the mean meridional magnetic disturbance [$(\cdot000005 + \cdot000016) \div 2 = \cdot0000105$].

2. The increase of magnetism at 12^h is nearly equivalent to the attractive force, multiplied by the square of the distance from the center of gravity of the system, and divided by the square of the earth's radius ($\cdot000004 \times 7707^2 \div 3963^2 = \cdot0000168$).

3. There is a tendency to equality of disturbances on each side of the meridian at 1^h and 8^h, as in the solar magnetic tide.

4. The greatest disturbance occurs at the hours of 10^h and 11^h P. M., both in the solar and in the lunar tide.

5. There are some indications of an increase of gravity, and decrease of magnetic force, when the tidal flow is toward the center of gravity of the terrestrial system, and *vice versa*.

6. The rotation-tide has the customary quarter-daily phases of alternate increase and diminution.

X. The phenomena of magnetic storms indicate the existence of controlling laws, analogous to those which regulate the normal fluctuations. See Proceedings Amer. Phil. Soc., Oct. 21, 1864.

The foregoing comparisons have been based on Gen. Sabine's discussions of the St. Helena records. It would be desirable, if it were possible, to confirm them by observations at other stations near the equator; but the need of such confirmation is in great measure obviated, by the variety of ways in which I have shown the probable connection of gravity and magnetism. At extra-tropical stations, the rotation tide becomes so preponderating that it is difficult to trace the diminished gravitation- and differential-tides, still I shall look confidently to a fuller development of the theory of tidal action, for future additional support to my views.

ART. XL.—*On the Origin and Formation of Prairies;* by LEO LESQUEREUX.

THIS paper is intended as a review of Prof. Winchell's new theory on the origin of the prairies of the Mississippi Valley,[1] and as a defense of my own views on the same subject.[2] I shall therefore omit all details relative to the surface of prairies, their conformation and appearance, and their geological and geographical distribution, since these details do not directly concern the elucidation of the question.[3]

From the brief mention made of my opinion concerning the formation of prairies, it appears to have been misunderstood by those who have quoted it, or rather it was entirely unknown to them. For this reason it is advisable to first expose, as they were originally given, the essential points of a simple explanation, which does not merit to be spoken of as a theory. I merely translate from my letter to Professor Desor:

"You well know that prairies are at our time in process of formation along the shores of the Lakes:—Lake Michigan, Lake Erie, etc. and also along the Mississippi and some of its affluents, especially the Minnesota river. The formation of these recent prairies, whose extent is not comparable to that of the primitive ones, is peculiar, and has a great analogy to that of peat bogs. Where the waves of the lakes, or their currents, strike the shores or the low grounds, and heap materials, such as sand, pebbles, mud, etc., they build up more or less elevated dams or islands, which become covered with trees as soon as they are raised above water. These dams are not always built upon the shores, and do not always even follow their outlines, but often enclose wide shallow basins, whose water is thus sheltered against any movement. There the aquatic plants, sedges, rushes, grasses, soon appear, and these basins become swamps, as may be seen near the borders of Lake Michigan. Though the forests may surround them, the trees do not invade them, even when the swamps become drained by some natural or artificial cause.

"Along the Mississippi and Minnesota rivers, the same phenomenon is observable, with a difference only in the process of operation. During a flood, the heaviest particles of mud are deposited on both sides of the principal current, along the line of slack water, and by repeated deposits, dams are slowly formed and upraised above the general surface of the bottom land. Thus, after a time, the water thrown on the bottoms by a flood, is, at its subsidence, shut out from the river, and both sides of it are converted into swamps, sometimes of great extent. Seen from the high bluffs bordering its bottom lands, the bed of the Minnesota river, in the spring, is marked for miles by two narrow strips of timbered land bordering the true channel of the river and emerging like fringe in the middle of a long continuous lake. In the summer, and viewed from the same point, the same bottoms appear transformed into a green plain, whose undulating surface resembles immense fields of unripe wheat. In reality, they are impassable swamps covered with sedges, rushes, and coarse grasses. By successive inundations, they become, by and by, elevated above the level of the river. They then dry up in summer, mostly by infiltration and evaporation, and when out of the reach of the floods, they become at first wet, and afterward dry, prairies.[4] In that way, admirable locations for river towns have been made. On the Mississippi, Prairie du Chien, Prairie la Fourche, Prairie la Crosse, etc., are, as indicated by their names, towns located on formations of this kind.

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[1] This Journal [2], xxxviii, 332.
[2] Letter to Prof. Desor in Bull. de la Soc. des Sci. Nat. de Neuchatel, Dec. 1856.
[3] Prof. Whitney in the 1st Chap. of the Report of the Geological Survey of the State of Iowa has given a clear and very accurate descriptive account of the prairies.

"These splendid patches of praries, though of a far more recent origin than the immense plains above them, are nevertheless true prairies. Although bordered on one side by the high-timbered banks of the bottoms, and still separated by a fringe of trees from the actual bed of the river, yet the trees do not enter them. This peculiarity of formation explains first the peculiar nature of the soil of the prairies. It is neither peat nor humus. It is a black, soft mould, impregnated with a large proportion of ulmic acid, produced by the slow decomposition, mostly under water, of aquatic plants, and thus partaking as much of the nature of peat as of that of true humus. In all the depressions of the prairies, where water is permanent and unmixed with particles of mineral matter, the ground is true peat.

"It is easy to understand why trees cannot grow on such kind of ground. The germination of the seeds of arborescent plants needs the free access of oxygen; and the trees, especially when they are young, absorb by the roots a great amount of air, and demand a solid point of attachment to fix themselves. Moreover, the acid of this kind of soil, by its particularly antiseptic property, promotes the vegetation of a peculiar group of plants mostly herbaceous. Of all the trees, the tamarac is the only species which, in our northern climate, can grow on a peaty ground, and this even happens only under rare and favorable circumstances, that is, when stagnant water, remaining at a constant level, has been invaded by the *Sphagnum*. By their absorbing power, their continuous growth and the rapid accumulation of their remains, these mosses slowly raise the surface of the bogs above water, and it is then, in this loose ground, constantly humid but accessible to atmospheric action, that the tamarac appears.[5]

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[4] The lowest part of these fluvial prairies is of course farthest from the rivers along the bluffs. Here, generally, the percolation of water through the banks forms springs and deeper swamps, which are often transformed into peat bogs.

"Now, let us examine the prairies according to this idea of their formation, and see if, from the first trace of their origin to their perfect completeness, there is any thing in their local or general appearance which is not explained by it or does not agree with it.

"The bay of Sandusky is now in process of transformation into prairies, and is already sheltered against the violent action of the lake by a chain of low islands and sand-banks, most of them covered with trees since a long time, at least, judging from the size of the trees. All these islands are built up with the same kind of material, that is, with lacustrine deposits, either moulded into low ridges under water, or brought up and heaped by waves and currents. Around the bay, especially to the southwest, there are extensive plains covered with shallow water. The bottom, in the depression toward the lake and where the aquatic vegetation is only at its origin, is sandy clay. But, in the more shallow places, the clay is already muddy and blackened at its surface by the detritus of the herbaceous vegetation which has grown upon it. Farther toward the borders, and in proportion to the shallowness of the water, the detritus thickens, and still farther, we have wet prairies, with exactly the same vegetation as that of the lake swamps, and a black soil with a substratum of clay, the same materials also as those of both the deeper and more shallow swamps of the lakes. In receding from the borders of the lake toward the high prairies, the transition from wet to dry prairies is by so insensible degrees that it would be impossible to fix a point of separation between them. All the surface appearances are the same. Vegetation is here and there modified by the presence of some peculiar species of herbaceous plants, but nothing else. The homogeneousness of the soil is still more striking. There is the same kind of clay as subsoil, and this is overlaid by the same kind of black spongy mould. And if here and there you see knolls covered with trees, the cuts of the railroad show that the materials of which they are formed are a different compound from the ground of the prairies, even if they are scarcely elevated above the general level, and that they are of the same nature and of the same formation with those of the low wooded islands of the lake.

On the borders of the numerous lakes which dot the high rolling prairies, especially in Minnesota, the process of formation of the prairies is repeated in the very same way. These lakes are of every size, sometimes small and circular, true ponds; sometimes thirty to forty miles in circumference, and in this case shaping the outlines of their shores according to the undulations of the prairies; divided into innumerable shallow branches, mere swamps, covered with water plants, and emptying themselves from one to another; passing thus by slow degrees toward rivers, not by well marked channels, but by a succession of extensive swamps. These are the sloughs which separate the knolls of the prairies—so to speak, the low grounds of the rolling prairies. They are nearly dry in summer, but in the spring are covered by one to three feet of water. Their vegetation is merely sedges and coarse grasses. I have never seen fishes in these sloughs, but plenty of crawfishes and a great quantity of freshwater shells.[6] Wherever the borders of the lakes are well shaped, not confounded with or passing into swamps, they rise from five to six feet above the level of the water and are timbered, mostly with oak and hickory. This elevated margin is more generally marked on the eastern sides of the lakes, a record of the action of the waves under the dominant winds. Or it is shaped as a low range of hills surrounding the lakes, and is due then to original irregularities of the surface. The materials are the same as those of the dams, or low islands, of the great lakes; indeed, the same as those of the under bottoms of the swamps, or those over which the prairies have been formed. But they have been removed from the influence of stagnant water: this is the only difference."

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[5] In my letter to Prof. Desor, this question is discussed at length. I have omitted here the details, as they are unnecessary for the elucidation of the subject.

"From all these remarks, what other conclusion can we deduce but that all the prairies of the Mississippi valley have been formed by the slow recession of sheets of water of various extent, first transformed into swamps, and, by and by, drained and dried. The high and rolling prairies, the prairies around the lakes, those of the bottoms along the rivers, are the results of the same cause, and form a whole, an indivisible system."[7]

"To this assertion you will object, I suppose, and say: How is it that the prairies are not everywhere perfectly horizontal; and as there is some unevenness of surface, have these undulations not been formed like the low islands or high borders of the lakes, and why then are they not timbered?"—"I believe that, although the surface of the prairies may be now undulated, it was originally horizontal enough to form shallow lakes, and then swamps, like those which at the present time cover some parts along the shores of Lake Erie, Lake Michigan, etc. Where this horizontality has disappeared, it is only by very slow degrees, under the eroding action of water, which, in its slow movements, tends to follow every change of level, seek an outlet, and so establish channels of drainage. I have followed for whole days the sloughs of the prairies, and have seen them constantly passing to lower and well marked channels, or to the beds of the rivers, by the most tortuous circuits, in a manner comparable to the meanderings of some creeks in nearly horizontal valleys. Indeed, the only difference is, that, in the high prairies, there is not a definite bed, but a series of swamps, extending, narrowing, and bending in many ways. The explanation appears to me so natural, that I cannot understand how high prairies could ever be perfectly horizontal. Along the lakes and in their vicinity, the horizontality is a necessary consequence of the primitive evenness of the bottom and of the proximity to water. The level of the low prairies being scarcely above that of the lakes, their surface, after an overflow, becomes dry, rather by percolation and evaporation than by true drainage. But wherever the rivers have cut deeper channels,—as is the case in the north part of the Mississippi basin, where they run sometimes from one to three hundred feet lower than the surface of the high prairies—the drainage has constantly taken place toward those deep channels, and the water, though its movements may be very slow, furrows the surface in its tortuous meanderings. From this, results that irregular conformation of surface generally and appropriately called *rolling.* In Indiana and Illinois, in the vicinity of the Wabash river, for example, there are some high prairies whose surface is apparently horizontal. But these prairies, as at Terre Haute, are surrounded by a margin of low wooded hills, and have originally been shallow lakes of difficult and slow drainage. Moreover, their horizontality is rather apparent than absolute; some parts of them are already dry enough to be cultivated and ploughed in the spring; some parts are used as wet meadows, and still others are covered with water and inaccessible. This apparent horizontality results from the great width of what we may call already channels of drainage. These will, by and by, contract and deepen, and thus the prairie become undulating. This opinion is in contradiction to what you say in your excellent paper on the drift of Lake Superior: that the irregularities of surface of the prairies have been caused by currents at the time when they were under water. If the ground of the high rolling prairies had been prepared in advance, as you suppose, the prominent parts or the knolls would have been timbered like the low islands of the lakes."

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[6] Especially *Planorbis trivolvis*, *P. lentus*, *Lymnea appressa*, *L. emarginata*, *L. decidiosa* Say, etc. The lakes have the same species, with many bivalves, and a great abundance of fishes, especially catfishes (*Pimelodes*).

[7] Atwater, this Journal, i, 116, 1819, and Bourne, ibid., ii, 30, 1820, have both considered the prairies as originating from swamps, without, however, giving an explanation of the phenomenon.

Next follows, in my letter, an examination of the evidence that the prairies have been covered with water to their highest points; that the water has not been drained by any violent action but by slow upheaval of the surface; that the deepening of the great channels, and the formation of low prairies, around the lakes and along the rivers, are merely a continuation of the same phenomenon or a result of the slow and continuous upheaval of the great Mississippi basin.

The letter ends in refuting opinions, admitted by some authors, that the absence of trees on the prairies is caused by atmospheric dryness, and by others that it results from fires, etc.

After reading this exposition of my views, Prof. Desor remarked that what he calls my theory has the merit of combining in the same explanation two phenomena, the formation of the peat bogs, and the formation of the prairies, whose relation to one another is too natural to be without foundation. But he objects to some of my ideas, and especially thinks that my explanation of the formation of the *rolling* of the high prairies cannot be admitted, because the spaces between the knolls are not deep and narrow sloughs or simple trenches, but broad depressions, broader than the knolls themselves; that this could not be the case, if they had been formed by the erosion of water.

This objection, I think, is groundless. As we have seen, in considering the surface of the low and of the flat prairies, wherever the drainage is insensible, water can scarcely have any action in digging trenches. In the spring, or after heavy rains, its slow movements extend over the whole breadth of the low grounds, scarcely displacing or carrying away the finest material. This can hardly be considered as *erosion.* Nevertheless, it is certain that all the sloughs of the rolling prairies find their way to lower and deeper channels, where they definitively shed their water. It is certain, also, that in reaching the Mississippi, or the Minnesota, etc., these sloughs are deeper and all nearly perpendicular to the direction of the rivers. For this reason the *rolling* of the prairies along the great rivers resembles a succession of fronting abutments. Prof. Whitney, in the first chapter of the geological report of Iowa, makes a corresponding remark, when he says, (p. 17,) "The streams of the prairies usually take their rise in small depressions of the high prairie, scarcely to be noticed as being below the general level of the region. As their course continues, the beds generally sink, etc." Prof. Desor holds that the knolls of the prairies were formed under water, and in deep water, and, to confirm his assertion, he says that the bottoms of our great lakes, and of the ocean also along the shores, are marked by swells and deep furrows. In fact, the knolls of the prairies are of quite a different form from the long continuous parallel undulations of the bottom of the seas. Moreover, there are such traces of marine action in Iowa and Minnesota in the timbered *Coteaux*, as the Coteau des Bois, Les Bois-rouges, etc., which cut the nakedness of the prairies and resemble those narrow strips of land bordering the ocean and soon becoming long peninsulas covered with a luxuriant arborescent vegetation. The first origin of these undulations may have been in deep water, or they may have resulted merely from the action of the waves. In any case, they cannot be compared with the barren knolls of the prairies.

Prof. Whitney,[8] admitting that the prairies have been covered with water, even to their highest point, considers the absence of trees as caused by the fineness of the soil, which he attributes in part to the nature of the rocks underlying it, and in part to *accumulation in the bottom of immense lakes of a sediment of almost impalpable fineness, under conditions which will be discussed hereafter.*[9] This unfinished explanation cannot fully satisfy the mind. Prairies cover every kind of geological formation, even granitic rocks, as in Minnesota, between St. Peter and Fort Ridgely. Most generally they overlie the Drift. It is evident that the black soil of their surface, as well as the clayey subsoil, though the thickness of these strata may be great, has been formed, in place, by the agency and growth of a peculiar vegetation. In stagnant water, whenever water is low enough to admit the transmission of light and air in sufficient quantity to sustain vegetable life, the bottom is first invaded by Confervas, especially by *Characeæ* and a *peculiar* kind of floating moss (*Hypnum aduncum* Hedw.). These plants contain in their tissue a great proportion of silica, lime, and even oxyd of iron.[10] Moreover, they feed a prodigious quantity of small mollusks, whose shells add to the detritus of the plants, and the final result of the decomposition of the matter is that fine clay of the subsoil of the prairies, truly impalpable when dried and pulverized. This formation has been observed and described long time ago, especially by Chrome, who attributes to the decomposition of Confervas, Characeæ, etc., the clay over which peat bogs generally rest. I have seen it in process of prodigious activity in a large pond of the King's Garden of Fredericksburg in Denmark, which every year is filled with about one foot of clayey matter, by the decomposition of Characeæ, small mollusks and infusoria. This kind of formation is so general that it can be observed in nearly every open swamp. In the lakes of the high prairies it has sometimes a peculiar character. At the depth of from one to three feet, the plants above named, Mosses, Confervas, and Charas, form a thick carpet, which hardens, becomes consistent like a kind of felt, and, floating about six inches from the bottom, is nearly strong enough to sustain the weight of a man. The carpet is pierced with holes, where fishes pass in and out, and the bottom under it is that fine impalpable clay, evidently a residue of the decomposition of its plants. At the depth of three and a half to four feet, this vegetation suddenly ceases and the bottom of the lakes is pure sand and pebbles with shells. Nearer to the borders, on the contrary, at the depth of one foot, the carpet of mosses, etc., begins to be intermixed with some plants of sedges, becoming more and more abundant in proportion as the depth decreases. As soon as the leaves of these plants rise out of the water, they absorb and decompose carbonic acid, transform it into woody matter, under atmospheric influence, and their detritus is at first clayey mould, and then pure black mould, the upper soil of the prairies. Of course, near the borders of the rivers, or under peculiar circumstances, the formation is somewhat modified by the addition of transported matter, or of foreign elements. The clay may thus take a different color, and have a somewhat different composition; but the process of formation does not materially change.

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[8] Geological Survey of Iowa, chapter i.

[9] Geological Report of Iowa, i, 25.

[10] DeCandolle *Physiologie Vegetale*, p. 183 and 188. When exposed to atmospheric influence, the Charas become covered with an efflorescence of scarcely carbonated or pure lime.

Considering the whole explanation of the formations of the prairies, as it is exposed in this paper, I think that it covers the whole ground and can apply to most of the cases (if not to all) where the ground is naturally naked or without trees. It gives the reason of the formation of the prairies from the base of the Rocky Mountains to the borders of the Mississippi River; of the prairies around the lakes, and of those of the broad flat bottoms of our southern rivers; of the Platas of the Madeyra river; of those of the Paraguay; of the pampas of Brazil; and even of the desert plains of our western Salt Lakes. For this formation is produced, in the same manner, in the salt marshes of the sea, as in the freshwater swamps of our lakes. And if, passing to other continents, we examine, in Europe, the natural meadows of Holland, the barrens or heaths of Oldenbourg, all the plains on the shores of the North and of the Baltic Sea, and, in Asia, the vast steppes of the Caspian, etc., etc., we find everywhere the same appearances, and the same results of a general identical action, modified only by local and mostly climatic circumstances.

The glades on the slopes of some mountains of Arkansas, of the Alleghanies, etc., have been quoted "as a phenomenon contradicting the theory of formation of prairies by water."[11] I have carefully examined those glades in connection with the geological survey of Arkansas, and found them always the result of water. When the springs descending the slopes are stopped by some obstacle, the waters slowly overflow the surrounding surface, and generally favor the growth of a thin stratum of peat, where only grasses and prairie flowers vegetate. Sometimes the peat of these glades is one to two feet thick. They are generally dry in the fall, but always true swamps or wet prairies in the spring.[12]

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[11] R. W. Wells, this Journal, i, 334, 1819.

Prof. Winchell observes that *a theory* [for the absence of trees] *often urged is the considerable humidity of the soil of certain prairies and especially the wetness of the subsoil*, &c., and refutes it by this mere assertion: *it is singular that such an opinion could be entertained when it is so well known that there is no situation so wet but certain trees will flourish on it. The willow, cottonwood, tupelo, water oak, tamarac, American arbor vitæ*, etc.[13]. And, when considering Prof. Whitney's supposition that the extreme fineness of the prairie soil is the cause of the absence of trees, he sets it aside, in the same way, by another assertion: that *the fatal objection to this theory, and all theories which look to the physical or chemical condition of the soil for an explanation of the treeless character of the prairies, is discovered in the fact that trees will grow on them when once introduced.*[14]

As it is not proper to refute an assertion by a contrary one, let us examine under what circumstances trees may grow in some swamps, and what the highest scientific authorities have to say on the subject.

It is a well known fact of botanical physiology that trees absorb by their roots a certain amount of oxygen necessary to their life. It is in accordance with this principle that trees, to thrive well, ought not to be planted too deep; that most species of trees perish when their roots are buried in a stratum of clay, impermeable to air; that whenever the water of a creek is dammed, to make a pond, all the trees are killed on the whole flooded space. Running water furnishes a sufficient amount of air and oxygen for their life to certain species of trees (most of the species quoted by Prof. Winchell) whose roots, when immersed, have the property of dividing themselves into innumerable filaments. Hence, such trees grow, indeed, in those swamps inundated by the water of adjacent rivers, or periodically invaded by the tides. The water of such swamps is not permanent, and its whole mass is subjected to some kind of movement. It is thus that in the south the bald-cypress and the tupelo grow even in the middle of creeks and bayous. But look anywhere else, along the rivers, or on the shores of seas or lakes, and wherever a sheet of shallow water is sheltered against the waves, the tides or the currents, you find invariably treeless swamps, passing to prairies.[15] The only fact, to my knowledge, which could be mentioned as sustaining the assertion of Prof. Winchell is, that certain kinds of shrubs, as the button bush, the swamp rose, etc., grow and form thickets around some true swamps of our forests. But, examining the process of germination of the seeds, it is easy to observe that the germs are not developed in water, but in dried decayed vegetable matter of rotten prostrated trees. In summer, and only when the margins of the swamps are dry, the roots penetrate the ground. Thus the swamp is surrounded by a belt of shrubs, but its central part is open and occupied by herbaceous plants only.

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[12] The relation of glades to peat bogs is beautifully exemplified on the slopes of Mt. Marcy and other peaks of the Adirondac Mts. of New York. Here, in the middle of deep and nearly impenetrable woods, such openings, half prairies, half bogs, are suddenly entered without transition whatever, and their surfaces entirely barren of trees, appear like clearings and meadows produced by human agency. They are of every size, cover slopes of various degrees, and ascend as high as to 5000 feet above the sea.

[13] A. Winchell on the prairies. This Journal, [2], xxxviii, 348.

[14] A. Winchell, ibid., 344.

Now, what says DeCandolle in his classical book on Vegetable Physiology? He remarks that the constant irrigation required for the rice culture in Lombardy is a serious inconvenience, because the water penetrates the ground of the neighboring estates and *kills the trees:*—That water, left stagnant for a time on the ground, rots the trees at the collum, prevents the access of oxygen to the roots, *and kills them:*—That in the low grounds of Holland they dig, for planting trees, deep holes, and fill the bottoms with bundles of bushes, as a kind of drainage for surplus water as long as the tree is young enough to be *killed by humidity:*—That true swamps and marshes have no trees, and cannot have any, because *stagnant water always kills them.*[16] Authorities to the same effect could be quoted by volumes.

The second assertion, *that trees will grow on the prairies when once introduced* (or planted, I suppose), is certainly true. But we should take care to make a distinction between the results of an artificial process and those of a natural one. When trees are planted on the prairies, the soil is conveniently prepared. The clayey subsoil mixed with the black mould forms a compound which combines density of certain parts with lightness of others, and contains a great proportion of nutritive elements. If the clay of the subsoil is not too thick to be impermeable to water, and thus retain it around the roots, this prepared or artificial ground is indeed very appropriate to the growth of trees. But has any one ever seen oak or hickory, or any other kind of trees, grow on the prairies from a handfull, say even from a

[15] Species of trees like the magnolia grow over the southern peat bogs for the same reason that tamaracks grow in the peat bogs of the north.

[16] Physiologie Vegetale, pp. 1206-1212.

bushel, of acorns or nuts thrown upon their surface? Why, then, if trees *will grow* on prairies, do not isolated or widely separated clusters of trees insensibly cover a wider area? Some of these trees have lived there for ages; their trunks are strong and thick; their branches widely expanded, and their fruits are swept far away by the impetuosity of autumnal storms; nevertheless, their domain is restricted, by the nature of the ground, to their own narrow limits; these they never pass.

[*To be continued.*]

ART. XLI.—*On a Process of Fractional Condensation: applicable to the Separation of Bodies having small differences between their Boiling-points;* by C. M. WARREN.[1]

IT is well known that the process in general use for the proximate analysis of mixtures of volatile liquids,—viz: that of simple fractional distillation, either from a tubulated retort or from a flask with bulbs, as proposed by Wurtz,[2]—affords but very imperfect and unsatisfactory results, and not unfrequently leads to gross errors and misconceptions, except in those cases in which the boiling-points of the constituents are widely different, or in which some auxiliary method can be advantageously employed.

The want of a more efficient process for effecting such separations has long been recognized. There are numerous natural and artificial products, of the highest scientific interest,—such as petroleums, essential oils, tars, and other mixtures of oils obtained by the distillation, under varied circumstances, of bituminous, vegetable, and animal substances,—of which it may at least be said that we have but very imperfect knowledge,—I might almost say no knowledge, except such as could be derived from the study of very impure materials,—still mixtures of different bodies,—with which, instead of the pure substances sought for, chemists have felt compelled to content themselves, as the best results which they were able to obtain by the means at their command.

In repeated instances, apparently after persevering and protracted efforts, investigators have been forced to assert either the impossibility, or their inability, to obtain, from such mixtures, bodies of constant boiling-point,—a property which is generally received as a test of purity for liquid bodies.

I may here specify a few recent instances of this kind.

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[1] From the Journal of the Acad. Arts and Sciences, Boston, May 10th, 1864.

[2] Annales de Chimie et de Physique, [3], xlii, 132.

1. Warren de la Rue and Hugo Müller,[3] in their paper entitled "Chemical Examination of Burmese Naphtha and Rangoon Tar," after detailing the preliminary treatment by distillation in a current of steam, add that "A further separation of the various products was effected by repeated fractional distillations; but no absolutely constant boiling-points could be obtained, notwithstanding the great number of distillations and the large quantity of material at command. It is true that considerable portions of distillates could be collected between certain ranges of temperature, tending to indicate a constant boiling-point; nevertheless, it soon became evident that distillation alone could not effect the separations of the various constituents, and that recourse must be had to other processes." The other processes resorted to were, treatment with sulphuric and nitric acids, either separately or mixed; but still with very imperfect results. This acid treatment, which was first proposed by De la Rue, and subsequently employed by C. Greville Williams,[4] Schorlemmer, and others, will be further noticed below.

2. Frankland,[5] in speaking of a mixture of the hydrocarbons of the formulæ C_nH_n and C_nH_{n+1} (now generally considered as C_nH_{n+2}), which have a difference of 6° to 7° C. between their boiling-points, says, "The separation of two such bodies by distillation alone is impossible;" and suggests that the employment of anhydrous sulphuric acid may accomplish the object by dissolving out the body of the formula C_nH_n.

3. And so recently as 1862, Schorlemmer,[6] in his first paper "On the Hydrids of the Alcohol-Radicals existing in the Products of the Destructive Distillation of Cannel Coal," remarks that "it was, however, found impossible to obtain a product of constant boiling-point by repeated fractional distillations;" and he also had recourse to the acid-treatment above referred to.

4. Pebal,[7] after an elaborate research on the petroleum from Galicia, in which Wurtz's bulbs were employed, and also Eisenstuck,[8] who made an extended investigation of the petroleum from Sehnde, near Hannover, also with the use of Wurtz's bulbs, both assert in the most positive manner the impossibility

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[3] Proceedings of the Royal Society, viii, 221.

[4] Philosophical Transactions, 1857, 447.

[5] Quarterly Journal of the Chemical Society, 1851, 3, 43.

[6] Journal of the Chemical Society, xv, 419.

[7] Annalen der Chemie und Pharmacie, cxv, 20, asserts the "Unmöglichkeit, das Gemenge durch fractionirte Destillationen zu entwirren."

[8] Annalen der Chemie und Pharmacie, cxiii, 169, says as follows: "Mit den 5° zu 5° aufgesammelten Destillaten wurde die fractionirte Destillation wieder von Neuem vorgenommen, aber nachdem diese Operation sieben Wochen mit etwas 50 Pfund Steinöl fortgesetzt worden war, erhielt ich doch kein Product von irgend constantem Siedepunkt. Nach diesen Versuchen halte ich es für Unmöglich, das Steinöl durch fractionirte Destillationen allein in Producte mit constantem Siedepunkt, zu scheiden."

of separating from petroleum, by fractional distillation, products of constant boiling-point.

Such is the general character of the results obtained in the attempts which have been made to separate the constituents of such mixtures by fractional distillation.

The treatment with strong acids, etc., as an auxiliary to the common method of fractional distillation, which is claimed to have given good results in some cases, is open to serious objections in its application to mixtures of unknown substances, as must be readily apparent. The further consideration of this subject is reserved for another occasion, when I shall submit the results which I have obtained by my process in the study of mixtures almost identical with some of those in the investigation of which the acid process has been employed. I shall then be able to show that the results obtained by that process are, to a considerable extent, inaccurate and by no means exhaustive; and that it is still of the highest importance to have a process which shall be generally applicable in all such cases, without resort to any harsh or uncertain treatment.

With regard to the value of constancy of boiling-point above referred to, as a test of purity of a liquid substance, I may here say that, without scarcely lessening the importance of obtaining constancy of boiling-point, before resorting to harsher treatment, in the study of mixtures of unknown substances, I think I shall be able to show, on another occasion, that this property is not necessarily indicative of so high a degree of purity as has generally been supposed; and that a body may have a constant boiling-point, and yet contain enough of a foreign substance to appreciably—and, in delicate cases, seriously—affect the determination of its constitution and of some of its other properties. But in no such case have I yet found that the removal of the impurity by chemical means has essentially changed the boiling-point,—i. e., never to the extent of 1° C. of temperature. I propose, at a future time, to study this question synthetically, operating with pure liquid substances, with the view to determine, in a few cases, how much of a foreign substance may be present,—which would probably be variable in different cases,—without sensibly affecting the boiling-point. A solution of this question would, I think, be of considerable practical value in some instances.[*]

Of the New Process.—The chief distinctive feature of my process, as compared with the common one, consists in this,—that the operator has complete and easy control of the temperature of the vapors given off in distillation; and consequently can readily cool these vapors to the lowest limit of temperature which the most volatile portion, under the circumstances, is able to bear and retain its vaporous condition. It will be seen at a glance that, under these conditions, the operator has it in his power to secure in any case the very largest possible amount of condensation of the heavier from the lighter vapors. The liquids resulting from the condensation of the less volatile portions of course fall back into the retort, while the vapors of the more volatile parts continue to go forward to a cold condenser, descending in the opposite direction, from which the condensed product falls into a special receiver. In this manner he is able to obtain, in each successive operation, a series of products which shall contain the minimum quantity of the less volatile constituents, which a single distillation is capable of affording.

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[*] Since this was prepared for the press, I notice that late experiments by Berthelot go to show the correctness of my conception of the value of constancy of boiling-point, as above stated.

Of the common process, on the contrary, nearly the reverse of all this is true: the operator having no control whatever; being forced to receive the vapors at the temperature which they naturally acquire in passing from the retort, and laden with such proportion of the less volatile bodies as may be carried forward with them.[10]

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[10] The only apparatus, of which I have any knowledge, which can be regarded as bearing any analogy to my own, is that employed in the rectification of alcoholic spirits, on a manufacturing scale. In one of the older forms of this apparatus, that of Solimani, to which my attention was first called by a friend, after my process had been in use more than a twelvemonth, the temperature of a dephlegmator is kept within such limits as to give alcohol of any required strength more readily than by the common methods. The mode of construction of this apparatus is, however, only adapted to manufacturing purposes, and it could not be utilized in the more exact experiments required in scientific research. Either on account of its complication, or some other cause, the apparatus of Solimani has, I believe, long since been abandoned.

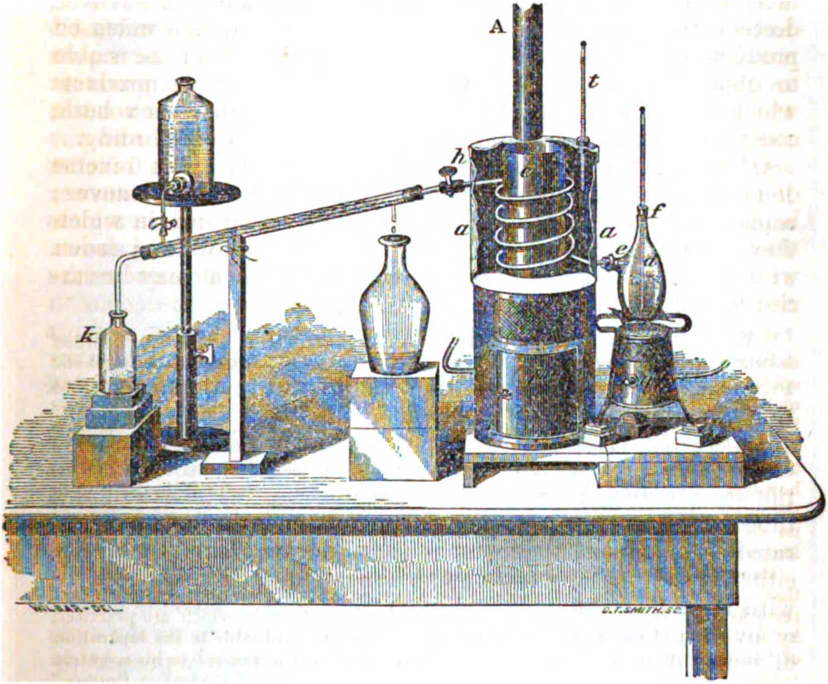
Mansfield (*Quarterly Journal of the Chemical Society*, 1849, i, 264), observing that "the boiling-point of benzole is the same as that of alcohol of sp. gr. 0·825," remarks that "any of the summary processes of rectification which are practised by distillers in the manufacture of alcoholic spirits, are applicable to the separation of benzole from the less volatile fluids of naphtha;" and, appended to his scientific treatise on coal-tar, under the title "*Of a Practical Mode of Preparing Benzole,*" goes on to describe a process for that purpose, which, I believe, he had previously patented. It appears that Mansfield did not employ this process in his research, but obtained his benzole, as well as the other less volatile hydrocarbons, in the usual manner,—by simple distillation.

In the belief that no process of fractioning at all analogous to mine has ever been employed in scientific research, and that I am not in any way directly indebted to any of the devices of my predecessors, I have taken no special pains to consider these devices in much detail. I may say, however, that I have found no record of any one's ever having employed the oil bath and a separate fire to regulate a heated condenser, this being the essential feature on which the superiority of my process is based; adapting it at once to both high and low temperatures, and for the most delicate work.

The employment of bulbs, above referred to, as proposed by Wurtz, is simply a modification of the old process. The bulb apparatus furnishes the same, or, at most, but slightly better results than a simple retort; being no more than equivalent to increasing the height of the sides of the retort itself, without introducing any control over the accuracy of the results; the only advantage gained being, that these results are obtained somewhat more quickly.

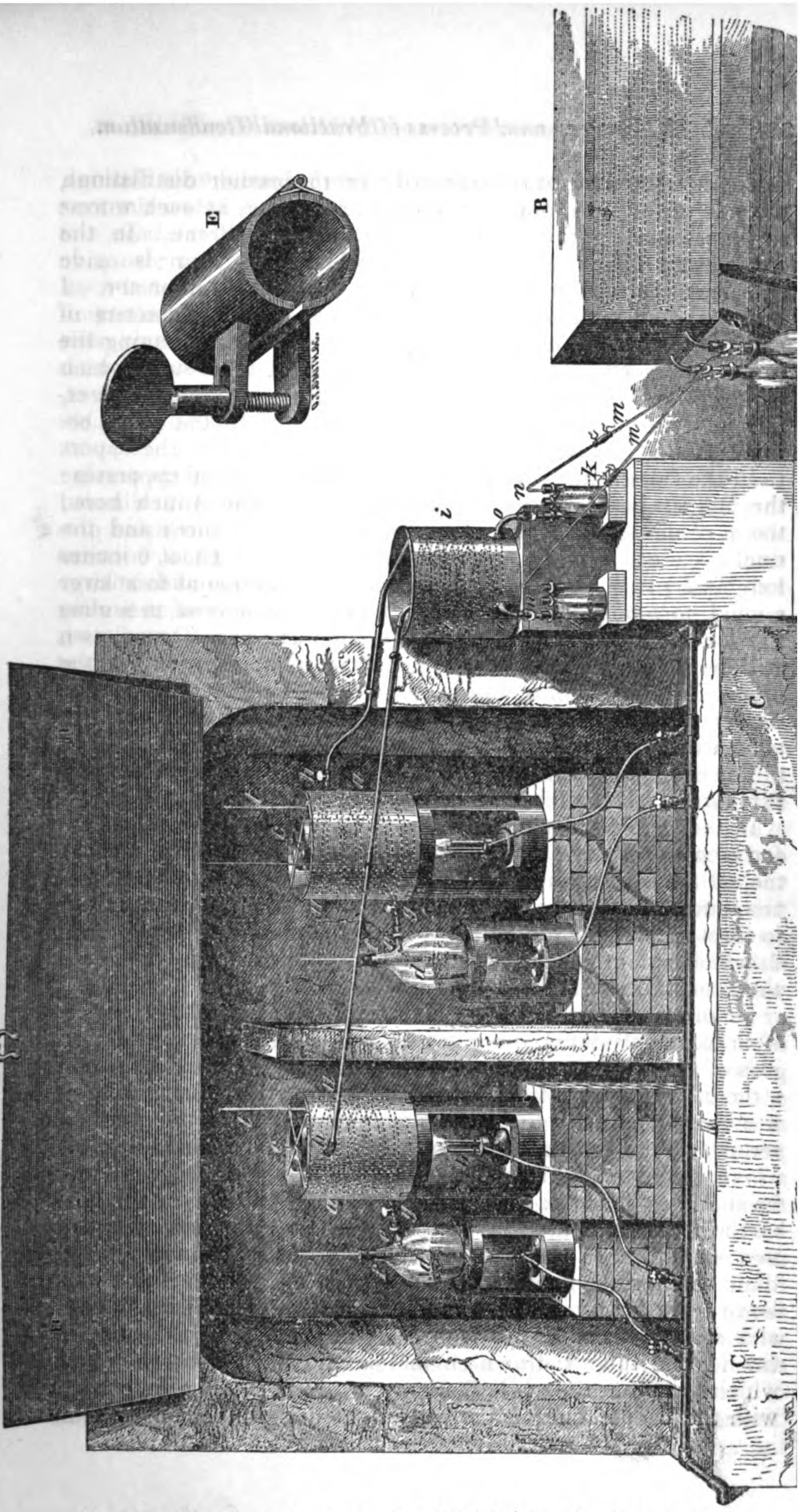
In the new process, perfect control of the temperature of the vapors is secured by simply conducting these vapors upward through a worm contained in a bath, *aa*, figs. 1 and 2, the temperature of which is regulated by means of a separate lamp, *b*, fig. 2, or by a safety-furnace, *p*, as shown in fig. 1. The bath may be of oil or water, or of metal for very high temperatures, as the case may require, and is furnished with a thermometer, *t*.

1.



That this bath may be equally adapted for the separation of liquids boiling below the common temperature, an empty vessel, *c*, figs. 1 and 2, is permanently secured in the interior of the bath by means of straps of metal across the top, to serve as a convenient receptacle for ice or iced water, by means of which a low temperature may be steadily maintained. The interior vessel also serves a good purpose in economizing time, and fuel in heating the bath, as it diminishes the quantity of oil required to cover the worm. It is made to extend to within about three inches of the bottom of the bath, and large enough to fill the greater part of the space in the center of the coil. The bath and interior vessel are both made of sheet-copper, with joints brazed so that they will bear a high temperature. I generally use, also, copper worms, especially in the earlier distillations, the quantities then operated upon being larger, as such worms are conveniently procured, and not liable to break. In the larger-sized apparatus, the tube of which the worm is made measures ten feet in length and half an inch in diameter. I have tried several lengths of worm and several diameters of tube, but not, as yet, with any special view of determining the precise proportions, in relation to the size of the retort, which would be best adapted to the purpose. There appears, however, to be nothing gained by increasing the length of the worm beyond what is required to reduce the temperature of the vapors to that of the bath. I have in use three sizes of apparatus: the largest has a copper worm 10 feet long and ½ inch bore; the medium size, a worm 5 feet long and ⅜ inch bore; and the smallest size, for very small quantities, a worm 1 foot 6 inches long and ¼ inch bore. Each of these has been found to answer a good purpose. The distillation may be conducted in a glass flask, or more conveniently in a glass retort of the form shown at *d*, figs. 1 and 2. The body of this retort, as appears in the figure, is of the form of the corresponding part of the common retort; but which, in place of a long neck, has only a short tubulure, *e*, in the side, for escape of the vapors, and another tubulure, *f*, in the top, which contains the thermometer, and through which the retort is charged.

In the larger apparatus the retort is connected with the lower end of the elevated worm by means of a glass tube of about the same diameter as the end of the worm. One end of this tube enters the retort at the lateral tubulure through a perforated cork, and the other end is joined to the end of the worm, either by being firmly bound with a strip of cloth thickly covered with vulcanized caoutchouc,—such as is found in commerce,—or by means of a perforated cork, which is made to fit the ends of both tubes as snugly as possible, and then tightly pressed together upon the joint by means of an iron clamp, as shown at *g*, fig. 2. This clamp is figured on a larger scale at E. As it is highly important that all joints in the apparatus should be perfectly tight, inasmuch as the least leakage, when continued a long time, would cause, in the aggregate, a serious loss of material, I would call special attention to the clamp joint as the best which I have tried. Before falling upon this device I had used exclusively the vulcanized caoutchouc joints, which were found to answer a good purpose, in most cases, except that they required too frequent renewal. I have found the cloth covered with vulcanized caoutchouc preferable to the common caoutchouc tubing. In the smaller sizes of apparatus I have the end of the worm itself project far enough from the bath to connect directly with the retort by means of a perforated cork, without the use of an additional connecting tube.


E
B
i
o
n
k
m
m
C
C

The upper end, *h*, of the elevated worm is brought out through the side of the bath at a point about three inches below the top; so that, when working with a low temperature of the bath, the worm may still be completely covered with oil, and also give sufficient space above the worm for the expansion of the oil when higher temperatures are employed. To avoid contaminating the atmosphere of the laboratory with the disagreeable fumes which are given off, in large quantity, from such a mass of heated oil, the top of the bath is tightly closed with a sheet-iron cover, from which a small funnel, A, fig. 1, conducts these fumes to a chimney.

In the larger apparatus, the vapors which succeed in passing through the heated worm are conducted downward into a cooled worm contained in a bath of water, *ii*, fig. 2, and the liquid product is collected in the receiver, *k*. The cold bath, *ii*, contains two condensing worms,—one for each apparatus,—and is large enough to condense for both without the necessity of renewing the water. I have represented two apparatuses combined, as it will be found more economical of time to operate with two at once. In the smaller apparatus, for the table, a Liebig condenser may be conveniently substituted for the cold worm, as shown in fig. 1.

For collecting liquids which boil below the common temperature, when such are present, I attach a refrigerator, B, fig. 2, which is provided with two block-tin condensing-tubes,—one for each apparatus. These are bent in a zigzag form, and attached to the inner sides of the refrigerator. The lower ends of the tubes extend through the end of the refrigerator far enough to form a convenient connection with the second receiver, *l*, fig. 2, which communicates with the first receiver, *k*, by means of the glass tube, *m*.

In order to successfully collect and condense the vapors of such extremely volatile liquids as are now under consideration, it is of course indispensable that the apparatus should be constructed with very tight joints; and for greater convenience, but more especially to prevent breakage, such of the joints as require to be frequently taken apart should be made flexible. A very convenient and perfectly tight joint of this kind may be made as follows:—the short stationary tube, *n*, in the cork of the receiver, *k*, fig. 2, is made with the opening somewhat divergent upward; the end, *o*, of the worm is enough smaller than the inside diameter of the upper end of the tube, *n*, to leave room for a piece of caoutchouc tube to be drawn over it, and still admit of its being inserted in the end of the tube, *n*; the flexible tube is drawn on far enough to prevent the drops which form on the end of the worm from coming in contact with the caoutchouc; a perfectly tight and convenient flexible joint is now made by pressing the tube, *n*, over the caoutchouc covering of the end of the worm, *o*. The joints of the receivers, *ll*, are made in the same manner.

The vapors which escape condensation in *ii* pass through the receivers, *kk* and *ll*, to the refrigerator, B, which contains ice, or a mixture of ice and salt, are there condensed and fall back into the receivers, *ll*; which should stand in a wooden vessel also containing ice or a freezing mixture. The refrigerator, B, is made with double bottom and sides, with an inch space between, which is filled with pulverized charcoal. Being tightly covered, a charge of ice and salt will serve for a long day's operations without renewal. In this manner I have been able to collect, in considerable quantity, bodies boiling nearly at 0° C., and this from mixtures in which such bodies had been quite overlooked by previous investigators.

It will be observed, on reference to fig. 2, that the larger distilling apparatus is represented as standing in a brick fire-place, with brick-work, CC, a few inches high, built up in front; and a sheet-iron apron, DD, folded above. This is for security against fire in case of accident, either to the retort or hot bath of oil. As arranged, the contents of either or both of these could run out and burn without danger to the operator or the premises, as the brick-work in front would prevent the liquid from spreading beyond the fire-place, and the dropping of the sheet-iron apron would cause an additional draft, and thus insure the passage of the flames into the chimney. Instead of placing the apparatus in a fire-place, where that is not convenient, equal security against accidents may be attained by the use of my safety heating-lamp,[11] *q*, fig. 1, to heat the retort, and safety-furnace, *p*, containing a Bunsen's burner, for heating the bath. The bottom of this furnace, and also a large part of the sides, is formed of wire gauze, such as described for the safety-lamp.[12] The gauze upon the bottom need not be permanently attached to the furnace, but may be simply laid over an opening cut in the stool or board on which the furnace is to be placed; if the furnace be then set upon it, taking care that the joint shall be tight around the edge, nothing more will be required. A strip of vulcanized caoutchouc, about an eighth of an inch in thickness, is riveted around the edge of the opening for the door; against this the door tightly closes, so that no ignition can take place through the cracks which would otherwise remain under the edges of the door.

For an apparatus to stand upon the table, the safety-lamp and furnace are especially desirable. I have also used them for the larger apparatus, placed upon the floor of the laboratory. As a practical test of the security which they afford, I may relate an incident which happened to myself. I had left the laboratory for a short time, with such an apparatus in full operation; the retort containing nearly a quart of light petroleum boiling below 100° C. Having been detained longer than I expected, on returning I found the laboratory filled with the vapors of hydrocarbons; and, on approaching the retort, found that the caoutchouc joint, connecting the retort with the elevated worm, had failed, and that the larger portion of the liquid had distilled into the room, having been mainly condensed in the upper worm, and conducted thence down the outside of the retort into the safety-lamp. This process was still going on, the lamp being highly-heated from the excess of fuel thus added to it, but no ignition took place outside the lamp. Although this experiment was rather injudicious, it furnishes a valuable test of the efficiency of the safety-lamp and furnace.

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[11] This Journal, 1862, [2], xxxiii, 275.

[12] Loc. cit.

Having described the apparatus, I now proceed to give such details of the method of conducting the separations as have been found, in my experience, most efficient and economical of time. In commencing with a crude mixture of unknown liquids, I deem it advisable to operate at once on a tolerably large quantity of material, especially if the constituents are supposed to be numerous, and to omit chemical treatment till after the separations have so far progressed as to indicate the number and species of bodies present, and, approximately, their several boiling-points.

Notwithstanding the precautions taken to avoid loss from evaporation and leakage, I have at times been surprised at the large waste of material which has been made apparent after a long series of operations. When it is considered, however, that the time required to make a complete separation of a very complex mixture of liquids must necessarily be very protracted, during which more or less of evaporation is constantly taking place, it will be a matter of no surprise that the loss is so considerable. The quantity of material required must depend also on the proportions in which the various constituents are contained in the crude mixture, and upon their degree of volatility; but as these cannot be known *a priori*, it may suffice to make a single preliminary distillation of a portion of the mixture, from a tubulated retort, to ascertain the range of temperature within which it distills, noting at the same time the proportions which come over between certain temperatures; as, for example, below 50° C.; between 50° and 100°, etc.; from these data one may judge pretty nearly of the quantity which it will be advisable to take. It is evident that, when very volatile bodies are present, even in considerable proportion, a much larger quantity would be required than if the material were but slightly volatile; as the waste in the former case, from evaporation, would be much greater.

But in many cases it will be found that highly volatile bodies are present only in very small proportion,—e. g., in viscid petroleum like Rangoon tar, and in the products of distillation of some species of asphalt. In such cases, the requisite quantity to be operated upon, to obtain the most volatile constituents in sufficient quantity for anything like a complete study of their chemical relations, would be extremely large,—too large to be conducted in the laboratory,—and one would have to resort to the manufactory for the first distillation. I have dwelt at some length on this point, having experienced the disappointment which one feels, after months of labor, on finding the products insufficient for his requirements, when the expenditure of a little more time, comparatively, might have given double the quantities obtained.

In the first series of fractioning, I generally operate on successive portions, of about one gallon each, of the crude material, and take off a fraction for every 20° C. rise of temperature of the retort. These fractions are preserved in well-stoppered bottles, and each carefully labelled with the temperatures between which it was obtained. The fractions for each fresh portion of the crude material, being collected between the same limits of temperature, are added to the corresponding products from the preceding operations, till enough of the crude material has been taken to insure, ultimately, a sufficiency of the pure products.

In the commencement, not only of this but of all subsequent fractionings, when the temperature to which the bath should be raised is unknown, I first bring the liquid in the retort into full ebullition, so that a steady stream of liquid shall flow back from the end of the worm into the retort. I then carefully raise the temperature of the bath until the vapors from the retort pass through the heated worm so freely that the liquid, in condensing from them, shall drop with tolerable rapidity into the cold receiver. In order that this dropping may be continuous, it is necessary that the temperature of the bath should rise *very gradually* as the more volatile constituents of the mixture are taken off; this is easily effected by carefully regulating the flame under the bath.

It is advisable to boil the retort as rapidly as possible without choking the lower end of the heated worm with the returning liquid. As this choking would give rise to additional pressure in the retort, and consequently occasion abnormal elevation of the temperature, and possibly a rush of liquid into the receiver, and thus introduce irregularities in the work, excessive heat under the retort should be avoided. The first indication of choking of the worm is a partial or entire stoppage of the stream of liquid which normally flows steadily from the end of the worm into the retort. Any interruption or unsteadiness of this flow would indicate too rapid ebullition.

As a rule, other things being equal, the greater the difference between the temperature of the bath and that of the retort, the slower the products will come off, and the more effectual will be the separation. I think it possible, however, that the earlier fractionings may be conducted so slowly that the loss of time would more than counterbalance what might be gained by more thorough separation, and that equally good results may be more economically obtained by more frequent operations, somewhat more rapidly conducted.

A striking illustration of the advantage to be gained by this process is presented by the fact that, during the first fractioning of a crude mixture, such as American petroleum or coal-tar naphtha, for example, the difference between the temperature of the bath and that of the retort may sometimes be as much as 85° C., or even more. While, as the products become purer, this difference between the temperatures of the bath and retort proportionally decreases, till finally, in operating on a pure product, the temperature of the bath must be brought to within a few degrees of that of the retort, in order to bring the vapors through. But the amount of this difference is variable for different bodies of equal purity.

The first fractionings must necessarily be quite arbitrary; for, as a general rule, when operating on such mixtures as those just mentioned, neither the thermometer nor the quantities obtained for any given range of temperature will indicate any decided preponderance of any one substance. On the contrary, the temperature rises uniformly, and about the same quantity is generally obtained for the same number of degrees of temperature throughout the operation. In other mixtures, in which certain bodies may seem to be present in much larger proportion than others, or in which there may be a greater difference between the boiling-points of the constituents than in the cases referred to,—facts which would be indicated by the thermometer of the retort, and by the relative quantities of the products obtained,—there might be something gained by exercising discretion in taking off fractions according to these indications.

In the second series of fractioning, the first or lowest fraction of the preceding series, which is large enough to operate upon by itself, is transferred to the retort, and brought into ebullition. The temperature of the bath is then adjusted as above described, and the distillation continued, the fractions obtained being placed in their appropriate bottles until the temperature of the retort shall have risen to, or somewhat above, the point at which the second or next succeeding fraction of the first series may be supposed, or has been found by experiment, to boil. This fraction is then added to the residue in the retort, and the distillation is continued as before. In the same manner, I proceed with the remaining fractions of the first series.

All subsequent fractionings are similarly conducted. As the work progresses, however, the fractions are taken for a gradually decreasing number of degrees of temperature, until finally it becomes necessary, for the attainment of absolute constancy of boiling-point, to take off a fraction of every degree, centigrade; and to continue thus to operate on these fractions, each representing one degree of temperature, until the desired end is attained.

The operator will observe that, in each series of fractions, in which each fraction has been taken for the same range of temperature, the difference between the boiling-points of any two contiguous fractions is nearly the same as the difference between any other two contiguous fractions,—in other words, that the difference referred to approximates to a common difference throughout the same series. Once ascertained, this difference serves as a valuable guide in determining with sufficient accuracy when to add the next fraction to the retort. By observing this systematic course, irregularities, from the improper mixture of products, may be avoided, and time thus economized.

After a few series of fractionings,—sometimes after two or three, variable in number, according to the nature or complication of the mixture,—it will be found that some of the fractions are considerably larger than others for the same range of temperature, indicating approximately the boiling-points of the several constituents. But fractions of constant boiling-point, or those, the boiling-points of which cannot be sensibly changed by further fractional condensation, are not obtained, as already mentioned, till after repeated careful fractioning of every degree of temperature. When fractioning of every degree, it is important to use every precaution to protect the thermometer from external influences, and to carefully apply the corrections for variations in the atmospheric pressure. This may even be desirable earlier; but it is of so much importance in the case specified, that, if omitted, the operator would be liable one day to mix products which he had separated the day previous.

In this way, certain larger fractions are obtained, which are not susceptible of further alteration in their boiling-points; but there are yet considerable quantities of liquid in the intermediate fractions, which still continue to change more or less in each succeeding operation. When the fractions of constant boiling-point have once been obtained, if it were not important to test for other bodies in the intermediate fractions, the operation might here be suspended, provided the pure products already obtained should be large enough for the purposes required.

But, in my investigations, I have undertaken to prove the negative as well as the positive. I have attempted to carry the process of separation so far, that I might assert the absence of other bodies, as well as the presence of those obtained; and this clearing up of the intermediate fractions has generally been the most tedious part of the work. I have continued to operate upon these by themselves, until they also have become distributed in regular course—no new bodies appearing—among the fractions of constant boiling point, or to such an extent that the intermediate quantities have become too small to admit of further continuance of the process.

This process has been in constant use in my laboratory during the last three years. In this time it has been applied in the study of petroleums, coal oils, the more volatile parts of coal- and wood-tars, the essential oil of cumin, commercial fusel oil, from corn whiskey, and even to mixtures more complex than either of these. As the result of this long experience, I can say that, as regards bodies not decomposed by heat in distillation, I have not yet found a mixture so complex that it may not be resolved by this process into its proximate constituents so completely, that these shall have almost absolutely constant boiling-points. In repeated instances, even from petroleums, I have obtained these constituents so pure, that the contents of an ordinary tubulated retort charged with one of them has been completely distilled off without any essential change of temperature; i. e., not to the amount of ½° C., the thermometer frequently remaining absolutely constant for more than half an hour, a constancy of boiling-point not exceeded by that of distilled water. This state of purity, I think I may safely assert, has never before been attained from such mixtures, by any system of fractional distillation.

As I shall soon be prepared to present to the Academy detailed results of the investigations above referred to, I may omit further allusion to them on this occasion.

I would remark, in conclusion, that it seems to me not improbable that this process may ultimately prove to be of great value in the arts. It is not too much to anticipate that, whenever the various constituents of the mixtures referred to shall have been separately and thoroughly studied in a pure state, some of them may be found to possess properties which will give to them great commercial value, sufficient to justify the expenditure necessary to separate them in large quantities.

Art. XLII.—*Examination of Petroleum from California;* by B. Silliman.

The specimen of petroleum here described came from a natural well or spring on the Simi estate in Santa Barbara county, California. It issues from rocks of the Tertiary Age. The discharge from this spring is sufficient to flow nearly two miles down the dry bed of the ravine in which it occurs.

It is of a dark brown color, dichroic, thin and mobile as water, and of a faint naphtha odor, quite without offensive smell. In a thin tube it has a yellowish brown color by transmitted light, and obviously owes its dark color to its holding in solution a portion of the asphaltum with which it is associated.

Its density is 0·861 at 15° C. or about 34·30° B. It burns, in its crude state, in a double current lamp, without smoke, with quite a bright flame and strong light for a few moments. After eight or ten minutes the wick commences to coal, and after about fifteen minutes it smokes and finally dies out.

1000 c. c. of the crude substance were subjected to fractional distillation in a tubulated retort to which was adapted a thermometer and condenser. A condensible vapor appeared at 60° C., the liquid simmered at 90° and boiled at 123°.

Of the 1000 c. c. of crude oil there were distilled from glass up to the boiling point of

Mercury,	500	c. c.
In iron,	496	“
Loss and coke,	4	“
	1000	“

This method of practical distillation gives, as is well known, very unsatisfactory results, as compared with the method of Warren.[1] As the experiment was, however, very carefully conducted by Mr. Peter Collier, of the Sheffield Laboratory, under my directions, it furnishes a good illustration of the differences arising solely out of an imperfect method of analysis, when practiced on the same raw material. I therefore append the results in detail.

The temperatures were noted by a mercurial thermometer the bulb of which was continually in the boiling liquid.

20	c.c.	had	distilled	at	153°	C.		260	c.c.	had	distilled	at	234°	C.
40	“	“	“	“	170	“		300	“	“	“	“	250	“
100	“	“	“	“	177	“		360	“	“	“	“	266	“
140	“	“	“	“	190	“		400	“	“	“	“	280	“
160	“	“	“	“	200	“		450	“	“	“	“	297	“
200	“	“	“	“	213	“		485	“	“	“	“	320	“
240	“	“	“	“	223	“		500	“	“	“	“	370	“

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[1] See this vol., p. 327.

When about 485 c. c. had passed over, the temperature rose very suddenly from 320° to 370° C.

The remaining 460 c. c. were distilled without noting temperature, in an iron retort, leaving a light carbonaceous residue.

The successive fractions gave the following sp. gr. and explosive tests, the latter being the commercial standard as marking the temperature at which the liquid exhales an inflammable vapor.

	Sp. gr.	Beaumé.	Expl. test.
1st, 100 c. c.	755	55·77	58° F.
2nd, 100 "	775	51·80	70° "
3rd, 100 "	793	47·62	100° "
4th, 100 "	815	42·70	156° "
5th, 100 "	838	37·83	196° "
6th, 100 "	867	32·08	190° "
7th, 50 "	872	31·13	190° "
8th, 100 "	890	27·79	181° "
9th, 100 "	900	26·00	171° "
10th, 100 "	900	26·00	140° "

The color of these products varies from Nos. 1 and 2, which are quite colorless, through 3 to 7, which, commencing in 3 as a faint pink, become red in 6 and 7, while 8, 9 and 10 can hardly be distinguished in color from much of the better quality of crude oil flowing from the best wells, having the same change of color (dichroism) by reflected and transmitted light. The unrectified products would arrange themselves thus by color—

I.	1st,	2nd,	3rd,		fractions.
II.	4th,	5th,	6th,	7th,	"
III.	8th,	9th,	10th,		"

But in the actual conduct of the manufacture of this oil, I presume the distiller would make three divisions:

1st,	Light oil,	including	the			1st	fractions,	about	53·5°	commercial.
2d,	Burning oil,	"	"	3	to	7	"	"	38·5°	"
3d,	Lubricating oil,		"	8	to	10	"	"	28·0°	"

By mingling a portion of the light oil with the heavier, it is probable that a product of about 60 per cent would be obtained suitable for illumination.

The light oil and the burning oil are perfectly decolorized by the usual treatment with sulphuric acid and carbonate of soda. The light oil has a highly agreeable smell, and none of the other products possess any of the heavy and disagreeable odors common in the petroleum products. The burning oil has a very high illuminating power, and fails to char or encrust the lamp-wick after an hour's burning. The explosive test is 155° F.

The densities of various specimens of crude California petroleum which I have determined range from 0·859 to 0·972, the difference being apparently due to the greater or less degree of evaporation and oxydation of the products consequent on exposure to the warm air of a very dry climate. The specimens thus far examined from California are all from surface springs. When the oil is drawn from artesian borings the same change of density will undoubtedly be observed as in Pennsylvania. I found the surface oil of Oil Creek, in 1855, having a density of ·880 to ·885, thick in warm weather as thin molasses, and in cold weather quite viscid. The flow of wells in the same region is now quite thin, and has a density of ·800 to ·850.

Appended are the results of Mr. Warren's examination of the same California oil now under consideration.

"To Prof. SILLIMAN, New Haven.

"*Dear Sir*—The results of my examination of the crude California petroleum which you sent me through Messrs. Spear, Burke & Co., of this city, and which bore the seal of Messrs. Wyeth & Bro., are as follows:

"Specific gravity at 17 c. 0·864=33° Beaumé.
1250 c. c. of the crude substance subjected to
my process of fractional condensation, gave—
41 c. c. of light oil between 93° and 100° C. } Specific gravity not taken.[*]
43 " " " " 100° and 140° C.
525 " Burning oil " 140° and 310° C. sp. gr.=44° B.
From this point the distillation was conducted in the ordinary manner from a common retort, no thermometer being used.
270 " lubricating oil of sp. gr. 29½° Beaumé.
293 " " " " 28° "

1172 c. c. = 93·8 per cent of total product.

"The residue left in the retort was dry coke. After treatment with sulphuric acid and alkali the light oil and the burning oil were nearly or quite colorless. The lubricating oil had a yellowish color. The odor of the burning oil was extremely agreeable, fully equal in this respect to the best kerosene or refined Pennsylvania petroleum. The other products were also entirely free from disagreeable odor, and indeed the same may be said of the crude oil itself. In this respect it is readily distinguishable from the Pennsylvania petroleum.

"The burning properties of the illuminating oil are not surpassed by any oil which I have seen.

"What I have called *light oil* is not very volatile, and would not, I think, rank in the market as naphtha, at least not that taken between 100° and 140° C.; and it is my opinion that the burning oil would take the whole of the light oil, and still bear the commercial fire test. In that case some of the lighter of the lubricating oil probably might be run into the burning oil, so that the yield of the latter would be over 50 per cent."

Boston, March 31, 1865."

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[*] The specific gravity of the mixture determined by me is 0·753=58° Beaumé.—S.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On the determination of lime.*—Fritzsche has made the very simple yet important discovery that oxalate of lime may be easily and completely converted into caustic lime free from every trace of carbonate by simple ignition over a blast lamp. The tedious processes recommended in works on analytical chemistry, such as ignition with carbonate of ammonia, conversion into sulphate of lime, etc., become unnecessary, and will hardly be employed in future. The author used in his experiments a platinum crucible 33^{mm}. in height and 27^{mm}. in width at the top: the ignition took place over a gas lamp consisting of six Bunsen's burners, (the ordinary Bunsen's blast lamp would doubtless answer even better). In four experiments Fritzsche obtained from known weights of oxalate of lime 99·9, 100·08, 100·08 and 99·98 per cent of the calculated quantity of lime. In all these experiments the ignition was not continued longer than 15 minutes. In a large platinum crucible, quantities of the oxalate not exceeding one gram could be rendered perfectly caustic in 15 minutes, but 30 minutes were necessary for a quantity of more than 3 grams. Precipitated carbonate of lime was also completely converted into caustic lime by ignition, and the method, as giving results which are more accurate and more easily obtained than those of the older processes, will doubtless come at once into general use.—*Journal für prakt. Chemie*, xciii, 335. W. G.

2. *On the sulphur compounds of uranium.*—Remelè has found that the brown precipitate which sulphid of ammonium produces in a solution of nitrate of uranium is neither sulphid of uranium nor a mixture of protoxyd of uranium and sulphur as was formerly supposed. The precipitate changes rapidly when formed in an aqueous solution and becomes orange and then yellow. When formed in an alcoholic solution, the precipitate is permanent and has the formula U_2O_2S, so that it is analogous to the well known chlorid U_2O_2Cl. By boiling in presence of sulphid of ammonium the compound is easily decomposed into sulphur and hydrate of protoxyd of uranium.—*Ann. der Chemie und Pharmacie*, III *Supplementband*, p. 196. W. G.

3. *Note on the regenerative gas-furnaces of the brothers Siemens.*—The regenerative gas-furnaces of the Messrs. Siemens are so interesting and suggestive that we make no apology for again calling attention to them, supposing of course that their general features are sufficiently familiar from the description given by Dr. Faraday, and quoted in the last number of this Journal. Two points which appear to us of special importance are, first, the extremely high temperature which can be obtained and which in fact is limited only by the nature of the materials employed in the construction of the furnace, and secondly, the possibility of employing at will either an oxydizing or a reducing atmosphere. The brothers Siemens have already applied their furnaces to puddling and reheating, but we venture to predict for them a very extensive use in metallurgical processes. Thus, it is well worth while to determine by direct experiment

upon a large scale whether the rich iron ores of Lake Champlain, Lake Superior and Missouri, cannot be directly reduced to the metallic state by heating them to a sufficiently high temperature in the chamber of a Siemens furnace, and then changing the gaseous mixture in the furnace to a reducing condition. This would in fact be blooming upon a large scale, and would not improbably avoid the inconvenience and expense of blooming in the small way, which in spite of the superior quality of the iron produced has been almost wholly superseded by the cheaper process of puddling. Experiment only can determine whether fluxes can be used with advantage in blooming in this manner, when poorer ores are employed. Ores of copper could doubtless be roasted and reduced in furnaces of this construction, and with some additions to the original plan, the sulphurous acid formed during the roasting might be directly converted into sulphuric acid in leaden chambers. But it is for the metallurgy of iron that the new furnaces will probably be found most advantageous. As the temperature attainable is extremely high, it may even be found practicable to melt the malleable iron formed by the direct reduction of the ore, the walls of the fire-chamber being lined with lime as more refractory than any fire-clay. But even if this rather bold suggestion should not be realized, it is at least probable that the earthy impurities of the ore would be reduced to a peculiarly fluid condition, so that the blooms could be easily treated under the hammer and brought into the form of malleable iron. For the rest, there is hardly a branch of manufacture in which heat is employed upon the large scale in which furnaces on the regenerative principle would not find an application. We suggest that small gas furnaces could be made upon the same principle for laboratory use and for various processes in the arts, using the ordinary city gas, however, as fuel instead of gas produced by a special furnace. The high temperature obtained in Gore's gas-furnaces appears to be due to the heating of the air and gas before they mix in combustion. W. G.

4. *On some aluminum compounds.*—BUCKTON and ODLING have studied the action of aluminum upon ethyl-mercury and methyl-mercury, and have thus obtained ethyl-aluminum and methyl-aluminum without difficulty and in considerable quantity. Ethyl-mercury, or, as the authors term it, mercuric ethid, with an excess of clippings of aluminum, was heated for some hours in sealed tubes in a water bath, when the mercury was found to be completely displaced by the aluminum. After distillation the aluminic ethid was obtained as a colorless mobile liquid which boiled at 194° C. and did not solidify at −18° C. It evolved dense white fumes on exposure to the air, and when in thin layers took fire spontaneously burning with a bluish-red edged flame, and producing an abundant smoke of alumina. Analysis gave nearly the formula $Al_2(C_4H_5)_3$ or $Al_4(C_4H_5)_6$. The vapor-density at 234° C. was found to be 4·5, the theoretical number for $Al_2(C_4H_5)_3$ being 3·9, whence it would appear that the true molecular formula is $Al_2(C_4H_5)_3$ and not $Al_4(C_4H_5)_6$. Water decomposes aluminic ethid with explosive violence. Iodine forms iodo-derivatives and iodid of ethyl. Oxygen gives a body apparently analogous to boric di-oxyethid. Aluminic methid was obtained as a colorless mobile liquid boiling at 130° C. and solidifying a little above 0° to a beautiful transparent crystalline mass. The liquid takes

fire spontaneously in the air. The formula of this body was found to be $Al_2(C_2H_3)_3$, with which the vapor-density 2·80 closely agrees, the theoretical number being 2·5. But the vapor-density was found to increase very rapidly as the temperature diminished, so that it is probable that at a temperature not far removed from the boiling point the molecular formula would be $Al_4(C_2H_3)_6$. The authors ask whether the observed vapor-density of aluminic chlorid may not correspond to the high density of aluminic methid, so that both may be anomalous and untrustworthy for determining the general formulæ of aluminum compounds. —*Chemical News*, No. 271, p. 61.

5. *On the density of the vapor of salammoniac.*—H. ST. CLAIRE DEVILLE has repeated his important experiments on the density of the vapor of sal-ammoniac, and has replied to the objections made by various chemists to the conclusions drawn from his first researches. The author shows conclusively, by an ingenious piece of apparatus, that chlorhydric acid and ammonia, each heated to a temperature of 360° C., combine with a sensible evolution of heat, so that at this temperature the vapor the density of which is taken cannot be regarded as a mixture of HCl and NH_3, but must be admitted to be NH_4Cl, the vapor-density of which corresponds to 8 volumes ($H=2$) and not to 4 volumes as in the case of other organic compounds. Deville suggests that the proof that no simple or compound substance corresponds for one equivalent to 1 or to 8 volumes rests with those chemists who make this assertion, and that until that proof is given the assertion amounts to a pure hypothesis. Among other difficulties, the following is suggested to the partisans of the so-called anomalous densities.

(1.) Sulphydric acid forms with ammonia two distinct crystalline and volatile compounds, the formulæ of which are respectively NH_4S, and NH_4S, HS. Sulphid of ammonium represents 4 volumes of vapor; its condensation is $\frac{1}{3}$: sulphydric acid and ammonia combine and remain combined at the temperature (for example 100° C.) at which we determine the density of the vapor. Sulphydrate of sulphid of ammonium represents 8 volumes of vapor; its condensation is nothing. If we suppose its elements separated at the temperature at which we take the vapor-density (100° for example), we must admit that it separates into ammonia and sulphydric acid, NH_3 and $2HS$, each giving four volumes and having 8 volumes as their sum. Now at this temperature the elements can only separate into sulphid of ammonium and sulphydric acid, giving the one 4 volumes and the other 2 volumes, the sum of which is 6. Hence, if the sulphydrate were decomposed at the temperatnre at which the vapor-density is taken, it ought to furnish 6 volumes, and as it furnishes in reality 8 volumes, it follows that it is not decomposed.

(2.) In the case of bodies, simple and compound, which present vapor-densities variable with the temperature, it cannot be said that these densities would become normal if the temperatures could be sufficiently increased, because, as Deville and Troost have shewn, as the temperatures increase the vapor-densities diminish toward a constant limit, so that the coëfficients of expansion must also become constant and differ insensibly from 0·00367. To admit that the coëfficient of expansion of arsenic and phosphorus vapors may be different from this number, in order to show that these vapors do not correspond to one volume (H=2) is to make an assumption which is inadmissible in the present state of science.—*Comptes Rendus*, lix, 1057.

6. *On the dispersion of light produced by the rotation of the plane of polarization in quartz.*—STEFAN has given a beautiful method of forming a spectrum by means of the dispersion of the planes of polarization produced by plates of quartz. A bundle of parallel plane-polarized rays of white light is allowed to fall upon a glass cone, the sides of which make with the axis an angle equal to the angle of polarization. The reflected rays are received upon a screen perpendicular to the axis of the cone and produce a circular sector in which the maximum intensity of light corresponds to the rays, the plane of reflection of which is parallel to the plane of polarization. If we introduce a plate of quartz cut perpendicular to the axis into the path of the bundle of incident parallel rays, the position of the maximum is different for differently colored rays, and the sector then exhibits the different colors of the spectrum. In this manner the dispersion of the planes of polarization is rendered visible. In the same manner, if we receive the polarized bundle of rays transmitted by a plate of quartz upon a plate of spar cut perpendicular to the axis which gives in the analyzer a system of rings traversed by a black cross, each of the branches of this cross will be replaced by a sort of fan exhibiting the different colors of the spectrum.

Measurements the details of which are not yet published have led Stefan to represent the rotations due to a plate of quartz 1^{mm}. in thickness by the formula

$$\varrho = \frac{8{\cdot}1088}{\lambda^2} - 1{\cdot}697$$

in which the wave length λ is expressed in thousandths of a millimeter. From this formula we should deduce the remarkable consequence that the rotation would be zero for rays of a wave length equal to $0{\cdot}002186^{mm}$, and would change its sign when this limit is exceeded.—*Ann. de Chimie et de Physique*, [4], iii, 501. W. G.

7. *On combustion by invisible rays.*—At the Newcastle meeting of the British Association in 1863, Dr. C. R. AKIN proposed three experiments by means of which, if successful, rays of heat could be converted into rays of light. One of these experiments consisted in converging the sun's rays by means of a concave mirror, then cutting off the light by "proper absorbents," and then igniting platinum foil in the focus of the invisible rays. Want of means appears to have prevented the execution of the proposed experiments; but Dr. Tyndall, who had devised experiments of a similar character, and who had discovered a liquid possessing precisely the required properties, has recently succeeded in the conversion of rays of heat into rays of light in a very striking and instructive manner. Tyndall employed an electric lamp of Duboscq and a linear thermo-electric pile, the spectrum being formed by lenses and prisms of rock salt. As in the case of the solar spectrum, the heat was found to increase in intensity from the violet to the red, rising to a maximum beyond the red at a distance about equal to the distance of the green on the more refrangible side. The curve of intensities rises beyond the red into a steep and massive peak, quite dwarfing the radiation of the luminous part of the spectrum. Water interposed cuts away the peak in part; the vapor

of water, according to Tyndall, would do the same, and this explains the difference between the heat-spectra of the electric and solar light, the peak being much higher relatively in the former than in the latter case. After several trials the author found that a solution of iodine in bisulphid of carbon, so opake as to cut off the light of the midday sun, was, within the limits of experiment, absolutely transparent to invisible radiant heat. When the rays from the electric lamp were concentrated into a cone by means of a small glass mirror silvered in front, a cell containing the solution of iodine absorbed the light completely, while the invisible rays of heat being brought to a focus set fire to paper, wood, charcoal and other substances. With a sufficient battery power a plate of platinized platinum is rendered white-hot at the focus of the invisible rays, and then yields a complete and brilliant spectrum. The image of the carbon points of the electric lamp formed upon the platinum plate becomes visible as the plate becomes red-hot, and a species of thermograph is obtained, as is well shown by drawing the points apart or causing them to approach each other, the red-hot image following the motion. In this case the invisible rays of heat have their refrangibility raised in becoming visible, while in the case of fluorescence the invisible rays of light have their refrangibility lowered. Tyndall proposes to apply the term *calorescence* to the phenomena of heat in question, a term which is certainly preferable to the word "calcescence" employed by Akin. We regret to see that a bitter personal controversy has been carried on in a prominent English scientific journal between Dr. Tyndall and Dr. Akin as to the proprietary rights of each in the beautiful experiment above described. Into the merits of this controversy we cannot enter; we may, however, be allowed to express the opinion that the experiment in question does not involve much originality of conception, but that it is a very simple and natural deduction from the experiments and results of Melloni upon smoked rock salt and upon the production of light without sensible heat. —*L. & E. Phil. Mag.*, March, 1865, p. 241. W. G.

8. *On the Electrical properties of Pyroxiline-paper and Gun-cotton;* by Prof. J. Johnston. (In a letter to Prof. Silliman, dated Middletown, Conn., January 25, 1865.)—You did me the honor, a year ago, to publish in the Journal a note of mine on the electrical properties of *pyroxiline paper* and *gun-cotton*, adding an approving note of your own. Having lately had opportunity to make some further experiments with the same substances, I herewith send you the results.

It is proper to say that both the paper and the cotton were prepared nearly a year ago, and may have undergone some change, though nothing of the kind was apparent to the eye, except that one sheet of the paper used seemed at one place a little discolored.

The substances experimented with were *amber*, *sealing wax*, *sulphur*, *gum lac*, *pitch*, *rosin*, *caoutchouc* (native rubber), *hard rubber* (stick obtained of Messrs. J. F. Luhme & Co. of New York), *common vulcanized rubber* (as used in forming gas bags), *gutta percha*, and various crystallized mineral substances. But these last, becoming always positive, will not be further alluded to. The sulphur by friction with the gun-cotton always became positive, and also by friction with different sheets of the paper, except in a single instance, when using the paper which was slightly discolored, it appeared to be feebly negative. Rosin, pitch, gum lac, and amber, both with the paper and the cotton, became always positive, as did also the native rubber, by which I mean the rubber as it is imported. Sealing-wax with the cotton became always positive, but with the paper occasionally negative. Vulcanized rubber (the kind used in making gas bags) would sometimes become positive and sometimes negative, and the same was true of gutta percha, two different specimens being used. The hard rubber (from Luhme & Co.) became always negative, both with the paper and the cotton. This being contrary to the results obtained by yourself with this substance, I made very many trials, but always with the same result.

In all cases after friction with other substances, whether the latter became positive or negative, the paper and cotton would be found invariably negative. Sheets of the paper, when handled, especially in cold, dry weather, often become highly excited,—always negatively, so far as has been determined; and my son informs me that sometimes, when handling considerable quantities of the recently prepared paper, he was even fearful that it might become ignited by the sparks produced!

I will just remark in closing, that in making experiments like these, great care is required in order that the results arrived at may be satisfactory. This is particularly the case when it is necessary to rub a substance, as a roll of sulphur or a stick of sealing-wax, successively with different substances, some of which give the positive and others the negative electricity. Occasionally it will be found that the substance will be positive at some points and negative at others; and in such cases the only way is to lay the particular specimens aside until they shall have returned to their natural condition at all points. Sometimes a substance when first rubbed, after having remained undisturbed twenty-four hours or more, will take on one electricity, but, by continuing the friction a very little time, it will take on the other. Thus, a stick of sealing-wax in its natural state, when gently rubbed, *one* or *two strokes*, with a silk handkerchief, will often be found decidedly positive, but by a few strokes more it will become as decidedly negative; and it cannot be made positive again by friction with silk until allowed by repose first to return to its natural state.

II. MINERALOGY AND GEOLOGY.

1. *On Tin Ore at Durango in Mexico;* by Prof. C. F. CHANDLER.—I have recently examined a sample of 1450 grams of tin ore from Durango, and find it to be a handsome "wood tin" in pebbles and fibrous crusts, some of which are an inch in their longest diameter. The color varies from a very light brown to black.

Associated with the cassiterite, there are brilliant crystals of topaz, some of which are half an inch long. They vary from transparent to opaque, and from colorless to deep brown. I have not been able to examine them very closely, but have noticed the planes O, I, $i\breve{2}$, and 2. Owing to the development of the planes $i\breve{2}$ and 2, and their rich brown color, some of the crystals might easily be mistaken for cassiterite.

The sample examined gave—

Tin (by crucible assay),	50·90
Topaz crystals easily separated by the process,	8·10
Other topaz crystals, too small to be easily separated,	1·00 (?)
Oxygen and impurities (by difference),	45·00
	100·00

The material used for the assay was very carefully averaged, by pulverizing about 500 grams of the ore, after the topaz crystals had been separated. A correction was subsequently made in the result of the assay corresponding to the quantity of topaz removed.

School of Mines, Columbia College, N. Y., Dec. 28.

2. *Note by F. B. Meek and A. H. Worthen in relation to a genus of Crinoids from the Coal-measures of Illinois and Nebraska, proposed by them on p.* 174 *of the last number of this Journal.*—At the time we proposed the name *Erisocrinus* for this genus, we were not aware that M. Koninck had described it under the name *Philocrinus* in 1863, among some Carboniferous fossils from India (see *Memoire sur Foss. Palæozoiques Recueillis dans L'Inde*, p. 21. *Liege*, 1863). Owing to the fact that Mr. Koninck's figure, (probably from the wearing of the stone from which the impressions were taken,) does not show the basal pieces, nor distinctly their connection with the subradials, we had overlooked the generic identity of the American and Indian species. On comparison with his description, however, it becomes evident that our Crinoid agrees exactly in its generic characters with his. This being the case, we cheerfully withdraw our name, and arrange our species, which differ specifically from that described by him, under his name. In doing this it also becomes necessary to change one of our specific names (*E. typus*), since it is not the typical species of Koninck's genus. Hence our species may be called *Philocrinus pelvis* and *P. Nebrascensis.*

It is worthy of note that Mr. Koninck's figure of the arms and secondary radial pieces of this type, together with its other characters, show it to be closely allied to the still older genus *Bursacrinus,* Meek & Worthen, 1861, which only differs in the possession of an anal piece, and the consequent truncation of one of the subradials.

3. *On the Oil Formation in Michigan and elsewhere;* by Prof. A. WINCHELL, of the Michigan University. From a report on the "Baker Tract," near Lakeport, St. Clair Co., Michigan. 8 pp. 8vo. Detroit, 1864.—In most of the Northwestern States occurs a formation which has been known among American geologists as the *Black Slate* or *Black Shale.* It is well known in Ohio, Indiana, and Kentucky, and extends also into the surrounding States. It was formerly supposed by Prof. Hall, to be the western representative of the "Marcellus Shale" of New York; but the researches of western geologists tend rather to prove its equivalency with the "Genesee Shale" of New York. The color of the rock is due to the presence of a large amount of carbonaceous or bituminous matter, believed to be derived from the vegetation of the period in which the shales were deposited.

Similar black shales were brought to light by Dr. Houghton, many years ago, in the lower peninsula of Michigan; and the opinion was expressed by him and by Mr. Hubbard, his assistant, that these shales were of the same geological age as the black slate of Ohio and Indiana. The same formation was subsequently described by Mr. Murray, of the Canada geological survey, as making its appearance at various points in Canada West; and the geologists of that survey speak of the formation as constituting the principal part of the "Hamilton group" as known in Canada. In the prosecution of the geological survey of Michigan, I enjoyed the opportunity to establish, by direct comparisons, the identity between the Black Slate of Michigan and that of Ohio and Indiana, and, by proving that it lies stratigraphically above the limestones of the Hamilton group, have established its distinction from the Marcellus shale, and co-ordinated it with the Genesee shale of New York. It is locally known as the *Huron Group.*

The geographical distribution of this formation in Michigan, will be seen by a reference to my Report for 1860. The strata along the eastern slope of the lower peninsula of Michigan dip westward beneath the Carboniferous rocks of the interior. This dip, however, is extremely slight, and the Huron group does not terminate at the waters of Lake Huron and the river St. Clair. It prolongs itself eastward, and overlaps the angle of Canada West, finally thinning out along a line extending from the mouth of Maitland river to a point on the Great Western Railway, a few miles east of Bothwell.

The lithological characters of the formation are but little varied. Throughout its entire thicknesss of 600 or 700 feet, it is argillaceous and generally bituminous. The most bituminous and darkest colored portions seem to be situated near the bottom of the group. We know, at least, that they are not near the upper part of the group. Other portions of the series consist of bluish, argillaceous shales and plastic clays, ranging in color from bluish to whitish. Numerous bands of sandstone, and of arenaceous magnesian limestone, from four to eighteen inches in thickness, occur irregularly in the middle and upper portions of the group. Bands of iron pyrites, of local extent, but sometimes from one to six inches through, are found imbedded in the shales and clays. The "black slate" proper, of the series, is freely combustible; and in some cases, where fire has been communicated by accident to outcropping masses, the combustion has survived for months. By distillation, this shale affords petroleum and all the related compounds. Its appearance at the surface is generally regarded as an indication of coal, and many serious disappointments have resulted from ignorance of the fact that the "black slate" is several hundred feet beneath the coal measures.

The bituminous shales of this group of rocks are generally considered to be the source of most of the native petroleum of the Northwestern States. It is the opinion of Dr. T. S. Hunt, of the Geological Commission of Canada, that the product issues from a bituminous limestone—the Corniferous limestone—next underlying these shales; but though this limestone may unite its exhalations with those of the Huron group, it seems more probable that the latter supplies the principal amount of the bituminous flow. It is also asserted that, at least in a few cases, petroleum is eliminated in quantities of commercial importance from the rocks embraced in the coal measures.

Wherever the oil-producing shales are exposed to the air, or covered by a porous medium, the product of the slow spontaneous distillation going on escapes into the atmosphere, and is lost. Where the shales are covered by an impervious layer, as of shale or plastic clay, the oil and gas elaborated are retained in the rocks, filling cavities by driving the water out by elastic pressure, and saturating porous strata embraced in the formation, or intervening between it and the impervious cover above. In the oil region of Ohio and Pennsylvania, an overlying sandstone becomes the reservoir in which these products are accumulated; and the oil wells of that region consequently terminate in that sandstone without penetrating to the original source of the oil. Throughout the oil-producing region of Canada and Michigan, this sandstone is wanting, but the destruction of the outcropping shales by geological agencies has created an immense deposit of argillaceous drift material, which forms an impervious cover for the petroleum beds. In Enniskillen the lower portion of this drift deposit is rendered somewhat porous by the presence of gravel, and this, in consequence, has become saturated with oil in the same manner as the overlying sandstone of Ohio and Pennsylvania. By boring into the rock, however, a lighter and purer oil is found surcharging the beds of sandstone and arenaceous limestone by which the group is intersected. It is found that the quality of the oil improves with the depth from which it is obtained. That drawn from the drift gravel lying upon the surface of the rock, is dark, thick, and impure, and commands but a moderate price for purposes of distillation. It is now principally employed for lubrication.

Though it is not yet actually demonstrated that rock oil exists in Michigan in quantities of economical importance, we possess every favorable indication which has ever been discovered in the prosecution of this new branch of industry. In the first place, the topography of the oil region of Michigan is identical with that of Enniskillen. The surface is level, rather low, and inclined to dampness. The soil is thin, argillaceous and indifferent for agricultural purposes. The forest growth, nevertheless, is dense and luxuriant, consisting of Beech, Hard Maple, Swamp Elm, and Black Ash, with occasional trees of White Ash, White Oak, Hickory, Sycamore, &c., interspersed. The water in the vicinity of the oil manifestations is of a dark color. Black river of Michigan seems to be the counterpart of Black creek of Enniskillen. It would seem evident that the color of the water in both cases must be due to some constituent of the crude oil; and yet a chemical examination of the water of Black river taken at Port Huron, did not sustain my expectations. It should be tested six or eight miles further north.

When we compare the more direct indications, the resemblance is equally complete. The oil region of Michigan abounds in "gum beds," or masses of inspissated oil, some of which have hardened to the character of asphalt. The oil is still issuing, and it shows itself on the surface of standing water everywhere throughout the Baker tract. Moreover, inflammable gas, the almost infallible criterion of an oil district, exists in very great abundance, and, in several instances, has been known to escape with noise and violence from the bottom of wells. In one instance the jet, when lighted, illuminated the country over a radius of four miles, and the sound was like that of steam escaping from a high-pressure engine. Many similar cases have occurred.

Whatever differences exist between the geological indications observed in the two regions are rather in degree than kind; and the advantages are undoubtedly on the side of the Michigan region. Canada West is located upon the thinning outskirts of the formation; Michigan embraces the great mass of it. And, in the abundance of gas emitted from the underlying rocks, the Enniskillen region bears a very unequal comparison.

It is quite possible that no porous stratum will be found in Michigan reposing upon the surface of the rock. Its occurrence is accidental anywhere. Should it be found wanting, and "surface oil" consequently wanting, this fact would not have the slightest bearing on the probability of oil in the rocks below.

I have stated that the Huron group, or Oil formation, extends continuously from the peninsula of Michigan into the peninsula of Canada West. To embrace both regions under one designation, they might be spoken of as the *Peninsula Oil Region.* It is not to be imagined that oil will certainly be obtained at all points within this region. It may be, but probably will not. Slight undulations are liable to exist in all strata so nearly horizontal as those of the two peninsulas. The volatile emanations from the strata would therefore accumulate under the highest arches. Possibly one of these arches runs nearly north and south through the Canadian region, from Bothwell to Petrolea, and beyond, while another underlies the district of petroleum manifestations on the Michigan side of the river. For the present, it would be safest to confine large expenditures to the immediate vicinity of this belt of indications.

In the transverse section across the strata, already referred to, an attempt has been made to show in what way the oil-bearing rocks may rise in two or more gentle undulations, thus inducing the oil and gas to accumulate along geological axes, and become restricted to belts of country of limited width.

4. *Account of some new or little known species of Fossils from rocks of the age of the Niagara Group.* From the 18th Report on the New York State Cabinet; printed in advance of the Report, and issued January 1865, by Prof. James Hall. 48 pp. 8vo, with many illustrations.—These pages contain descriptions of a number of new species of Cystideans, with figures of seven of them, and of several Crinideans, Trilobites, and various Mollusks. The following important remarks on the age of the Galt or Guelph limestone of Canada, and Leclaire beds of Iowa, and their relations to the New York Niagara group, form an introduction to the paper:

In the study of the fossils of the Niagara group and associated strata, previous to the publication of the final report on the Fourth Geological District,[1] and also to the publication of the second volume of the Paleontology of New York, I separated a few species found in the impure drab-colored limestone from Wayne county, New-York.

The limestone containing these fossils became exposed only in the bed of the canal during its excavation; and the low country, or deeply drift-covered surface in the vicinity did not admit of any exposure of the rock in place. The materials were thrown out of the excavation in connexion with the soft marls of the Onondaga-salt group, and the specimens of rock containing the fossils preserved the peculiar celluliferous structure and characteristic color of the argillaceous limestone of that formation. Differing so especially from any known beds in the Niagara group, I did not hesitate to refer them to the Onondaga-salt group, since there was no evidence of any other formation in the neighborhood.

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[1] The same reference of species had in fact been made during the Geological Survey; and they were thus published in the Report on the Fourth Geological District in 1843.

Throughout the State of New York, the country along the junction of the Niagara and Onondaga-salt formations is low and level, or covered by drift accumulations; and no opportunity offered of discovering any exposure of similar beds along the course of the outcrop. In some places in Monroe county, we have been able to trace the two formations to within a few feet of their contact with each other; but no fossiliferous beds, similar to those of Wayne county, have been found. At a later period,[2] my attention was called to some peculiar fossils collected at Galt, in Canada West; and in visiting the locality, I discovered some pieces identical with those before known, from beds which I had regarded as of the Onondaga-salt group in New York. As this limestone at Galt [and Guelph] was clearly above the great Niagara limestone of the Falls, and contained an almost entirely different set of fossils, I very naturally inferred that it belonged to the next higher formation, or the Onondaga-salt group; and that the Wayne county locality was a feeble representation of the limestone of Galt.[3] For these reasons the two were treated as identical, and referred to the age of the Onondaga-salt group; an opinion at that time sustained by the members of the Canadian Geological survey.

At a later period, during the Geological Survey of Iowa, I recognized, at the Leclaire rapids on the Mississippi river, a limestone holding the same relative position, having the same lithological character, and containing some identical and many similar fossils with the limestone of Galt or Guelph in Western Canada; and I thus announced its apparent relations in the Report on the Geology of Iowa.

"Should the identity of the limestone of these two distant localities be proved, it will afford sufficient ground for separating these beds from the Onondaga-salt group. and for establishing a distinct group. It seems quite probable that the limestones of this period have their eastern extremity in Central New York, where, from their small development, as well as from similarity of lithological character, there seemed no sufficient ground for separating them from the non-fossiliferous beds of the Onondaga-salt group.[4] Since, however, in Canada, these beds attain considerable importance, and (admitting the conclusions above given) acquire a still greater thickness and more distinctive character on the Mississippi river, it seems necessary to elevate them to the same rank as the other groups of the series." *Geology of Iowa*, vol. i, p. 75, 1857.

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[2] After a considerable part of vol. ii had been printed.

[3] The name *Galt* being considered objectionable, on account of a similar term already in use, and the same rock occurring also at Guelph, it has been called the "Guelph formation," in the nomenclature of the Geological Survey of Canada.

[4] My views regarding the presence of the Onondaga-salt group proper in Wisconsin and Iowa have somewhere been called in question, and I have only to remark in this place that I have seen no reasons on my own part, nor facts adduced on the part of others, to change my opinion in reference to the occurrence of this formation in the localities I have heretofore cited.

Some two or three years later, I explored the geology of the central and eastern portions of Wisconsin and the adjacent parts of Illinois. I here found the limestone of Racine, and a part of Waukesha and some other localities, resembling in all respects that of Leclaire, and holding many of the same fossils. It is likewise underlaid by the even-bedded darker-colored limestone, bearing *Halysites catenulatus*, *Pentamerus oblongus*, and many large Orthoceratites, which are everywhere regarded as evidence of the Niagara age. I could not hesitate, therefore, to parallelize the succeeding beds with the limestone of Leclaire, though we had failed to trace it across the country in a continuous outcrop. At the same time, on critical examination of the collection of fossils made at Racine and at some other points, I detected many species known as characteristic of the Niagara formation in the State of New York, requiring its recognition as a member of that group (rather than of the Onondaga-salt group), and uniting with it, as identical in position, the Leclaire limestone.[5]

At the same time, we have recognized, from Racine and adjacent localities, including Leclaire in Iowa and a single locality in Illinois, the following species which are identical with, or are very closely allied to, those from Galt in Canada West: *Pentamerus occidentalis*, an *Obolus*-like fossil, a *Favosites* and a species of *Amplexus* which are identical in several localities, *Cyclonema sulcata*, *Murchisonia Logani*, *Murchisonia* identical with or closely allied to *M. mylitta* (Billings), an undescribed *Murchisonia* from Racine identical with one from Galt, *Subulites ventricosa*, *Pleurotomaria solaroides?* *Loxonema longispira*, besides other forms which are closely allied to species of the Guelph limestone.

An examination of several localities in Wisconsin shows that this peculiar fossiliferous limestone is very unequally distributed. At Racine it has a very considerable thickness;[6] while in other places, either from denudation or other causes, it is very thin, or even absent. In some places in the vicinity of Milwaukee and Waukesha, there are indications of beds of passage from the regularly bedded limestones below to the unequally bedded rock above. There appears, indeed, very good evidence of the irregular or unequal accumulation of this higher rock in many of the localities along a considerable portion of the outcrop; and where the lower part of the formation comes to the surface, the upper rock does not appear to be developed. I am therefore induced to believe that this limestone at Racine, the mass at Leclaire and extending thence into Iowa, as well as the Guelph formation in Canada and the feeble representation of the same in New York, are really lenticular masses of greater or less extent, which have accumulated upon the unequal surface of the ocean bed in a shallow sea during the latter part of the Niagara period. These isolated masses of limestone have close relations with each other, while their relations with the Onondaga-salt group, though very intimate in the single locality in Central New York, become less and less conspicuous in a westerly direction.

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[5] Report on the Geology of Wisconsin, p. 67, 1861.

[6] I am inclined to believe that I have overestimated the thickness of the limestone at Leclaire from the presence of lines of false bedding, but I have had no opportunity for a re-examination of the locality.

5. *Observations on the Geology of Southern New Brunswick*, made principally during the summer of 1864 by Prof. L. W. Bailey, and Messrs. G. F. Matthew and C. F. Hartt, prepared and arranged, with a geological map, by L. W. Bailey, A.M., Prof. Chem. in the Univ. of New Brunswick, &c. Printed by order of the House of Assembly. 160 pp., 8vo. Fredericton, 1865.—In this Report, the distribution of the formations is given, with many details as to structure, fossils, geological relations, and economical products. The formations included are the Paleozoic, with the Azoic below these, and the Triassic Red Sandstone above, together with Post-tertiary beds. The Report contains a large colored geological map, showing the limits of the out-cropping rocks. The outcrops are nearly parallel with the coast, or about N.E. by E. in direction. The Azoic is in the region of Portland; and at St. Johns, between it and the Bay of Fundy, occur Primordial rocks, affording, according to determinations by Mr. Hartt, *Paradoxides* and other characteristic fossils, besides some Brachiopods. Mr. Hartt observes upon these fossils:—

"Representatives of four genera of Trilobites have been obtained thus far from the Saint John rocks, viz:—*Paradoxides*, *Conocephalites*, *Agnostus*, and *a new genus?* allied to *Conocephalites*.

The number of species in each genus has not yet been satisfactorily made out; but of *Paradoxides* there are at least five, of *Conocephalites* seven, and of *Agnostus* and the *new genus* each one.

All the species appear to be new. One of the *Paradoxides* bears a close resemblance to *P. rugulosus*, Corda, from the *Etage* C of Barrande, in Bohemia, and one of the *Conocephalites* is allied to *C. coronatus*, Barrande, from the same fauna and horizon, though neither is identical with the European species.

There are six species of Brachiopoda, belonging to the genera *Orthisina*, *Discina*, *Obolella*, and *Lingula*. I have not been able to identify any of the forms with described species.

Though all the species from the Saint John group are apparently new, yet the occurrence of *Paradoxides* and *Conocephalites*, genera confined entirely to the so-called *Primordial fauna* of Barrande, and everywhere characteristic of it, together with the strong likeness borne by the Saint John species, in their facies, to those of the same genera of the faunæ of the "Primordial" in Europe and America, enable us unhesitatingly to assign to the Saint John group, or at least to that lower part of it which has afforded Trilobites, a geological position equivalent to Barrande's Etage C, or to the Potsdam proper of America.

The lower part of the Saint John Group, at Coldbrook, has been divided by Mr. Matthew, on lithological grounds, into three Bands, viz:

No. 1. The lower or arenaceous band, with no determinable fossils, and constituting passage beds from the Coldbrook Group.

No. 2. Argillaceous shales, rich in fossils, *Paradoxides*, *Orthisina?*, *Conocephalites*, *Obolella*.

No. 3. Carbonaceous shales, full of fossils, *Paradoxides*, *Conocephalites*, *Orthisina*, *Discina*, &c., all much distorted."

Prof. Bailey, speaking of the *Albertite*, states that in his opinion and that of Mr. Matthew, "it is neither coal nor jet, but an oxydized oil, derived from the decomposition of fish remains, and subsequently changed by chemical action," giving as reasons the position and constitution of the material, the nature of the adjoining rocks, and the fact that "springs containing oil are not uncommon throughout the district in which the Albertite is found."

6. *On Devonian Insects from New Brunswick;* by S. H. SCUDDER. (From a communication addressed by Mr. Scudder to Dr. C. F. Hartt, of New Brunswick, dated, Boston Society of Natural History, January 11, 1865.)—I have made as careful an examination as my present circumstances will permit of your most interesting collection of the fossil remains of insect-wings from Lancaster. There are ten specimens in all, eight of which are reverses of one another, thus reducing the number to six individuals; of these, one, a mere fragment, belongs, I think, to the same species as another of which the more important parts of the wing are preserved, so that we have five species represented among these Devonian insects, and these remains are all, I suspect, composed of portions of the anterior wing alone. The data being thus fragmentary, the conclusions cannot be quite so satisfactorily determined as we could wish, but we can still discover enough to prove that they are of unwonted interest. Besides the peculiar interest which attaches to them as the *earliest known traces of insect life on the globe*, there is very much in themselves to attract and merit our closest attention.

One of them is a gigantic representative of the family of *Ephemerina* among Neuroptera, some three or four times the size of the largest species now living with which I am acquainted.

Another borrows some striking points of the peculiar wing-structure of the Neuropterous family *Odonata*, and combines with them those of families remote from that, and even belonging to a distinct section of the Neuroptera, exhibiting to our view a synthetic type which combines in one the Pseudoneuroptera and the Neuroptera, and represents a family distinct from any hitherto known.

Other fossil insects, found in Carboniferous concretions in Illinois, and described in this Journal ([2], xxxvii, 34), which Professor Dana has kindly allowed me to evamine,[1] also belong to hitherto unrecognized families, exhibiting similar relations to sections of Neuropterous insects in our day disconnected; and your third species is a member of still another family of Neuroptera, which finds its natural relations between the two described by Professor Dana.

A fourth, of which only an unimportant fragment was found, would seem to belong to the Neuroptera; but by some peculiarities of the minuter cross-veins, thrown off in the middle of the outer edge of the wing in a most irregular and unusual manner, suggests no intimate relations with any known family, but must have belonged to a group of large and weak-winged insects.

The fifth and last to be mentioned is of very striking interest, because, while it exhibits the peculiar venation which forms the well known tympanum or stridulating apparatus of the male in the Orthopterous family *Locustariæ* (though differing somewhat from that), it also most resembles the Neuroptera in all, or nearly all, the other peculiarities of its structure, and suggests the presence in the insect-fauna of those ancient times of a synthetic type, which united the characteristics of the Orthoptera and Neuroptera, in themselves closely allied; this point however requires patient and severe investigation, and only my earliest impressions are here recorded, made, however, immediately after a close examination into the relations of other fossil insects.

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[1] The results of this examination will shortly be communicated to this Journal.

7. *Note on the Azoic age and metamorphic origin of the Iron Ore of Mexico, described by N. S. Manross, at page* 309 *of this volume;* by J. D. Dana.—The great thickness and extent of the magnetic and specular iron masses of Guerrero leave little doubt that the rocks belong, like those of Northern New York, Michigan and Canada, to Azoic or præsilurian time, and thus they indicate the existence of an Azoic area in this part of the continent. The metamorphic nature of the iron ore is proved by the alternation of the beds with conformable layers or strata of granular or metamorphic limestone, and syenite or granite. In a section of the Chutla "iron mountain," figured by Mr. Manross, the great iron ore bed is represented as lying beneath "beds of limestone," and over granite. In another, of the iron mountain of Las Anonas, the largest bed of iron ore is divided along the middle by a bed of limestone, and on either side there are alternate beds of iron ore and syenite with epidote. In another, the Sochipala region, the iron bed has beds of granular limestone above it; and below, syenite, then another iron ore bed, 30 feet thick, and then syenite again. Thus the evidence is the same in kind as in the Azoic region of the north.

8. *Report on the Geology of Illinois;* by A. H. Worthen.—The Legislature of Illinois has recently appropriated $20,000 for the publication of 3000 copies of the Geological Report of Illinois, made by Mr. Worthen under the authority of the State. The plates will be numerous, and, as we know from an examination of the figures, excellent. In working up the Paleontology, Mr. Worthen has had the assistance of Mr. F. B. Meek for Zoology, and L. Lesquereux for Botany. The Report will yield to none hitherto published in the country in the value of its contributions to science. The State is especially rich in fossil shells, fishes and crustaceans of the Carboniferous age, and affords many new species of coal plants.

9. *Check List of the Invertebrate Fossils of North America: Miocene;* by F. B. Meek. Smithsonian Miscellaneous Collections. 32 pp. 8vo. Washington, Nov. 1864.—This list of Invertebrate Miocene Fossils of North America is complete to the time of publication, both as regards paleontological discovery, and zoological science. Critical remarks are appended to the list.

10. *Ichthyosaurian Skin.*—A specimen of the *Ichthyosaurus tenuirostris*, recently obtained at Barrow-on-Soar, shows a large extension of the dermal covering upon the surface of the slab, seeming to indicate that the animal had a prominent ridge along the dorsal surface similar in appearance to that which the males of the pond-newt, *Triton cristatus*, present in spring.—*Reader, Dec.* 3.

11. *Murchison on the Drift.*—The last annual address of the President of the Royal Geographical Society, Sir R. I. Murchison, contains a discussion at length of the question of the European drift, in which the author sustains the Iceberg theory. Only the want of space prevents our citing his views in this Journal.

12. *Annual Report of the Geological Survey of New Jersey, for the year* 1864, by Prof. Geo. H. Cook, State Geologist. 24 pp. 8vo. Trenton, N. J., 1865.—Prof. Cook presents, in his Report, a brief statement of the progress of his survey, dwelling mostly on economical results. A colored map of the formations is included.

III. BOTANY AND ZOOLOGY.

1. DeCandolle: *Prodromus Syst. Nat. Regni Vegetabilis.* Pars 16, Sect. posterior, Fasc. I, *sistens Cupuliferas*, etc. Nov., 1864. pp. 160.—In pursuance of the plan adopted for bringing out the concluding volumes of the Prodromus in parts, as soon as the respective orders are elaborated, we have here the *Cupuliferæ*, upon which Mr. DeCandolle himself has bestowed so much labor,—the leading genus, *Quercus*, extending to 281 species, *Castanopsis* (to which belongs *C. chrysophylla* of California), 14 species, *Castanea*, only two well determined species, and *Fagus*, 15; the *Corylaceæ*, admitted as an order, after Hartig and Doll; also the *Juglandaceæ*, the earliest botanical work of Casimir DeCandolle, whose separate memoir upon the subject we have already noticed with approbation; the *Myricaceæ* by the same promising hand (38 species of *Myrica*, including *Comptonia*, and Chapman's *Leitneria* of Florida); the *Platanaceæ*, and, in a note, *Liquidambar*, by Alphonse DeCandolle. The *Betulaceæ* and *Salicineæ* will follow in this volume, we believe at no distant date. A. G.

2. J. D. Hooker: *Handbook of the New Zealand Flora.* Part I. London: Lovell Reeve & Co. 1864. pp. lxxviii, 392, 8vo.—This is a new member of the series of British Colonial Floras. The present part includes all the known Phænogamous plants and Ferns, not only of New Zealand, but of the outlying islands which may be regarded as of the New Zealand group, such as the Chatham and Kermadec islands on the one hand, and the Lord Auckland and Campbell's islands on the other. The Cryptogamous plants are to follow. The basis of the present work, of course, is Dr. Hooker's Flora of New Zealand and the Flora of Lord Auckland and Campbell's islands, making three large illustrated volumes of the Botany of the Antarctic Voyage under Capt. Ross,—volumes much too bulky and costly to subserve the main purpose of the Handbook, i. e., to be freely used by the colonists themselves. Moreover, large discoveries have been made since the Flora Novæ-Zelandiæ appeared, mainly in the middle island, adding fully one-third to the number of flowering plants previously known. The genera are here brought up to 303, the species to 935,—still exemplifying the insular paucity. Of these species 677 are peculiar to these islands; 222 are Australian, and 111 American. There are, besides, 51 Australian representative species, and 32 American representative species. Among the American identical species are *Myosurus aristatus*, of the Pacific United States and Chili, and our *Elatine Americana*, which has a wide range in the southern hemisphere, viz: in S. America, New Zealand, Feejee Islands, Australia, and Tasmania. Dr. Hooker remarks that few if any plants appear to have been collected in the Lord Auckland's Group by Commodore Wilkes's expedition. A single officer, Lieut. Holmes we think, passed a few hours on land, collecting, however, among other plants, a single specimen of an apparently new *Ranunculus* (*R. Aucklandicus*), which has been accidentally overlooked in the present work. A. G.

3. MARTIUS: *Flora Brasiliensis;* fasc. 36, 37, 38. fol. Dec. 1864.—The *Gesneraceæ*, by Dr. Hanstein, the curator (succeeding the late Dr. Klotszch) of the Berlin Herbarium; with eleven plates. The *Salsolaceæ* (*Chenopodiaceæ*), by Frof. Fenzl, of small importance in Brazil; with five plates, one of them illustrating *Chenopodium anthelminticum*, and one *Roubieva multifida*. *Magnoliaceæ*, *Winteraceæ* (retained, as likewise *Schizandreæ*, as ordinally distinct), *Ranunculaceæ*, *Menispermaceæ*, and *Berberideæ*, by the acute Dr. Eichler; twenty-six plates, one of them devoted to the anatomy of the wood of *Drimys Winteri*, and two to that of Menispermaceous stems, which the author appears to have discussed with much ability. A. G.

4. *The Journal of the Linnæan Society*, No. 31 (Dec. 1864), is especially rich in articles upon Dimorphism and even Trimorphism in plants, and upon the agency of insects in their fertilization. There is 1. *Notes on the Fecundation of Orchids, and their Morphology*, by the late Dr. Cruger, Director of the Botanical Garden, Trinidad; *Catasetum* and *Stankopea* being the principal subjects, and the conclusions of Mr. Darwin being fully confirmed. 2. *Dimorphism in the Flowers of Monochoria vaginalis*, by Dr. Kirk. The additional kind of flower would seem to be somewhat after the fashion of that of *Utricularia clandestina*, and arranged for self-fertilization. 3. *On the Individual Sterility and Cross-Impregnation of certain species of Oncidium*, by Mr. John Scott. He shows by experiment "that the male element of *O. microchilum* will fertilize the female element of the two distinct species, *O. ornithorhynchum* and *O. divaricatum cupreum*, and yet be completely impotent upon its own female element; nevertheless, the susceptibility of the latter (female element) to fertilization is shown by its fertile unions with another individual of the same species, and likewise by a fertile union with an individual of a distinct species;" and the same is true of *O. microchilum*. 4. *Notes on the Sterility and Hybridization of certain species of Passiflora, Disemma, and Tacsonia*, by the same author. Species which, in cultivation, are perfectly sterile upon the application of the pollen to the pistil of the same individual, are readily fertilizable by the pollen of other individuals of the same or of an allied species, and their pollen is likewise potent upon such individuals; although in hybridization the influence frequently is not reciprocal. For instance, *Passiflora racemosa* may be fertilized by the pollen of *Tacsonia mollissima*, upon the ovules of which, conversely, the pollen of the *Passiflora* is utterly impotent. There are two Passionflowers, totally impotent when self fertilization is attempted, the pollen of one of which effects the development of the ovaries of the other, but never the seeds, while conversely even the ovary fails to develop. And there are two species of *Tacsonia*, the pollen of one of which causes the ovary and even the seed-coats of the other to develop, but never the embryo, while conversely the effect is sometimes the same, but generally nothing at all. Although general conclusions should be hesitatingly drawn from limited experiments upon cultivated plants, yet the known facts conspire to show that no sharp line is drawn in nature between fertility and sterility in crosses. 5. *On the Sexual Relations of the three forms of Lythrum Salicaria*, by Charles Darwin. Here we have the results of an investigation which Mr. Darwin has before

referred to. A curious case it is, and treated with the wonted sagacity and point of this prince of biological inquirers.

"In *Lythrum Salicaria* three plainly different forms occur: each of these is an hermaphrodite; each is distinct in its female organs from the other two forms; and each is furnished with two sets of stamens or males, differing from each other in appearance and function. Altogether, there are three females and three sets of males, all as distinct from each other as if they belonged to different species; and, if smaller functional differences are considered, there are five distinct sets of males. Two of the three hermaphrodites must co-exist, and the pollen be carried by insects reciprocally from one to the other, in order that either of the two should be fully fertile: but, unless all three forms co-exist, there will be waste of two sets of stamens, and the organization of the species as a whole will be imperfect. On the other hand, when all three hermaphrodites co-exist, and the pollen is carried from the one to the other, the scheme is perfect; there is no waste of pollen and no false co-adaptation. In short, Nature has ordained a most complex marriage-arrangement, namely, a triple union between three hermaphrodites,—each hermaphrodite being in its female organ quite distinct from the other two hermaphrodites, and partially distinct in its male organs, and each furnished with two sets of males."

One must study this instructive paper to see how neatly it is shown, "that only the longest stamens fully fertilize the longest pistil, the middle stamens the middle pistil, and the shortest stamens the shortest pistil. And now we can comprehend the meaning of the almost exact correspondence in length between the pistil of each form and the two half-dozen sets of stamens borne by the two other forms; for the stigma of each form is thus rubbed against the same spot of the insect's body which becomes most charged with the proper pollen." For the use which Mr. Darwin makes of this case, and the theoretical deductions drawn from a genus which presents trimorphic, dimorphic, and monomorphic species, the illustration of the advantage of trimorphism, and of the now established fact that sexual differences,—"thought to be the very touchstone of specific distinction,"—may characterise and keep separate the coexisting individuals of the same species in the same manner as they do those groups of individuals which we denominate species, we must refer to the memoir itself, not having space for a full abstract. Mr. Darwin, on raising from seed some individuals of our *Nesæa verticillata*, ascertained that this plant is also trimorphic. We commend it to the particular attention of any who may be disposed to prosecute farther such investigations, which, though requiring genius to originate, are easy to follow up, and almost inexhaustible in interest.

Of the remaining papers in this number of the Linnæan Journal, one is *On a Peloria and Semi-double Flower of Ophrys arenifera*, by Dr. Masters, to which a note is added, mentioning a nearly similar monstrosity of *Pogonia ophioglossoides*, (known to him from a description by Prof. Gray), collected by Rev. J. A. Paine, in a bog near Utica, N. Y. Several flowers were detected, last year, all sharing more or less of the peculiarity, viz: having three *labella* and the column resolved into small petaloid organs; the two accessory labella and a small petaloid body on the other side of the flower answering to the three suppressed stamens of the outer series, while two little filaments answer in position to the suppressed lateral stamens of the inner series. It is hoped that other specimens may be detected the ensuing summer, and preserved in spirit for more searching examination. In another paper, Dr. Hooker identifies *Pinus Peuce*, Griesb., of the mountains of Macedonia, with *P. excelsa* of the Himalayas. Mr. Mitten describes new *Musci* and *Hepaticæ* of Japan and the coast of China, and Prof. Oliver, new genera of plants of Western Tropical Africa.

The longest paper in No. 32 (February, 1865) is one by Dr. Masters, *On the Morphology and Anatomy of the Genus Restio*, Linn., *together with an Enumeration of the South African Species*. There is also an extract from a letter of Prof. Brewer to Sir Wm. Hooker, *On the Forests of Sequoia (Wellingtonia) gigantea of California*, correcting the popular impression that this magnificent tree is extremely local and in danger of extinction. A. G.

5. *Further remarks on larve budding;* by Dr. W. C. Minor.—In a preceding number of this Journal (January, 1865) some notice was taken of a newly observed form of insect larve budding. The first number of the *Zeits. f. wiss. Zoölogie* for this year has a further account by Dr. Wagner, the original observer, which, beyond the interest of a detailed reply to questions from Dr. Siebold, contains some generalizations upon the phenomena of budding, which we give in brief. Dr. Wagner offers the following table of the grades between the simplest asexual and the most perfect sexual multiplication in the articulates.

I. Asexual spontaneous multiplication of the larve-nurse (Amme), with sexual generation of the developed animal. The germ is metamorphosed out of the fat or granular substance of the larve-nurse, and the animal has three or four transformations, in the forms of protoscolex, deuteroscolex, strobila and proglottis.—*Cestodes* and *Trematodes*.

II. Larves with sexual organs. The germs developed in these organs become individuals within the body of the larve-nurse, and are born alive, forming thus two metamorphoses.—*Aphides*.

III. Multiplication only in the perfect sexual animal:—*a*. In both males and females, but without sexual influence.—*Daphnidæ*. *b*. In one sex only, without sexual influence.—*Bees*, and some *Butterflies*. *c*. In one sex only, under the influence of fructification.

The author admits that nothing more than one point of view is attempted here, as deeper embryological studies will, by bringing out homologies and complications of structure, probably separate the higher and lower groups here put together. The case he has discovered in the Diptera seems to him to form a transition from the first to the second group. He refers to Leydig's comparison of the Aphis buds with the summer eggs of Daphnia, and of the true Aphis eggs with the winter eggs or ephippia of Daphnia, and extends the comparison to the larve buds and eggs here.

Upon the distinction between Parthenogenesis and Alternate genesis, he urges, that the former is a germination of buds in *special sexual organs*, though without fructification; while the latter is a *self-transformation*, also unfructified, of tissue into germs or buds, without any special organ for the transformation, and in a degree independent of the nurse, and hence spoken of as *spontaneous*. The first of the groups given above comes under the head of alternate genesis—Generationsweckse1 —while the second, and in part the third, are instances of parthenogenesis. From this point of view, also, is the present case intermediate between the first and second groups.

He calls attention to the compensation, by the budding of the larve, for the limited egg-bearing capacity of the mature fly, and regards this compensational balance as usual in these cases. Also, considering the fat or granular substance of the larve-nurse to be directly transformed into these larves—a view confirmed, it will be remembered, by Dr. Meinert—he points to the hypertrophied eggs as a store of material for its production. That this granular substance is of a nutritive nature, is made altogether probable by its transference into the stomach through the Malphigian vessel, in cases of prolonged abstinence, and also by the presence of sugar in it; and he observes that the imperfect tracheal system and sluggish movements, for a time at least, of the larve-nurse tend to leave this store intact for the sole use of the buds.

Both in this, and the first paper of Dr. Wagner's, which we have now received, are numerous points of anatomical and physiological interest; such as the condensation in the mature fly of the separate ganglia of the larve, the valves at the posterior end of the dorsal heart—also figured by Dr. Pagenstecher—and which, as well as the glands along its side, supposed from analogy—in leeches—to form the blood corpuscles, we have ourselves seen.

It is perhaps not premature to state here that the writer has found a number of large oval germs in some minute larves observed lately, and he would urge the attention of American observers, who may be better situated than himself, to this inquiry; since, to judge from the difference in shape of the larve's head, these were not of the same genus.

6. *On the Hymenoptera of Cuba*, by E. T. Cresson. (From the Proceedings of the Entomological Society, Philadelphia, Jan. 1865. 200 pp. 8vo.)—The Entomological Society of Philadelphia has shown great activity since its formation, having issued many long and thoroughly labored memoirs. Mr. Cresson's work is based principally on the collection of Prof. F. Poey of Havana, purchased by Dr. T. B. Wilson of Philadelphia and presented to the Society, and also on specimens in the collection of Dr. J. Gundlach of Cuba. Most of the Chalcididæ, Prototrupidæ and Formicidæ are reserved for future papers.

7. *Synopsis of the Bombycidæ of the United States*, by A. S. Packard, Jr. Part II. 66 pp. 8vo. From the Proceedings of the Entomological Society, Philadelphia. Nov. 1864.

IV. ASTRONOMY AND METEOROLOGY.

1. *Correction of D. Trowbridge's article on the Nebular Hypothesis;* by the Author.—In my article on the Nebular Hypothesis, Art. xlvi, and note 41 (this Jour., [2], xxxix, 31, 32), I have committed an error in my reference to Prof. Ferrel's article on the retarding effect of the sun and moon on the earth's rotation, in Gould's Astronomical Journal. One part was owing to a careless misconception, and the other to a

misprint in his article. The retarding effect is given in his article in angular velocity, and not in absolute velocity as I conceived (though it was so given that one might easily make the mistake). The effect of the earth on the moon is 5625 times as great, and not 562½ times, as printed. My general reasoning, however, as applied to the nebular hypothesis, is not affected by the error which I made. My error was in numbers (I read Prof. F.'s article several years ago when it was published) and not in principles.

Hector, N. Y., March 23d, 1865.

2. *Cambridge Observatory in* 1864. Extract from the Annual Report of the Observatory Committee to the Board of Overseers of Harvard College, (made previous to the decease of the Director, G. P. Bond).—The labors of the year have been chiefly directed to the continuation of the zone observations, the examination of nebulæ, observations on the asteroids, on changeable stars, and on the two telescopic comets of the year. During the last winter and the early part of the spring, observations of minute inquiry were made on Orion on fifty-nine nights, resulting in the discovery of many new features, as well as corroborating characteristics heretofore published. Among the discoveries were six variable stars in the neighborhood of the Trapezium.

In the prosecution of the zone observations, Mr. Safford obtained the position of four thousand seven hundred stars by the great equatorial, besides performing the necessary reductions and computations involved. Very few persons, who have not had the privilege of examining the great mass of manuscripts already accumulated, can form any conception of the enormous labor required in this valuable undertaking.

The director repeats his great obligations to Mr. Safford for his devotedness to all the interests of the Observatory, and the skill and fidelity with which he has executed the several parts assigned him.

3. *Extracts from the Address of* W. De La Rue, *President of the Astronomical Society of London, connected with the awarding of the Society's medal to* Mr. G. P. Bond.—The announcement that the Royal Astronomical Society has awarded its gold medal to Professor Bond will be hailed with universal satisfaction by those interested in the science which he has so ably cultivated. At the anniversary meeting of the Society, held on Friday, Mr. Warren De La Rue, under whose auspices the Society has taken such a large stride in its path of usefulness, according to custom, accompanied the announcement by an address, which should add greatly to the pleasure of the medallist, so warmly does it appreciate the work done by the transatlantic astronomer, who has for so long had the finest telescope in the world to use, and has used it so well. The address is too long to be reproduced *in extenso*, but we give several extracts from it, reserving our report of the other business done at the meeting for a future occasion.

Mr. De La Rue commenced by observing, that 'when father and son have worked zealously together in the same direction, and especially in those cases where the son has succeeded to the position created and held for a number of years by the father, it is extremely difficult to draw any line of demarcation, from which to date the commencement of the son's independent career, or the cessation of the momentum imparted to his course of activity by the father. This is especially the case with respect to the late Professor W. C. Bond and his son, Professor George Phillips Bond, to whom the council have awarded the highest mark of distinction this society can confer.

In the estimate formed of his scientific work, the professional astronomer is generally placed somewhat at a disadvantage in comparison with the amateur. In the case of the latter, the whole of his work is weighed in the balance, while, in that of the former, large deductions are made from his labors as belonging properly to the duties of his official position. An official astronomer, in this age of zealous activity and speedy publication, inaugurated I believe by the example of the present Director of our own Royal Observatory, may, consequently, produce much good and original work without his name coming with due prominence before his peers, so long as that work falls, or rather appears to fall, within the range of his official duties. . , .

Unquestionable evidence that Prof. G. P. Bond has done more for our science than even a scrupulous discharge of his duty demanded, was given by the appearance of the 'Annals of the Astronomical Observatory of Harvard College,' vol. iii, 1862. At first sight, indeed, that volume might appear to be nothing beyond a record of excellent but, nevertheless, official work; when, however, I shall presently come to speak of its details, it will, I doubt not, be conceded that it belongs in a great measure to the category of private labor independent of official duty.'

After stating that Prof. G. P. Bond had been a most active contributor to astronomical discovery and methods for a long series of years, and that, besides his great and indeed unique work on the comet of 1858, an amount of good work had been accomplished by him which fully entitled him to the distinction now conferred upon him, Mr. De La Rue remarked that 'his earlier exertions appear to have been directed to the discovery of comets and the computation of their orbits, and in the Report of the Committee of Harvard College he is mentioned as having detected independently eleven of those bodies. It is not surprising, therefore, that so zealous an observer of these strange visitors, about the nature of which so little is known, should have been one of the most assiduous observers of so splendid a comet as that of Donati. It was also a natural consequence that Professor Bond should have desired to compare his own results with those of other observers, but it was by no means a necessary sequence that he should have entailed upon himself the enormous labor of collecting, systematically arranging, and reducing, the whole of the data resulting from this comparison.

The circumstances attending the apparition of the comet of 1858 were peculiarly favorable; its early discovery by Dr. Donati, while yet a faint telescopic object, and the prediction of its great brilliancy and splendor, having had the effect of directing the attention of a great number of astronomers to this remarkable body. Hence, in the history of cometary observations, there has never been collected so large a mass of observations as in the case of Donati's comet, the absence of moonlight, the short duration of twilight, and the remarkable continuance of clear weather during the most important part of the comet's apparition, largely contributing to this result. The darkness of the sky, which served as a background to the comet, was peculiarly favorable to the delineation of the fainter outlines and peculiar features of this splendid visitor, which was visible to the naked eye from August 19 to December 9, one hundred and twelve days. The whole period of visibility in the telescope extended from June 2, 1858 to March 4, 1859, an interval of two hundred and seventy-five days. One of the most successful workers in recording the phenomena of this comet was Professor G. P. Bond, and the sketches and drawings made at the Observatory of Harvard College form the main contribution to the splendid graphic illustrations which are a remarkable feature of this altogether remarkable production. The pains taken to insure a truthful representation of this comet of 1858, both in its eye- and telescope-features, few are better able than myself to appreciate; and I am able, from my own practical experience, to state that the success which has attended these efforts has been deservedly won by battling with greater difficulties than would probably be imagined by the uninitiated.'

'The comprehensiveness of Professor Bond's work will be at once recognized by an enumeration of the various sections into which it is divided. They are as follows:—

I. Figure and Position of the Tail; II. Observations upon the Secondary Tails; III. Reduction of Observations upon the Figure and Position of the Tail; IV. Probable Errors of Observations of the Tail; V. On the Deflection of the Tail; VI. Columnar Structure of the Tail; VII. Reduction of Observations on the Secondary Tails; VIII. The Nucleus and Envelopes; IX. On the Outline of the Head of the Comet; X. On the Branches and Central Darkness of the Tail; XI. The Nucleus; XII. The Envelopes; XIII. The outer faint Veil; XIV. On the Direction and Initial Axis of the Tail; XV. Summary of the Contents of the Volume.'

'The sections I to VII relate to the figure, dimensions, and positions of the tail, from its first appearance on August 14, 1858, seventy-three days after its first discovery, when it was seen at Copenhagen by D'Arrest and at Vienna by Hornstein, until the last recorded observation at Santiago, in Chili, by Moesta, on February 7, 1859. Arranged alphabetically under the same date, are the names of the several observers, sixty-seven in number, whose statements are given verbatim in the language in which they were written; but the value of this record is greatly enhanced by the occasional remarks of the author, who draws the reader's attention to points of special interest, and thus brings under notice the changes which actually occurred, as well as those which did not take place although anticipated from previous hypotheses. The reader is informed, in the introductory chapter, that from June 2 to September 8, the earth was on the north side of the plane of the orbit, and on the latter day crossed the line of nodes, giving an opportunity for observations on the figure of the tail projected on a plane at right angles with the comet's orbit. After the middle of September the tail was presented in nearly its full-length proportions; within a day or two of the perihelion passage on September 30, the axis of the tail was brought to a position at right angles to the line of vision, and, ten or twelve days later, when the comet had reached its least distance from the earth, its profile was almost precisely that of a section in the plane of the orbit. Professor Bond remarks, that where so little is known, *à priori*, respecting the actual figure, there is an obvious advantage to be derived from these accidental circumstances of its position, by which the influence of perspective foreshortening is, in a great measure, eliminated.'

'In order to recognize and explain any errors or ambiguities in the observations themselves, a provisional chart was constructed, showing the path of the comet and the position of the tail among the neighboring constellations. This chart naturally gave confidence and insured accuracy in the subsequent discussion of the observations, which are duly 'weighted' according to the dependence which can be placed upon them.'

'Lastly, the normal outlines of the tails are given in a series of charts which represent the final results deduced from the whole series of observations between September 16th and October 17th, referred to the common epoch for each date of 7^h mean solar time at the Observatory of Harvard College. The figure of the tail is preserved free from any considerable distortion by the method adopted; namely, by projection on a tangent cylinder, the position of which, in regard to the path of the comet, was selected with that view.'

After having given an idea of the comprehensiveness of the first seven sections of Professor Bond's work on the comet of 1858, the speaker referred to those parts which treat of the telescopic observations of the nucleus and envelopes—which are characterized by the same care, skill, and resource, though the difficulties of dealing with the discrepancies must have been enormously greater.

'Professor Bond gives in his work a plate representing the drawings or engravings of the nucleus and envelopes in the form they reached him. A cursory inspection of this collection will suffice to show how great is the diversity of portraiture of the same object, even when made at about the same period of absolute time.'

'The form of the head of the comet received considerable attention; the result was such as to show that its outline did not accord with a parabola, but that its contour was nearly that of a catenary curve. Moreover, advantage was taken by Professor Bond, of the apparition of Comet III, 1860, and more recently, of the great Comet of 1861, to test some of the hypotheses discussed in reference to the Comet of 1858, notably the phenomena of the successive throwing off of a series of envelopes from the nucleus, and the diminution of their elevation velocities as they receded from it. A recent careful revision of these phenomena has, according to the report of Professor Bond for 1864, completely confirmed the results previously announced.

Before dismissing the consideration of the volume on the comet of 1858, Mr. De La Rue added a few words in honor of Mr. Josiah Quincy, of Boston, who defrayed the expenses of the letter-press of the volume, and of the twenty-four gentlemen of Boston, Waterton, Cambridge, and Salem, who subscribed the funds necessary to cover the cost of the unrivalled engravings with which it is illustrated. It is most creditable to these gentlemen that they so fully appreciated the importance of doing ample justice to the labors of the Director of the Observatory of Harvard College, and enabled him to lay before the public such a written and graphic record as could not fail to attract the attention and obtain the confidence of the astronomical world. It should be remembered that the Observatory in question is not an institution endowed with ample funds to meet all its requirements, but that, on the contrary, appeals for aid to patriotic citizens have frequently been necessary, and have always been generously responded to whenever any object of scientific importance could be thus advanced.'

Passing from this splendid addition to our knowledge of the physical phenomena of comets, Mr. De La Rue next referred to a very important step made by Professor Bond in connection with the theory of planetary perturbation. 'It is well known that when the excentricity and inclination of the orbit of the disturbed planet are considerable, the problem of three bodies represents difficulties which have baffled the efforts of the most consummate mathematicians since the time of Newton. Fortunately, when the question relates to the computation of the perturbations of the principal bodies of the planetary system, such difficulties are not found to exist.' 'But for comets, which revolve in orbits of every degree of excentricity, and inclined to the ecliptic at angles of all degrees of magnitude, a special method is necessary. The chief merit of improving and applying this method of computation must, in all fairness, be assigned to Professor Encke. Nevertheless, Encke and other German astronomers were to a very great extent anticipated by Professor G. P. Bond, whose suggestions were made so early as the month of May, 1849, in a paper published in vol. iv. of the 'Memoirs of the American Academy of Arts and Sciences,' whereas Encke's labors bear date from 1851.' 'It is but right, however, to add that at the time when Professor Encke communicated his researches to the Berlin Academy he was totally unaware of Professor Bond's previously published paper on the same subject.'

Mr. De La Rue next adverted to the magnificent monographs of Saturn in 'The Annals of the Astronomical Observatory of Harvard College,' vol. ii, part i. 'Although the chief merit of that elaborate treatise is due to Prof. W. C. Bond, a reference to the initials of the various observers brings out prominently the great part taken by his son, G. P. B. being affixed to 'two very important series of observations connected with the physical history of that interesting planet, namely, in the first place those which in 1848 led to his discovery of the Eighth Satellite, Hyperion, independently of and simultaneously with Mr. Lassell; and, in the second place, observations of a peculiar luminous appearance within the then so-called inner ring, associated with a dusky belt crossing the planet in such a position that it could not be mistaken for the shadow of the bright rings on the ball, which shadow was seen at the same time in its proper place. I need scarcely say that it was the dusky ring of Saturn that was then for the first time noticed, though it was only subsequently recognized as such when, on Nov. 15, 1850, Mr. Tuttle, of the Harvard Observatory, suggested that the existenee of a new ring of a dusky character would explain the phenomena. Mr. Dawes subsequently, namely, on the 25th and 29th of the same month, discovered the same ring independently, with a refractor by Merz, of 6½ inches aperture.' The address concluded as follows:—

'I believe I am right in stating that the only orbit of Hyperion yet published was computed by Professor G. P. Bond, from his own observations. He has also published an analytical investigation of the question of the stability of Saturn's rings, in which he arrived at the unexpected conclusion that they are in a fluid state.[1]

Among other titles to our esteem for him as an astronomer must be mentioned his share in the adaptation of the great Refractor of Harvard College to the observations of very small stars in zones of declination, the first-fruits of the plan being a catalogue of 5,500 stars near the Equator, forming vol. i. part ii. of the above named "Annals," the observations having been made by himself and Mr. Tuttle.

This plan, it will be remembered, consists partly in recording, by the electric method, the times of transit of stars as they cross certain vertical lines drawn on a piece of transparent mica attached to the diaphragm of the spider-line micrometer. The electric recording apparatus is that originally contrived by the late W. C. Bond and his two sons, in which the movement of the recording barrel is regulated by their well-known spring-governor. On the piece of transparent mica exhibiting the transit lines, is also drawn a series of horizontal lines, 10″ of declination apart, grouped into threes and sixes, to indicate half minutes and minutes, the former denoted by making every third line somewhat longer than the adjoining ones, and the latter by causing every sixth line to extend quite across the slip of mica. By means of a cross-light illumination the lines are made to appear white on a dark ground. "The observer, with the aid of an assistant, whose office it is to record the declinations, magnitudes, &c., has it in his power to give the elements of position and magnitude of each star at the rate of seven per minute, the average frequency of observations being only two in each minute." Since Professor G. P. Bond's appointment to the direction of Harvard Observatory in 1859, these zone observations have been diligently continued; and in his report for 1864 it is stated that the region between + 1° 00′ and 1° 10″ had been nearly completed, and that great progress had been made in the zones between 1° 10′ and 1° 20′.

It is only necessary to read the Reports of the "Committee of the Overseers" of Harvard College, and the accompanying Reports by the Director of the Observatory, to show that the same zeal animates Professor G. P. Bond that was so strongly evinced by his father. The vast amount of work accomplished in the way of observation, reduction of the results, and their publication, is truly surprising, for we must in forming an estimate always bear in mind that the Observatory of Harvard College has very small means at its disposal, in comparison with the magnitude of its undertakings.

There is one claim to recognition which I, of all persons, must not pass over without notice—namely, the first application of photography to astronomical observations; for it was my seeing in the exhibition of 1851 a lunar photograph, which emanated from this Observatory, that stimulated me to undertake experiments in that direction. The first application of photography to the delineation of our satellite and of some of the fixed stars I had attributed to Prof. W. C. Bond, in connection with Messrs. Whipple and Black, of Boston; but I am informed that it originated with G. P. Bond, to whom, therefore, in conjunction with these last-named gentlemen, the merit of this important step must be assigned.

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[1] Presented to the American Academy of Arts and Sciences, April 15, 1851.

Professor Bond's latest and yet unfinished, work, is that upon the Nebula of *Orion*, which object is also engaging the attention of Lord Rosse. Professor Bond has already sent to England a proof from a steel plate engraved from his drawing; he has catalogued the stars in and around the nebula with more minuteness than the Russian astronomers; and he has been so fortunate as to discover in the nebula a grand feature which has hitherto escaped the attention of astronomers. This new feature is a great re-entering loop of nebulous matter extending around nearly the whole of the previously known portion, and enclosing, as with a nebulous wall, a large space exterior to the well-known figure. It was discovered by employing with the 22-feet refractor of 15 inches aperture, an eye-piece magnifying 90 times, with a field of 30′; by this arrangement it was seen with great distinctness. It is well known how clearly the low powers of comet-seekers bring out the faint details of comets; and the conversion, so to speak, of the Harvard Refractor into a huge comet-seeker has led to this interesting discovery.

In 1851, Prof. G. P. Bond visited Europe, and his reception at the principal astronomical establishments, and especially at the classical Observatory of Pulkowa, as detailed in the Annals of Harvard College Observatory, vol. i. part i.,[a] may perhaps be regarded as evincing something more than the ordinary courteous welcome due to a stranger. In 1863 he again visited Europe, when I had the pleasure of making his acquaintance, and I am convinced that on this, as on the occasion of his previous visit, Professor G. P. Bond not only gained many personal friends, but also the high opinion and respect of those astronomers with whom he came into contact.'—From *The Reader*, *Feb.* 11, 1865; but somewhat altered and extended after comparison with the publication of the Address in the *Monthly Notices of the Astr. Soc.*, vol. xxv, Feb. 10, 1865, p. 125, received after these pages were in type.

4. *Comet* V, 1864.—A comet was discovered by Bruhns, at Leipzic, on the 30th of Dec. 1864. The following elements were computed by Engelmann, from observations of Dec. 30th, Jan. 3d, and Jan 21st.

$T = 1864$, Dec. $27{\cdot}76616$, $i = 17^\circ\ 7'\ 13''{\cdot}7$,
$\pi = 162^\circ\ 21'\ 55''{\cdot}1$, $\log q = 0{\cdot}047095$,
$\Omega = 340^\circ\ 53'\ 52''{\cdot}6$, Motion retrograde.

5. *Duration of the flight of shooting stars.*—Dr. Jules Schmidt, director of the Observatory at Athens, has communicated to Mr. Haidinger some of the results of his observations during the last eight years upon shooting stars. He has recorded the estimated duration of flight of 1357 meteors, out of about 16,000 seen. The mean duration of those of different colors was, of 846 white shooting stars, $0^s{\cdot}709$; of 361 yellow, $0^s{\cdot}947$; of 101 red, $1^s{\cdot}787$; and of 49 green ones, $2^s{\cdot}685$. The mean of all was $0^s{\cdot}925$. This is somewhat larger than the mean duration obtained on page 203 of this volume. Dr. Schmidt having been accustomed to estimate small intervals of time in astronomical observations, his estimates deserve not a little confidence. We trust that he will classify them according to the hour of the night. His earlier observations were made mostly in the evening. The times of the observations of this series are not specified. We believe that there is a decrease in

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[a] Appendix, pp. 158-60, inclusive.

the mean duration in the later hours of the night. Such decrease is distinctly shown by Wartmann's observations in 1838. H. A. N.

6. *Heights of Auroral Arches.*—Mr. B. V. MARSH has obtained data for computing the altitudes of three auroral arches. A fine arch was seen early on the evening of the 16th of January, 1865, and from observations at Germantown, Pa. and Brunswick, Me., the computed altitude is 97 miles. A second arch on the 20th of February was observed at Germantown, Newburyport, Mass., and Brunswick. The Brunswick and Newburyport observations give an altitude of 67 miles; those of Germantown and Newburyport 92 miles. The mean of the two determinations is 79½ miles. A third arch on the 21st of February, observed at Boston and Philadelphia, had the computed altitude of 57 miles. The mean height of the three arches was therefore 78 miles. The data were not very exact in either case, but taken together they are believed to furnish reliable approximations. H. A. N.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The Agassiz Expedition to South America.*—On the 29th of March, Prof. Agassiz, with a large corps of assistants, sailed in the steamer Colorado for Rio Janeiro, on an exploring tour in South America. His corps consists of O. H. ST.JOHN and C. F. HARTT to collect fossils and to aid in geological exploration, J. G. ANTHONY to collect mollusks, J. A. ALLEN to collect birds and mammals, G. SCEVA to make skeletons of mammals, birds, the large reptiles and fishes, and Mr. BURKHARDT to make drawings. Prof. Agassiz will devote himself, with native and such other assistance as he may obtain, to the collection of marine invertebrates, yet will have, for his main object, the study of the embryology of some of the remarkable fishes of the Amazon, and investigations with regard to the drift phenomena, or ancient glacial action, in the Andes.

The party is accompanied also by Dr. B. E. Cotting as surgeon, with the wives of Prof. Agassiz and Dr. Cotting, a son of Mr. N. Thayer of Boston, and a son of Mr. S. G. Ward of that city.

The expedition goes first to Rio Janeiro, whence the geological assistants will journey by land north to the Amazon, while the rest of the party, after completing investigations there, will take vessel for the same river. Prof. Agassiz and party will then ascend the Amazon, to the Andes, and, finally, after explorations in the mountains, descend to Lima.

Professor Agassiz at first intended only a visit to Brazil for his health, and proposed to take along one or two assistants to aid him in making collections for the Museum of which he is Director at Cambridge. On mentioning his plan to Mr. Nathanael Thayer, this generous patron of science at once said "Agassiz, go home, find six assistants, and I will pay the bill." The Pacific Mail Steamship Co., hearing early of the projected tour of exploration, immediately tendered to Professor Agassiz and his wife free passage to Rio; and, afterward, on learning of Mr. Thayer's munificent proposition, Mr. McLane, in the name of the Director of the Company, offered to the whole party free passage in the new steamer Colorado, about sailing for Panama *via* Cape Horn. The arrangements were soon completed, and within three weeks after Mr.

Thayer's promise was made; the expedition left New York in the Colorado.

The Secretary of the Navy has given Prof. Agassiz a letter addressed to all officers of the navy whom they may meet, in order to secure for them free transportation when desired. He offered also a government vessel to take them to their place of destination in South America, but they were already provided, through the liberality above mentioned. Every facility may be looked for also from the Emperor of Brazil, who has, for some time past, manifested great interest in the welfare of the Museum of Comparative Zoology at Cambridge.

Great results may be expected from an expedition under such a leader, so ably supported and so well equipped. The explorations will be mainly inland, and therefore the richer in novelties to science.

2. *Temperature of the climate near large bodies of water.*—Mr. J. S. Lippincott, of Haddonfield, N. J., in an article published in the Second Annual Report of the West Jersey Fruit-Growers Association (1864–5, Philadelphia), attributes the milder climate near the great lakes and the ocean to the atmospheric humidity occasioned by the proximity of water, referring to the principle established by Tyndall that moisture in the air absorbs the heat received from the earth and retains it about its surface, preventing thus its escape into space. and to his views on the subject. He explains in this way why western New Jersey is much less favorable for vineyards than the vicinity of the great lakes or the lakes of New York, situated farther to the north.

3. *Carinthian Lake-habitations.*—Prof. Hochstetter has found evidences of lake-habitations on piles in four of the lakes of Carinthia; namely, those of Wörd, Keutschach, Rauschelen, and Osseach. From the lake of Keutschach, the only one yet particularly investigated, numerous black potsherds, pieces of half-burnt clay, half-carbonized fragments, a whetstone, a portion of a stag's horn, are among the relics obtained.—*Reader, Dec.* 3.

4. *A new meteorite from Arkansas.*—Prof. J. Lawrence Smith has received a portion of a new meteorite from Arkansas, consisting of mixed iron and stony matter, which he has under investigation.—*Letter to G. J. Brush*, dated *Louisville, Ky., Apr.* 24.

5. *Cancerine.*—A fertilizer, named *Cancerine*, is made on the coast of New Jersey, out of *King-crabs*, a crustacean otherwise called "*Horse-shoe*," and in science *Limulus*. They are dried and ground; and, thus prepared, the material is said to be worth half as much as Peruvian guano.—*Prof. G. H. Cook's Report on the Geol. Survey of N. Jersey*, 1864.

6. *Large mass of Amber from India.*—Sir David Brewster has described a mass of amber from India weighing upward of two pounds. It is traversed by veins of carbonate of lime.—*Proc. Roy. Soc. Edinb.*, Jan. 16.

7. *Earthquake at Buffalo, N. Y.*—On the 29th of January last, at 4 A. M., there was a shock of an earthquake at Buffalo.

8. *Louis Saemann.*—The Comptoir Minéralogique et Paléontologique —or establishment for the Sale of Minerals, Rocks and Fossils—of Louis Saemann, in Paris, has been removed to Rue de Mézières, No. 6, where he stands ready to supply on reasonable terms, all wishing to buy, or complete, mineral, lithological, or paleontological cabinets.

9. *Dr. A. Krantz.*—The 7th edition of the Catalogue of minerals, rocks, fossils, casts and models for sale at the large and well known house of Dr. Krantz, at Bonn, in Prussia on the Rhine, has recently been issued, and exhibits great completeness in his stock in all departments, and favorable prices for purchasers. Besides the various other collections, we observe one of 114 models of occurring crystalline forms of various minerals, in wood, well labelled, with reference to standard works, for 16 thalers; and another of 675 forms, for only 120 thalers (the thaler being equivalent to about 75 cents).

OBITUARY.—THOMAS B. WILSON.—The scientific world has sustained a severe loss in the death of Dr. Thos. B. Wilson, the late President of the Academy of Natural Sciences, in this city, which took place on the 15th of March last, at his late residence in Newark, Delaware. Dr. Wilson, who was a native of Philadelphia, has for many years devoted himself to the encouragement and promotion of zoological science, especially in connection with the Academy of Natural Sciences, an institution which, mainly by his energy, ability and princely liberality, has been raised from comparative mediocrity to an equality with the leading kindred institutions of the Old World. The superb collection of Birds, which ranks as the third in importance in the world, and the invaluable Library of the Academy, are but a partial evidence of Dr. Wilson's unostentatious munificence. Every department of the institution bears his mark and will feel his loss. He has also contributed largely of late to the Entomological Society at Philadelphia. Although his residence had been removed to Newark, Delaware, half of his time was regularly spent in his native city and occupied with his favorite pursuits. It is difficult to estimate the value of such men to the cause of science, or the loss which a community sustains when their labors are cut short.—*Daily Evening Bulletin, Philadelphia, March* 21.

GEORGE H. EMERSON.—Mr. George H. Emerson, a young chemist of much promise, originally from the city of Hartford, Ct., died at Greenfield, Mass., after a long and painful illness, on the 28th of December last, at the age of twenty-seven. Mr. Emerson is the author of a memoir "On crystals and precipitates in Blowpipe Beads," presented to the Boston Society of Natural History, an abstract of which was published in volume xxxvii of this Journal (p. 414). Having discovered that nearly all those earths, or metallic oxyds, which produce an opaque bead on flaming, give rise, with proper manipulations, to definite and characteristic crystals, he examined, with great labor and care, a large variety of substances, and carried his investigations so far as to determine and describe the appearance of the crystals or precipitates in different fluxes. His researches—which he regarded as but just begun,—promised to contribute much to the progress of the science of Blowpipe analysis. But he was compelled to discontinue them by his failing health. He had been a student of Chemistry in Prof. Cooke's laboratory at Cambridge but one year when he obtained his interesting results. But the enthusiasm and activity with which he applied himself would have severely tried a stronger constitution than his. Having sought to improve his health by a journey to Florida without avail, he died at Greenfield a few months after his return.

Mr. Emerson belonged to that rare class of men, who perpetuate their good works, and after death remain useful to humanity. By his will he gave his whole estate to the endowment of four scholarships for the relief of needy and meritorious graduates of the Lawrence Scientific School of Harvard College. Such a grand and simple life of devotion needs no drapery of euphonious words. He loved science, and gave for her advancement all he possessed, even his young life. A. H.

WILLIAM J. WALKER.—Dr. William Johnson Walker died on the 2nd of April last, at Newport, Rhode Island, in his 76th year. He had been eminently skilful in surgery and the treatment of disease, but quitted the practice of his profession about twenty years since. He was a liberal patron of science and of education, and not less munificent in his gifts than in his bequests. During his later years his gifts amount to four hundred thousand dollars, of which the Boston Natural History Society had a share. In his will he bequeaths one million of dollars (out of an estate of about one and a quarter millions) in equal parts to the Boston Society of Natural History, Amherst College, Tufts College and the Institute of Technology at Boston.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Naturalists' Directory.* From the Proceedings of the Essex Institute, Salem, Mass. F. W. PUTNAM, Editor.—Advance sheets of Part I of this Directory have been received. It forms part of the April number of the Proceedings, but has separate paging so as to be bound by itself. This first part contains the names and addresses of the naturalists of North America and the West Indies, together with their special departments of science, and other useful information. The publication of this work is to be continued quarterly until completed. The next number will contain the names of foreign naturalists, with their addresses and special branches of study, so far as they can be ascertained. In the part already printed the names have been arranged alphabetically for greater convenience, but in future numbers it is proposed to arrange those of each country according to their special departments.

The utility and convenience of a work of this kind must be obvious to every one interested in Natural History, and the first number gives assurance, by the great labor and care that has been bestowed upon it, that the entire work will, notwithstanding the inherent difficulties of the task, be rapidly and successfully completed, and thus supply a want that has long been felt. A. E. V.

2. *The Social Science Review: a Quarterly Journal of Political Economy and Statistics;* ALEXANDER DELMAR and SIMON STERN, Editors. Vol. I, No. 1, January, 1865. 96 pp. 8vo. New York.—The subjects discussed in this first number of the Social Science Review are—Government; Mr. Fessenden's Report; Herbert Spencer on Social Statics, &c.; Mr. About's Progrès; The limits of Political Economy.

3. *The Preparation and Mounting of Microscopic Objects;* by THOMAS DAVIES. 144 pp., 12mo. New York: Wm. Wood & Co., 61 Walker street.—In accordance with the author's aim, as expressed in his Preface, this little manual supplies a want long felt, and supplies it well,

giving the methods of preparing and mounting (and to some extent, also, of collecting) specimens for the microscope that are in general use, along with the results of his own experience; and with such explanations of the rudiments of the art as may be required by a beginner, as well as those details that the advanced student might need. The subjects of the chapters are: I. Apparatus; II. Preparation and mounting of objects "dry;" III. Mounting in Canada Balsam; IV. Preservative liquids; V. Sections, and how to cut them, with some remarks on dissection; VI. Injection; VII. Miscellaneous information on objects of interest for the microscope.

4. *Trübner's American and Oriental Record.*—The Record, published in London by Messrs. Trübner & Co. (60 Paternoster Row), of which the first number made its appearance on the 16th of March last, is to be "a monthly register of the most important works published in North and South America, India and China and the British Colonies, with the occasional addition of notes on German, Dutch, Danish, French, Italian, Spanish, Portuguese, and Russian books." Its object is stated to be two-fold: *first*, to form a medium of communication between American and Oriental authors and publishers and the English public; and, *secondly*, to make American and Oriental works better known in Europe. It has therefore a special interest to American authors and publishers, and to the American people generally. Coöperation from America is asked for, and especially early intimation of publications in contemplation, with a mention of such particulars in each case as the trade and the reading public demand. The house of Trübner & Co. has long dealt largely in American works, and done much toward extending their circulation through Great Britain and Europe. The first number of the Record runs to 24 pages small 4to. Price 6*d.*

5. *American Journal of Conchology;* edited by G. W. TRYON, Jr.—The first and second numbers of the American Journal of Conchology have appeared, and by their contents promise much for the progress of the science of both recent and fossil shells. The numbers of this quarterly contain 96 pages each; the first is enriched by 9 plates of shells, and the second by 8, several of them colored. The price per number is three dollars, and per year ten dollars. These numbers contain articles on Tertiary fossils by T. A. Conrad; and on recent shells by S. S. Haldeman, T. Bland, W. G. Binney, W. Stimpson, C. M. Wheatley, A. D. Brown, J. G. Anthony, and the Editor, besides "Reviews and Summary of Conchological Publications" and "Scientific Intelligence."

6. *Woodward's Country Homes;* by G. E. & F. W. WOODWARD, Architects. 166 pp., 12mo. New York: G. E. & F. W. Woodward, 37 Park Row, office of the Horticulturist.—This neat little volume is illustrated by numerous cuts representing dwelling houses of various tasteful styles, and plans for their construction, and is well calculated to distribute through the country, taste in domestic architecture and some knowledge of the ways of exhibiting it.

7. *The New York Medical Journal.* Vol. I, No. 1, April, 1865. 88 pp. 8vo. Miller & Mathews, New York and J. B. Lippincott & Co., Philadelphia. $5 per year.—This new monthly is to be sustained by the highest medical and surgical talent of the country, and promises to be the leading Medical Journal.

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ERRATUM.

P. 285, line 12 from top, for *C. Tomlinson, M.D.*, read *Mr. C. G. Stowell.*

THE

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[SECOND SERIES.]

Art. I.—*On the Deep Placers of the South and Middle Yuba, Nevada county, California, in connection with the Middle Yuba and Eureka Lake Canal Companies;* by B. Silliman.

Geographical and Geological position of the Gold in California.—The gold of California is found chiefly on the western slopes of the Sierra Nevada in rocks believed to be not older geologically than the Jurassic. The great gold region of the State, that area from which much the largest amount of gold bullion has been derived, stretches over about three degrees of latitude from near Fort Miller on the San Joaquin river in Fresno county, north to Deer Creek and the Forks of the Feather river in Plumas county, an area of about 200 miles in length and of an average width of about 40 miles, although it widens toward the north to about 70 miles from east to west. Its approximate area is probably about 1000 square miles. There are other important gold-bearing areas, especially those to the north near the sea coast, viz: the Waldo or Sailors' diggings, the Sea Shore diggings, and a large but not very productive district, bounded on the north by the Klamath river, and south by Trinity river and the forks of the Sacramento near Shasta. In the southern part of California are several subordinate gold fields of inconsiderable area, chiefly in the Coast range mountains; such are the Santa Cruz diggings near the coast, the San Antonio, the Francesquito, San Fernando, San Gabriel and Holcomb valley. These are chiefly interesting historically as having furnished the earliest evidence of the existence of gold in California, some of the southern gold fields having been well known to the Spanish missionaries from an early period, long anterior to the date commonly mentioned (1849) for the first discovery of gold in California. Prudential considerations led these ecclesiastics to prevent as far as possible the spread of any knowledge respecting the existence of gold on or near their mission lands.

From this statement it will be observed that the gold in California is probably of two distinct geological ages, that of the Sierra Nevada being Jurassic or Triassic, that of the Coast range Cretaceous or Tertiary.

Sources to which the Gold in California is referable.—The original source from whence all the gold of California has been derived is undoubtedly the veins of gold-bearing quartz which occur so abundantly in all the slates and metamorphic rocks of the western slope of the Sierras within the areas known as the gold regions. But this original or great source of the precious metal is historically secondary to the shallow and deep diggings or placers, in the former of which the gold was first discovered, and which during the early years of California history furnished nearly the whole of the metal sent into commerce. That the placers were derived from the degradation or breaking up of the auriferous veins and the distribution of the detritus thus formed by the agency of running water and ice does not admit of a question. It appears also to be pretty conclusively proved that the gold-bearing gravel is of two distinct epochs, both geologically very modern, but the later period distinctly separated in time from the earlier, and its materials derived chiefly from the breaking up and redistribution of the older or deep placers. These appear to be distinctly referable to a river system different from that which now exists, flowing at a higher level, or over a less elevated continental mass, and with more power, but generally in the direction of the main valleys of the present system. The reasons for this opinion will be hereafter stated more at length.

The sources to which the gold in California is referable are therefore

1st. The distribution of placer gold by the present River System, giving the 'Shallow diggings.'

2d. The distribution of placer gold by an ancient River System, known as 'Deep diggings.'

3d. The gold-bearing quartz veins in the metamorphic rocks of the Sierra Nevada.

This is also the order in which the development of the country by human industry has brought the gold to light: the comparatively small number of exceptions to this generalization from the early workings of quartz mines forming in fact a confirmation of its general accuracy.

The first rush of adventurers was to the shallow placers, where the gold, chiefly washed out of the older placers, was found distributed, within reach of the miners who had only a pick and shovel or pan. Here the first fruits of labor were sometimes very ample, and the capital and skill employed quite small. Gradually, as the gold thus superficially distributed became partly exhausted, streams of water and various contrivances for 'sluicing' were introduced, involving more skill and the union of labor with capital.

It was pretty early discovered that very extensive and valuable deposits of auriferous gravel lay at levels far above the present course of the streams, and that to wash these deposits required the adoption of new methods adapted to meet the case. Hence came the so-called *Hydraulic process*, which, although in use now for more than ten years, has yet made barely more than a commencement upon the great mass of deep lying auriferous shingle which remains to be treated by this method of gold washing.

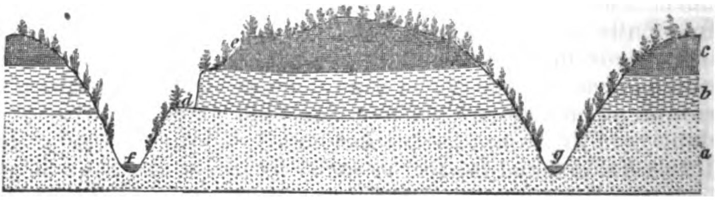
Finally comes the era of quartz mining in depth, the successful prosecution of which demanded more skill and capital, as well as cheaper labor and better machinery than the early days of California furnished. In this man undertakes to do for himself, by the use of his own skill, what in an earlier age nature had done for him on a grand scale in breaking up the matrix of the precious metal, commencing at the fountain head of the stream of gold.

I propose at present to consider with some detail the second of the great sources of gold production, viz: *Deep-lying Placers.* The character of these deposits is well illustrated by a description of the ground between the south and middle forks of the Yuba River, in Nevada county, where this description of gold deposit is well exposed in consequence of the considerable amount of mining work which has been performed there, the whole of this ground being controlled by the waters of the Middle Yuba Canal Company, and of the Eureka Lake Water Company.

The Deep Placers of the Yuba.—The Yuba is an affluent of the Feather river, which it joins at Marysville on its way to its junction with the Sacramento. The South and Middle forks of the Yuba river unite with the North Yuba, the course of which is nearly at right angles to these two branches, whose mean course is west about 13° south (magnetic), the Feather river running about north and south.

The ridge of land embraced between the South and Middle forks of the Yuba is from six to eight miles in width, and to the limits of the auriferous gravel, as thus far explored, about 30 miles, forming an area of about 200 square miles. The elevation of this ridge above the sea is, at its western extremity near French

Corral, about 1500 feet, from whence it gradually rises into the high Sierras, the Yuba Gap Pass being 4570 feet above the sea, and the Downieville Buttes about 8840 feet. This Mesopotamia is cut up by ravines descending from a central axis both ways into the valleys of the two rivers forming 'gulches' with steep sides, often beautifully wooded. The more elevated portions of the land are covered by a heavy bed of volcanic ashes and breccia, which evidently at an earlier day formed a continuous sheet over not only the tongue of land under consideration, but over the adjacent region, as is conspicuously seen in the sections afforded by the various rivers. This mass of volcanic ashes contains numerous angular fragments of cellular lava, trachyte, basalt, porphyry and volcanic mineral aggregates, quite foreign to the general geology of the country. Its thickness varies with the topography and drainage of the surface, but it forms the summits of all the hills above a certain horizon, and in places reaches an elevation of from 2000 to 3000 feet above the level of the rivers. Below Columbia the denudation of the surface has removed the volcanic matter, leaving the auriferous gravel exposed as the upper surface. This volcanic deposit receives from the miners the general name of 'cement,' a term it well deserves from its compact and tenacious character, much resembling pozzuolana or Roman cement. The accompanying section was made by Mr. Hugo Hochholzer to accompany his map of the ridge, and represents the line drawn from the Fellows Quartz Lode, on the Middle Yuba, southeast through Snow Point and Mount Zion, to the South Yuba.



This section represents a point above most of the deep diggings, the gravel on this portion of the ridge being generally covered by the volcanic cement, so as to be inaccessible by the process of hydraulic working, and available at a later day only by deep mining.

It shows the ravines of the two branches of the Yuba at *f* (middle) and *g* (south), the 'bed rock' *a*, the auriferous gravel *b*, and the volcanic cement *c* covering all. The Snow-Point Diggings are seen at *d*, and the position of two of the water-ditches at *e*.

The auriferous gravel varies in thickness from 80 or 100 feet, where it has been exposed to denudation, to 250 feet or more, where it is protected from such action. Probably 120 feet is not an over-statement for its average thickness in the marginal portions, where it has been exposed by working the deep diggings or hydraulic claims. This vast gravel bed is composed of rounded masses of quartz, greenstone, and all the metamorphic rocks which are found in the high Sierras.

It is often locally stratified, but I could find no evidence of any continuity in its beddings. The lower portions are composed of larger boulders than the upper, as a general rule, but this does not exclude the occasional presence of huge boulders in the central and upper portions. In a fresh fracture of the whole thickness of these deposits, such as may be seen daily in the 'claims,' which are being actively worked, a striking contrast of color is seen between the lower and upper portions of the gravel mass, consequent on the percolation of atmospheric waters and air, oxydizing the iron resulting from the decomposition of pyrites, and staining the gravel of a lively red and yellow color in waving lines and bands, contrasting boldly with the blue color of the unoxydized portions. A close examination of the blue-colored portion of the gravel shows it to be highly impregnated with sulphuret of iron, forming, in fact, the chief cementing material which holds the pebbles in a mass as firm as conglomerate, requiring the force of gunpowder to break it up.

In the upper portions of these beds are frequent isolated patches, often of considerable extent, composed of fine sand, clearly showing water lines, curved, sloping or horizontal, but never for any distance regular, and in these portions occur frequently large quantities of lignite, or fossil wood, little changed from its original condition, but blackened to the color of coal and flat with pressure. Among these remains are logs similar in appearance to the Manzanita, now growing abundantly on the hills of auriferous gravel. Some of these, which I measured, were fifteen to eighteen inches in diameter and ten to fifteen feet in length. Occasionally the mass of this ancient driftwood accumulated in these eddies of the current, where they were deposited with the fine sands, amount almost to a continuous bed of lignite.

Wedge-shaped and lenticular masses of tough yellow and whitish clay also occur in the ancient drift, replacing the gravel and affording, by their resisting power, a great impediment to the operations of mining.

The 'slacking down' or disintegration which a few months exposure of the hard gravel 'cement' produces, is due mainly, if not entirely, to the decomposition of the associated pyrites before noted. It is remarkable how large a part of the smoothed and beautifully rounded stones, even those of large size, undergo a similar slacking by atmospheric action, even in a very brief period of time, rendering it almost impossible to preserve specimens of the gravelly concrete unless they are protected by varnish. The most unyielding of the 'cement' masses are sometimes left over one season by the miners, exposed to the air and frost, to secure the benefits of this disintegration, without which but little of the contained gold can be obtained.

The gold is disseminated throughout the entire mass of this great gravel deposit, not uniformly in value, but always in greater quantity near its base or on the bed rock. The upper half of the deposit is found to be always less in value than the lower part, sometimes so poor that it would be unprofitable working by itself, but inasmuch as there is no practicable mode of working the under stratum without first moving the upper portion, in practice the whole is worked.

The gold rarely occurs in large masses in this ancient gravel. Often on the polished and very smooth surfaces of the 'bed rock' and of the superincumbent masses of gravel when freshly raised from their long resting place, the scales of brilliant yellow metal are beautifully conspicuous. These are frequently inlaid so firmly upon the hard granite floor of the ancient river or glacier as to resemble hard stone mosaics. In fact the whole surface of the bed rock requires to be worked over by the pick to secure the gold entangled in its surface, to a depth, when soft, (as of mica or chloritic slate or gneiss,) of several inches.

The method of extracting the gold from these deposits by hydraulic process is described under a subsequent head.

The Bed Rock, as it is significantly termed by the miners, shows everywhere when freshly exposed the most conspicuous evidence of aqueous or glacial action. The course and direction of the motion which has left its traces everywhere is plainly discernable. Nothing would so satisfactorily convey a correct idea of the remarkable features of this ancient floor, moulded and rounded by water or ice, as a series of good stereoscopic photographs such as I hope to secure the present season through the kind assistance of Mr. C. F. Watkins, of San Francisco, so well known for his admirable California views. The "bed rock" varies of course in different portions of the area now under consideration, being either granite, gneiss, greenstone or shale. In the granite are observed numerous minute quartz veins pursuing a course parallel to each other often for hundreds of feet without interruption.

In the 'American Claim,' at San Juan, the granite is succeeded on the west by a large jointed blue siliceous shale, of the same strike with the main joints of the granite. This latter rock is covered by numerous very large boulders of metamorphic conglomerate, of which no traces are seen in place.

The course of the ancient current where I had an opportunity of measuring it, appears to have been about 20°–25° west of north, (magnetic,) which it will be observed is nearly at right angles to the mean course of the middle and south forks of the Yuba; but it is not far from parallel with the axis of the Sacramento river valley, or of the great valley between the Coast Range and the Sierra Nevada. I have noted the same general direction of the scratches elsewhere in the great gold region, but additional observations are required to justify any comprehensive generalization. This much appears clearly shown, however, by the present state of our knowledge on this subject, viz: that the spread of the ancient gold-bearing gravel was produced by a cause greatly more elevated than the existing river system, or, which is more probable, at a time when the continent was less elevated than at present,[1] and moving in a direction conformable to the course of the valleys of the Sacramento and San Joaquim. We find it impossible to admit the existing river-system as a cause adequate to the spreading of such vast masses of rounded materials; the facts plainly point to a much greater volume of water than any now flowing in the valley. The section already given illustrates perfectly the relations of the present river-system to the more ancient one whose grand effects are chronicled in the bed rock and its vast superincumbent mass of auriferous gravel. It serves also to illustrate the process now still in progress, by which the existing river system derived its gold-bearing sands, in great part at least, from the cutting away and secondary distribution of these ancient placers.

Those who have had the opportunity of visiting other portions of the great gold region of California than that now under consideration will at once recognize the local characters of the details given as perfectly consistent with the general phenomena of the ancient placers as observed elsewhere; while at the same time great differences are found in many of the details. Thus in Calaveras and Tuolumne counties, 80 or 100 miles farther south, the volcanic matter capping the auriferous gravel is found in the form of basaltic columns, beneath which recur the same phenomena already described. Here the wood contained in the gravel beds is beautifully agatised, or converted into semi-opal, as is the case also at Nevada City, Placerville, and elsewhere, associated with beautiful impressions of leaves of plants and trees similar in appearance to those now found in this region.

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[1] It is the opinion of geologists that the larger part of the erosion excavating the valleys of the continents took place after the Tertiary period. It was probably during this same time that the deep-lying auriferous gravel was produced from the degradation of the metamorphic schists and quartz veins of the Sierras by the joint action of water and of glaciers.

This general description of the deep-lying placers of the Yuba might be greatly extended from my notes, but enough has probably been said to convey the impression that the phenomena here described are on a grand and comprehensive scale, and referrable to a general cause long anterior in date to the existing river-system; a cause which has been sufficient to break down and transport the gold-bearing veins of the Sierras, with their associated metamorphic rocks, thus laying up in store for human use deposits of the precious metal in amount, on a scale far beyond the notions generally prevailing of the nature of placer deposits.

Quantity of Gold in the Deep Placers of the Yuba.—The extensive mining operations which, since 1852, have been carried on upon the ridge of land between the South and Middle Yuba rivers, have supplied the data requisite for a pretty accurate estimate of the average value of gold actually saved in mining and washing a given quantity of auriferous gravel. Without making an exact survey of the ground, it would be impossible to give a precise statement of the total quantity of gravel which has been washed away, much less of what yet remains to be washed. Fortunately, Mr. George Black, a skillful English engineer long resident in California, has twice made a reconnoissance of the ground now under consideration, and his Report,[1] privately printed, has been placed at my disposal. I shall use its data with freedom so far as they are required to confirm or extend my own observations.

The mining ground in this area stretches along both margins of the delta from French Corral, a place near its western extremity, in a line pretty closely parallel to the Middle Yuba, skirted by the claims known as Birchville, Sweetland's, Sebastopool, the Eureka claims (at North San Juan), Badger's Hill, through Grizzly Gulch to Woolsey's Flat, Moore's Flat, Orleans Flat, and Snow Point to Eureka, and thence crossing to the South Yuba slopes; it includes Mt. Zion, Relief Hill, Bloomfield, Lake City, Grizzly Hill, Columbia, Pleasant Hill, and Montezuma, the entire circuit being over sixty miles.

But I was fully convinced from my own examinations of this ground, in November of last year, that but a very small part of the mining ground available for early development and quite within easy control of the existing flow of water furnished by the Middle Yuba Canal Co. and the Eureka Lake Co. has been taken up, much less opened for work. Mr. Black estimates the length of the mining claims at present supplied with water by the Middle Yuba Canal Co. at five miles, with an average width of three hundred and fifty yards, and an average depth of forty yards, making a quantity of one hundred and twenty-three millions of cubic yards of auriferous gravel. He also estimates that eight per cent of this quantity has been worked away in the past twelve years, leaving one hundred and thirteen millions of cubic yards which remain for future operations. At an average of thirty-four cents of gold to the cubic yard (the average in the Yuba region appears to be from thirty cents to forty-five cents per cubic yard, saved in the hydraulic process), the volume of auriferous gravel here estimated would yield over thirty-eight millions of dollars. But the total area of the various places where gravel deposits have been worked on this ridge is estimated by Mr. Black as equal to fifteen square miles, all of which, and much more, is controlled by the water of the Eureka Lake Co., or of the Middle Yuba Canal. If this area is estimated at an average of forty yards in depth (it varies from eighty to two hundred and two hundred and fifty feet in depth), we shall have one thousand eight hundred fifteen million nine hundred and thirty-six thousand cubic yards of gravel, and if this be estimated to yield only thirty cents per yard, we reach the grand aggregate of five hundred and forty-four million six hundred and ten thousand dollars as its probable yield in gold.

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[1] Report on the Middle Yuba Canal and Eureka Lake Canal, Nevada Co., California. By George Black, Civil Engineer. San Francisco. 1864. pp. 32.

The average cost of the water required to wash away one cubic yard of gravel has heretofore been seven and a half cents: but if its price is reduced to six and a quarter cents (=16⅔ cents for one miner's inch of water) for each cubic yard, the cost of the water to perform this work will be nearly one hundred and twenty millions of dollars ($119,316,320). It is easy to see from these statements that the amount of gold contained in the deep placers of the Yuba alone is probably greater than the aggregate of all the gold yet exported from the whole Pacific coast, which (including silver) amounted on the first of January, 1865, to $695,944,786.[3] Mr. Black's estimate of the area of mining ground applies only to certain marginal parts of the Yuba district which are not covered by the volcanic tufa. I saw beyond these limits a vast amount of valuable mining ground in this area, all under control of the existing water-flow of the Eureka Lake Company. There is little doubt, that at a future time a considerable portion of the ground covered by volcanic ash beds will be made to yield its precious deposit in a day of cheaper labor, and with improved methods of mining. At Forrest City, Minnesota, and Alleghany town, situated on the north bank of the Middle Yuba, immediately opposite Snow Point, the volcanic ash bed covers the 'great Blue Lead' (part of the ancient auriferous gravel,) where it has yielded almost fabulous wealth to the explorers. The

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[3] Mercantile Gazette for January 12, 1865.

'Live Yankee Claim,' for example, at Forrest City, is reported to have paid its owners over three millions of dollars. The south side of the Middle Yuba yet remains to be explored for the continuation of this streak of 'rich pay' which points nearly in the direction of the section given in this Report.

It will be observed that these estimates apply only to the value of gold actually saved. That this amount is small compared with the total contents of the placers, will appear when we come to describe the method by which it is saved, and see how crude that process yet is. What goes down the ravines from the washing is not all lost, as it is partially washed again in the rivers below by the Chinese and others, but there are no data for determining how much is thus saved. No account is taken in this connection of those extremely rich deep placers, which, like the workings at Forrest City, &c., just named, and other localities, yield gravel so rich in gold that the whole mass is often worked in a crushing mill.

Process of Hydraulic Mining.—With the more or less complete exhaustion of the shallow placers in the ravines and river beds in California, where the gold was first obtained with little labor and by the most simple means, came the necessity of devising a system by which the deep placers, like these under consideration, could be economically worked. The accomplishing of this object demanded the use of a large amount of capital, to be expended in the construction of canals and aqueducts to convey water from the mountains and fountain heads of the streams, at a suitable elevation and in sufficient quantity to command the ground to be worked, as well also as for the opening of tunnels and shafts in the 'bed rock,' for the discharge of the gravel, an operation requiring much labor and skill, and consuming often several years in their prosecution.

The association of labor and capital thus demanded, called into existence in various parts of the State, Canal and Ditch companies, the associates being generally miners, whose limited finances were eked out by borrowing money from bankers, at rates of interest ranging from three to five per centum monthly. The Middle Yuba Canal Company was thus organized in 1853, and the construction of its hydraulic works commenced, the water being brought from a point on the Middle Yuba river just below Woolsey's Flat, where the stream was dammed, with a total cost, including the various branch ditches, reservoirs and extensions, to June 1858, of $397,171.91, (since then increased to about $600,000) and with a safe capacity of thirty-eight cubic feet of water per second, which can easily be increased by inexpensive improvements to sixty cubic feet per second. The first named quantity is equal to 1500 inches miner's measurement, the second to 2280 miner's inches.[4] The Eureka Lake Water Company's works, commenced in 1858, have cost about one million of dollars.

Experience has demonstrated that the larger the volume of water employed in the process of washing, the more the efficiency and greater the economy of the operation. The proper application of the great mechanical force furnished by large volumes of water under a great pressure was a problem solved satisfactorily only after many abortive trials and large experience. This problem involves the following conditions.

1st, The whole mass of auriferous gravel must be moved, whatever its depth, quite down to the 'bed rock.'

2d, This must be accomplished by the action of water alone, human labor being confined to the application of the water, and the preliminary preparations it involves, the amount of material to be moved and disposed of in every day of ten hours being from 1500 to 3000 cubic yards for each first class operation, involving the use of 400 inches of water.

3d, The mechanical disintegration of the compact conglomerate as a part of the uninterrupted operation of the whole system.

4th, The contemporaneous saving of the gold, without interrupting the continued flow of the water.

5th, The disposal of the accumulations resulting from the removal of such vast masses of auriferous gravel.

These conditions are in practice met by the following steps. The mining ground being selected, a tunnel is projected from the nearest and most convenient ravine, so that starting in the 'bed rock' on the face of the ravine, it shall approach the center of the gravel mass to be moved at a gradient of about one in twelve to one in twenty. The dimensions of this tunnel are usually six feet in width by seven feet in height, sometimes wider; and where possible, it is carried on a line of contact between the granite and the shales, for the greater ease of excavation. These tunnels vary in length from a few hundred feet to several thousand feet, some of the longer consuming from two to four or five years in driving, at a cost of from $10 to $60 per foot, varying with the character of the rock to be excavated. The end of the tunnel is designed to be from fifty to one hundred or more feet beneath the under surface of the gravel at a point where a shaft is sunk through the gravel and 'bed rock' until it intersects the tunnel. It obviously demands careful engineering to carry out works of such magnitude with the accuracy required, and for the want of sufficient care or skill in this particular, years of costly labor and anxious expectation were sometimes wasted in the early history of these enterprises.

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[4] The miner's inch of water, in California is that quantity of water which will pass through an opening of one square inch area under a mean pressure or head of six inches. In practice the water from the canal is conducted into a measuring box (see the accompanying map for a figure of this box) twelve or fourteen feet square, in the sides of which openings are made two inches in height, and extending across three of the sides. These openings are closed by slide valves when not in use. The sectional area through which the water flows determines of course the volume by measurement. Thus twenty inches in length of slit by two inches in depth, under a head of six inches, is called forty inches miner's measurement. A cubic foot (=7·49 U. S. gallons) equals 38 miner's inches. The water discharged by one miner's inch in 24 hours is equal to 2,274 cubic feet, or for one working day of ten hours, 1,098 cubic feet. The average consumption of water on each mining claim in active work is equal to three hundred miner's inches. This quantity of water flowing ten hours is equal to 329,400 cubic feet or 2,470,500 United States gallons, a greater quantity than is required for the supply of the city of San Francisco with a population of over one hundred thousand people.—*Black.*

The object of this laborious exploration is obvious. The long tunnel becomes a sluice-way through the whole length of which 'sluice boxes' are laid, at once to direct the stream and save the gold. For this purpose a trough of strong planks is placed in the tunnel two and a half feet wide and with sides high enough above the pavement to control the stream. The pavement is usually composed of blocks of wood six inches in thickness and as wide as the sluice, cut across the grain of the wood. These are placed about two inches apart at the ends and held in position by cleets of two inches square. In the interstices ('rifles') of two inches by four thus left, a small portion of quicksilver is placed, to aid in catching the gold which finds its way into these hollow spaces.

The vertical shaft is intended to furnish a fall of sufficient height to break up the harder masses of conglomerate and cement as the gravelly stream is precipitated, dashing from side to side of the shaft, and finally with great force upon its rocky bottom.

The water from the canal is brought by side flumes or aqueducts to the head of the mining ground, with an elevation of one hundred to two hundred feet above the bed rock, and it is conveyed into the bottom of the mining claims by iron pipes sustained on a strong incline of timbers. These pipes are of sheet iron, of adequate strength, rivetted at the joints, and measure from twelve to eighteen inches in diameter. These communicate at the bottom with a strong prismatic box of cast iron, in the top and sides of which are openings for the adaptation of flexible pipes, made of a very strong fabric of canvass, terminating in nozzles of metal of two and a half to three inches in diameter. From these nozzles the streams are directed against the face of the gravel to be washed, with a force comparable to that of ordnance. The volume of water employed varies, of course, with the work to be done; but it is not uncommon to see four or five such streams, each conveying two hundred inches of water, acting simultaneously on the face of the same bank. One thousand miner's inches are equal to twenty-six and a third cubic feet of water, weighing 1632 pounds, constantly discharged against the face of the bank under a pressure of ninety to two hundred pounds to the square inch, varying with the height of the column. Under the continuous action of this enormous mechanical force, aided by the softening power of the water, large sections of the gravelly mass come crashing down with great violence. The debris, speedily dissolving and disappearing under the resistless force of the torrent of water, is hurried forward to the mouth of the shaft, down which it is precipitated with the whole volume of turbid water. Boulders of one hundred to two hundred pounds in weight are shot forward by this impetuous stream, accompanied by masses of the harder cement, which meet in the fall down the shaft and in the concussion of the great boulders, the crushing agencies required to disintegrate them.

The heavy banks of 80 feet and upwards are usually worked in two benches. The upper half is never so rich as the lower, and being also much less firm is worked away with greater rapidity. The lower section is much the most compact, the stratum on the 'bed rock' being strongly cemented by sulphuret of iron and great pressure, resists even the full force of the water stream until it has been loosened by gunpowder. For this purpose adits are driven in on the 'bed rock' forty or fifty feet from the face of the bank, and a tunnel extended at right angles therefrom to some distance each side of the adit. In this tunnel a large quantity of gunpowder is placed, from fifty to two hundred kegs, and fired as one blast by a train laid from without. In this manner, the compact conglomerate is broken up, and the water then rapidly completes the work.

Sometimes the system of tunnels on the bed rock is extended much as in a coal mine, by cross alleys leaving blocks, which are then washed away, when the whole mass settles and disintegrates easily under the influence of the water.

The tunnels in the bed rock already described are made double, for the convenience of 'cleaning up' one of them while the other is in action. The process of cleaning up is performed every ten or twenty days, according to the size and richness of the work, and consists in removing the entire pavement of blocks from the bed of the sluice, and removing all the amalgam of gold and 'rich dirt' collected in the 'rifles,' and replacing the blocks in the same way as at first; advantage is taken of this occasion to reverse the position of the blocks when they are worn irregularly, and to substitute new ones for those which are worn through. The mechanical action of the washing process on the blocks is of course very rapid and severe, so as to command a complete renewal of them once in eight or ten weeks. Some miners prefer a pavement of egg-shaped stones set like a cobble-stone pavement, the gold being deposited in the interstices. Most of the sluices are however paved with rectangular wooden blocks as described.

Standing at the mouth of one of these long bed rock tunnels in full action, one unaccustomed to the process is filled with a sense of amazement amounting almost to terror, as the muddy mass sweeps with great velocity onward, bearing in its course great boulders which add to the roar of the water, the whole being precipitated down a series of falls, at each of which it is caught up again by new sluices of timber lined like the first one, and so onward and downward many hundreds of feet, until the level of the river is reached at the distance perhaps of half a mile or more from the mouth of the first tunnel. At each of these new falls of twenty-five or fifty feet, the process of comminution begun in the first shaft is carried forward and a new portion of gold is obtained.

Another ingenious device to secure the gold is by means of what are called *under currents*. At the end of the last sluice box, and beyond the mouth of the tunnel a grating of iron bars is arranged lengthwise in the bottom through which a portion of the water and finer material falls upon a series of more gently graded sluices of double the width of the main sluice. These sluices are placed at right angles to the other while the great body of the gravel with the large boulders go dashing forward over the fall, while the finer part thus diverted is more gently brought in contact with a new set of mercurial rifles, from which it rejoins the main torrent; and the same process is repeated at each succeeding fall, until the river is reached.

Rude as this method of saving the gold appears, experience shows that more gold is saved by it than by any other method of washing yet devised, while the economical advantages it offers are incomparably greater than any other. In fact, it would be entirely impossible to handle so vast a body of poor material in any other way now known.

To show the enormous advantage gained by the present system of working, compared with those formerly in use, Mr. Black states that, taking a miner's wages at four dollars per day, the cost of handling a cubic yard of auriferous gravel is as follows:

With the pan, - - - - - - -	$20.00
With the rocker, - - - - - -	5.00
With the long tom, - - - - - -	1.00
With the hydraulic process, - - - -	.20

In fact, man has, in the hydraulic process, taken command of nature's agencies, employing them for his own benefit, compelling her to surrender the treasure locked up in the auriferous gravel by the use of the same forces which she employed in distributing it!

I have dwelt with the more fullness on this process, so familiar to all Californians, because I am persuaded that few who have not seen it in action have any adequate notion of its magnitude and importance. By no other means certainly does man more completely change the face of nature than by this method of hydraulic mining. Hills melt away and disappear under its influence, being distributed in the river beds below, every winter's freshets carrying to lower and yet lower points portions of the detritus, while whole valleys are filled with clean-washed boulders of quartz and other rocks left behind in the general debacle. Meanwhile the Sacramento and its tributaries, and likewise the San Joaquim flow turbid with red mud, bars are formed where none existed before, and the hydrography of the bay of San Francisco is changing under the influence of the same causes. The desolation which remains after the ground washed by the miner is abandoned, is remediless and appalling. The rounded surface of the bed rock torn up with picks and strewn with great boulders shows here and there islands of gravel, rising in vertical cliffs with red and blue stains, serving to mark the ancient level and filling the spectator with amazement at the changes, geological in their nature and extent, which the hand of man has wrought.

But the *auri sacra fames* heeds no suggestion of the æsthetic; the gold is there, we have the means and the power to remove it, why should we stay our hand! The process is but just commenced. It has required already twelve years to remove, as we have seen, eight per cent of the mining ground, already recognized as such, in but a small portion of the area between the two forks of the Yuba. Not this generation, nor yet a fifth from this, will see the completion of the work, while each year will hereafter witness an increased production of gold up to the maximum possible with the water at command. The amount of gold sent into commerce from this very limited portion of the ancient auriferous gravel has for many years averaged over two millions of dollars annually.

Water Supply.—It follows as an obvious consequence of what has been said of the Deep Placers, and of the hydraulic process of mining, that an adequate supply of water at a suitable elevation is essential for the extraction of the gold contained in the ancient auriferous gravel. The water shed between the two branches of the Yuba River now under consideration, includes not only the tributary streams which rise in the Sierras, such as Cañon Creek, Pass Creek, and others, but a great number of small and larger lakes, from a few acres to several miles in area which nestle among the hills. Of these the largest is the Truckee Lake;

and although the waters of this considerable reservoir are not now flowing in either of the canals under consideration, I am credibly informed, on high authority, that they are likely to do so ere long. Cañon Creek Lake (called on the map Eureka Lake) is the most considerable reservoir of the Eureka Lake Canal Co. I visited it in November, after the first fall of snow, but before the waters had commenced accumulating. In four or five weeks time after it was full. A substantial dam built of blocks of granite, raises its waters to a present average height of forty-two feet above its outlet, with natural abutments of granite capable of receiving twenty feet more of height. Its base is transversely at bottom one hundred and twenty feet, its height seventy feet, and from bank to bank its top measures two hundred and fifty feet. The water face is protected by a double covering of sawed planking, securely fastened, and in all five inches in thickness. The flow of water is regulated by a sluice-way of arched masonry. When full, the present capacity of this reservoir is estimated at 933,000,000 cubic feet of water. By an increase of twelve feet in the height of the dam, Mr. Black estimates the increased capacity of this reservoir to be 262,000,000 of cubic feet of water, equal to twenty-five days supply of the canal with a constant stream, or in all about six months' supply.

But the canal receives its supply for four months, say from the middle of April to the middle of August, from the mountain streams, which afford a plentiful supply from the constant melting of the snows during this season; the snow accumulates in great quantities in the snowy Sierras during the winter months, the melting of which supplies not only the flow of the streams, but fills also to overflowing all the mountain lakes and artificial reservoirs, in which the waters are kept in reserve against the droughts of late summer and autumn. Rarely, as in the summer of 1863–4, does the snow-fall fail to meet the demands. During the year named, for the first time since the construction of the canals, the reservoirs were not half filled, but during the current year they were filled again by mid-winter. The rain-fall for 1863–4 was less than has been known in California since 1850–51. The snow in 1863–4 measured only four feet.*

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* Mr. Black gives the following table, &c.

Statement of the Rain-fall, as registered at Sacramento for fifteen years:

Year.	Rain-fall in inches.	Year.	Rain-fall in inches.
1849–50	36.	1857–58	15.003
1850–51	4.730	1858–59	16.021
1851–52	17.980	1859–60	22.107
1852–53	36·362	1860–61	16.097
1853–54	50.068	1861–62	35.549
1854–55	18.620	1862–63	11.579
1855–56	13.770	1863–64	7.372
1856–57	10.443	Mean rain-fall,	18.64

From this table it will be seen that the rain-fall for 1863, '64, is only 7.37, whilst

Besides the main reservoir are other smaller lakes or reservoirs, of which the principal one is Lake Faucherie, on the course of Cañon Creek, perhaps four miles below the main reservoir. A timber dam of thirty feet in height has been constructed across its outlet, forming a reservoir of about two hundred acres, giving a volume of 217,000,000 of cubic feet. Dams have also been erected across the outlets of several small lakes to the west and south of Lake Faucherie, from the outlet of one of which the main canal commences.

The Eureka Canal is constructed partly in earth and partly as a wooden flume. The dimensions of the main flume are five feet nine inches in width; depth, three feet; fall, sixteen feet per mile. The discharge is ninety-six and forty-four-hundredths cubic feet per second, or 3,485 miners' inches, taking the depth of water at two feet nine inches. If the full depth of three feet were attained, the discharge would be one hundred and thirteen and thirty-three-hundredths cubic feet, or 4,306 inches; but on account of the irregularity in the grade and the subsidences which have taken place, it is not practicable, at present, to fill it to its full capacity.

Taking 3,485 inches as the supply, and deducting ten per cent for loss by leakage, evaporation, etc., will leave 3,037 inches, the supply which can be made available. The practical result, which is 3000 inches, agrees very closely with this; 3,037 inches of a constant discharge during a working day of ten hours is equal to 7,289 inches for twenty-four hours, which latter quantity, therefore, is the available capacity of the canal, irrespective of the Miners' and other confluent ditches. The discharge per second, being ninety-six and forty-four-hundredths cubic feet, equals eight and one-third millions of cubic feet for twenty-four hours. The yearly complement is therefore 3,041,000,000 cubic feet.

The storage capacity for the supply of the Canal is thus stated:

Cañon Creek Reservoir,	933	millions cubic feet.
Lake Faucherie,	217	" " "
Smaller lakes,	100	" " "
	1,250	" " "

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the average fall for the last fifteen years has been 18·64. In the mountains, rain rarely falls after the month of September. It is then converted into snow through the coldness of the climate. In April it commences to thaw, and during the few following months all the water-courses and rivers have a plentiful supply of water. The rain-fall of the mountains (or the snow which falls there, when converted into water) is from fifty to seventy per cent more than the rain-fall of the valleys; (according to experiments which I made on the Middle Yuba in 1856–7, I found that seven feet of snow measured, after its fall, one foot of water; therefore, as an approximation, the fall of snow, divided by seven, is equal to the rain-fall.)

This amount divided by eight and one-third millions, the daily supply, equals one hundred and fifty days', or five months' storage supply. A partial deficiency exists during three months of the year, from the middle of January to the middle of April, during a part of which period, however, the demands for washing are at a minimum. In ordinary years the reservoirs are not drawn from before the middle of August. In 1864, however, the draft commenced as early as July 1st, owing to the very exceptional dryness of that season.

The main canal after leaving Cañon Creek takes a westerly direction, and follows the contour of the hills on the south side until it crosses Jackson Creek, a distance of about seven miles; thence taking a more northerly direction for two miles, it crosses a depression in the ridge, and keeping above Weaver's or Eureka Lake, follows the north slope of the ridge to Eureka, a distance of about eleven miles; continuing in the same direction two miles further, it crosses a low depression by means of the Magenta and National aqueducts. From this it still follows the same slope of the ridge to Snow Tent, a distance of thirty-two miles from its commencement. At this point, being on the crest of the ridge, it discharges itself on the north side into Bloody Run, a tributary of the Middle Yuba, and on the south side into Bloomfield Creek, a tributary of the South Yuba, thus obtaining the complete command of the country from river to river.

Among the wonders which strike a visitor on first seeing the mining regions of California, are the lofty aqueducts constructed on trestle-work for the purpose of carrying the water across deep ravines. Often these structures are slight and insecure, but the Magenta and National (or Washington) aqueducts on the line of the Eureka Canal are remarkable exceptions to this. They are thus described by Mr. Black:

"The Magenta and National aqueducts, which reflect great credit on the ability and skill of Mr. Faucherie, the engineer, are the finest works of the kind in California. During their construction, in 1859, I had frequent opportunities of inspecting them, as I was at that time engaged in carrying on the works of the Sierra Nevada Lake Water Canal, on the opposite side of the Yuba. The National Aqueduct is in length 1,800 feet; its greatest height sixty-five feet. The Magenta Aqueduct is 1,400 feet in length, and its greatest height one hundred and twenty-six feet. The size of the flume is seven feet by one foot and three inches; the inclination, or grade, one foot per hundred feet. The sides were made as low as practicable, so that the high winds which sweep across this gap might have little effect upon them. The posts of the treatles were all hewn from trees growing in the vicinity, no splicing existing in any part of them; they are whole from foundation to top. The sides of the flume, one foot three inches in depth, are formed of whole scantlings thirty feet in length and seven inches in width. The trestles are placed thirty feet apart from centers; they are well and securely braced. The whole aqueduct was built in sections of thirty feet each; each section, when completed, being raised on the spot where it was constructed, by which scaffolding was completely dispensed with. This work, to-day, is in good preservation, the foundations and superstructure being alike sound and substantial, and likely to last for many years."

The aggregate length of all the ditches in the Eureka Company's ownership is about two hundred miles.

In the ownership of the San Juan Company the aggregate length is about half this quantity, twenty-six miles being in the main San Juan ditch.

The safe capacity of discharge of the canals of both Companies, being a constant stream during 10 hours, is as follows:

	Miner's inches.	
Eureka Lake Canal	3,000	
Miners' Canal	750	
Other districts	250	
		4,000
Middle Yuba Canal		1,500
		5,500
5,500 inches for 10 hours, equals for 24 hours	13,200	
20 per cent deficiency in supply, stoppages, and other causes,	2,620	
		10,580

Say 10,000 inches per day.

The laws of California are quite peculiar in respect to the rights of miners to the control of mining ground and of water. The miner has no ownership in fee, but an absolute control so long as he conforms to the mining laws of his district. Mining being the great business of the State, the rights of miners have been most carefully guarded, and are esteemed prior to all other property rights of real estate. As respects the ownership and control of water, the laws permit and protect usages unknown under the English common law, such as the permanent diversion of water from its original channels for mining uses. The right of 'locating' streams for such purposes is fully recognized and established as against adverse interests, by numerous decisions of the highest Courts of law. An abstract of these mining laws may be found in Hittel's Resources of California, page 354.

ART. II.—*On the Ice in Kennebec River;* by Rev. FREDERIC GARDINER.

THE following observations were made upon the ice on the Kennebec river during the months of February and March, 1865. The location is in the town of Gardiner, at a point where the river is about 700 feet wide. The water is entirely fresh for many miles below, and the average ebb and flow of the tide here is five feet. The depth of water varies, according to the state of the tide and the particular locality, from 17 to 25 feet. In the course of the winter the ice is always observed to crowd ashore, crumpling up in ridges on the flats and near the edge of the channel. This process was already well advanced when, after various delays, these observations were begun, Feb. 6. A row of stakes was planted in the ice, by boring holes quite through to the water, at distances of about 100 feet apart, avoiding a very near approach to either shore. Their positions were determined by observing the range of each with a near and a distant fixed object on the shore, by means of an instrument with a small telescope, and also by the angles subtended at each position by fixed objects on the opposite shores. After an interval of time, the instrument was placed in the same range, and the distance from it to the stake measured. The stakes were soon broken off even with the ice by boys, and then a heavy snow fall with the consequent sinking of the ice and formation of a separate sheet of ice above, with water between and slush above, made it impossible to recover the ends of the stakes until March 18th. The distance between the eastern and western stakes was 500 feet. March 18, the easternmost stake was found to have moved to the eastward 12¾ inches. A stake 200 feet west of this had not sensibly changed its position. The western-most stake had moved to the westward 12 feet 2 in. There was thus a total expansion of the ice of 13 feet 2¾ in. in a breadth of 500 feet, or 2·646 per cent nearly, in 40 days. Of course this motion is entirely independent of the action of gravity, and is possibly due to variations in the temperature of the air, that of the water having been nearly constant, as will be seen below. It is to be regretted that there are no data for determining the proportion of this motion in successive proportions of time—a defect which it is hoped the observations of another winter, and of observers in other localities, may supply. The temperatures observed at my house, 120 feet above the river, during the time, are as follows, in degrees Farenheit: Mean temperature, Feb. 6 to 28 inclusive, 22·37°; mean of extreme heat of each day, 32°; mean of extreme cold, 12·74°; mean diurnal variation, 20·217°; extreme heat, 45°; extreme cold, -17°; extreme variation, 62°

March 1 to 18, inclusive.—Mean temperature, 33·138°; mean of extreme heat, 41·33°; of extreme cold, 24·944°; mean diurnal variation, 15·44°; extreme heat, 50°; extreme cold, 7°; extreme variation, 43°. These temperatures, of course, are each that of the shade; they would be much increased by taking into consideration the influence of the sun to which the ice was exposed.

When the ends of the stakes were recovered, they were floating in greatly enlarged holes. The stakes were pieces of pine turned, ¾ in. in diameter. They were placed in holes of 1¼ in. diameter, and frozen in firmly. When found, the holes varied from 1½ to 6½ in. diameter. This fact probably accounts for the anomalous temperatures given below, and must be due to the action of the sun's rays in the substance of the ice absorbed by the wood of the stakes. It also illustrates the effect upon the ice of objects within its substance. The larger holes were in the middle and near the eastern side of the river; the smaller were toward the western shore, where a high bank cuts off the sun early in the afternoon.

The subjoined temperatures of the water and ice were taken with thermometers enclosed in cylinders of pine of such size as to leave a minimum thickness of ¼ in. of wood outside the bulb. The piece of wood covering the graduated side of the stem was confined only by an india rubber ring which could be slipped off in an instant. These thermometers were made by Green of New York, accurately graduated to $\frac{1}{10}$° C., and easily read to $\frac{1}{20}$°. The temperatures of the air and snow were observed with naked thermometers. The minimum time of exposure in each case was a half hour, and it was not found that a longer exposure produced any change, although an hour was repeatedly tried. The thermometers were sunk perpendicularly in the ice, by boring holes with an augur a trifle larger than the thermometer cases, and, after inserting them, stopping the top of the hole with dry snow. The measurements, in all cases, are to the center of the bulb, which was about 1½ in. long. The temperature of the water was always observed near the bottom of the river, and also immediately under the ice; but no difference was observed except on Feb. 6, when the water near the bottom was 0·10, and just below the ice −0·05, the ice at the time wasting a little. Feb. 15, 0·10; Feb. 22, 0·15; March 11, 0·35; March 18, after a heavy rain, producing a freshet and filling the river with melted snow, 0·15. This freshet terminated the observations upon the ice.

At a point about the middle of the river and near the line of the stakes, varied, however, a few feet for each observation, the following observations were made: Feb. 6, mean temperature of air in shade, 0°; depth of snow on ice, 15 in.; thickness of "snow ice," 6½ in.; of black ice, 6½ in.; total, 13 in.—surface of ice 3 in. below surface of water. Temperature of snow just below its surface, 0°; just above the ice, -3·45°; of ice at depth of 10 in., -1·95. Feb. 15, air in shade, -4·15; depth of snow, 6 in. Below this was a sheet of ice generally 3½ in. thick, then a layer of water 2½ in., then about 12 in. of ice; in all, 18 in. The temperature of the ice was taken at a point where the two layers were frozen together. Snow near surface, -1·55°; just above ice, -0·75°; ice at depth of 2 in. 0·46°; at 14 in., 0·65°.[1] Feb. 22, thickness of snow, ice, and water, about the same. Temperature of air in shade, -8·90°; of snow near its surface, 0°; just above ice, 0·55°; of enclosed water, 0·05°. The layer of water prevented the examination of the ice below. March 18, snow on the ice all gone; upper layer of ice, 4 in., but much disintegrated and in places wholly gone; enclosed water varying from 2 to 5 in., and the entire thickness of the whole reduced to from 14 to 15 in. Temperature of enclosed water, 0·85°; temperature of air in shade, 3·90°; in sun, 7·25°. At another point, where the draft of air under a bridge kept the surface nearly free from snow, but still fully exposed to the sun, the following observations were made: February 15—air in shade, -4·70°; in sun, 0°; thickness of "snow ice," 3 in.; of black ice, 19 in.: total, 22 in. Temperature of ice at depth of 2 in., -0·35°; at 10 in., -2·15°; at 18 in., -2·25°. Feb. 22,—air in shade, -2·25°; in sun, 9·50°; snow ice, 2½ in.; black ice, 21¾ in.: total, 24¼. Temperature of ice at 2 in., 2·15°;[1] at 10 in., 1·00°; at 18 in., 0·65°; at 22 in., 0·90°. The surface of ice in the sun was thawing freely. March 11,—air in shade, -2·20°; in sun, 11·10°; surface of ice thawing in the sun. Thickness of black ice, (there being no snow ice), 22½ in. Temperature, at 2 in., 1·90°;[1] at 10 in., 2·35°;[1] at 18 in., 1·25°.[1]

These observations show that the ice expands without reference to the temperature of the water, and that the temperature of the ice itself, for such thickness as above given, varies considerably, its changes having little reference to the water below. It also appears that the rays of the sun at these depths, (and probably the same would be true of much greater depths,) are absorbed largely by an enclosed object, even of a light color. In the uniform temperature of the water at various depths, there is evidence that the sudden disintegration of the ice, and its disappearance, is not in this instance due to the action of the water. This occurs constantly on the large ponds in the neighborhood, but rarely on the river. It never takes place until the "snow ice" is entirely melted, and is believed to be due to the action of the sun.

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[1] Chips of the ice left in the hole were melted, and thermometer case somewhat moistened.

Gardiner, Maine, March 2, 1865.

ART. III.—*On the Origin and Formation of Prairies;* by LEO LESQUEREUX.

[Concluded from vol. xxxix, p. 327.]

WE now come to the examination of the new theory of Prof. Winchell.[1] A clear abstract is given by the author himself in the following words:

1. The soil of the prairies is of lacustrine formation, as proved by its physical characters, and by the necessary effect of geological changes of level which are generally admitted to have taken place.
2. Lacustrine sediments enclose but few living germs.
3. Diluvial deposits, on the contrary, are found everywhere replete with living germs.
4. The living germs of the diluvial deposits were buried during the glacial epoch.
5. In proportion as the diluvial surface became exposed, the flora of the pre-glacial epoch was reproduced.
6. The vegetation which finally appeared on the drained lacustrine area was extra-limital, and was more likely to be herbaceous than arboreal.

The above quoted conclusions are sustained with ability by various arguments, and merit, therefore, careful consideration.

The *first* is, "*The soil of the prairies is of lacustrine formation,*" etc.—As Prof. Winchell states, in the beginning, that his system is not intended to explain the formation of the low wet prairies, but only that of the high rolling prairies, and as, in remarking on the extent of the ancient lakes, he says that its arms reached into Iowa, we may admit that the high rolling prairies of this State are considered by him as resulting from the same lacustrine action. We might go farther west, and follow the same formation over a surface continually rising to an elevation of 5000 feet.[2] But it is not necessary for our present argument. In Iowa the knolls of the high prairies are 1,500 feet above the Ocean, and as Lake Michigan is only 600 feet above the sea, its waters, to reach the high prairies of Iowa, should have been 900 feet higher than they now are. How would this agree with the actual configuration of our country? Moreover, as the level of the *high* prairies of Southern Illinois is now at least 150 feet lower than that of Lake Michigan, this part of the country, in the supposition of the author, should have been, at the time of the formation of the Iowa prairies, under at least one thousand feet of water. With such an elevation of the lakes as supposed above, all the highlands of Indiana and Ohio should have been also deep under water. For, the highest point of the Ohio canal, at Licking, being 890 feet above the sea, there is still about 500 feet of difference between this point and the level of the Iowa prairies. The whole high country in Ohio and Indiana is nevertheless covered with forest.

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[1] This Journal, [2], xxxviii, 332.

[2] Prof. Whitney in Geol. Report of Iowa, p. 19; for this and some of the following measurements. Those for Ohio are taken from official surveys for canals.

Nobody, indeed, can deny that the whole surface of the prairies, from the lowest to the highest point, has been recently (geologically considered) under water. But to reduce to a mere extension of Lake Michigan, or of the basin of our lakes, an epoch of submergence which has left its traces over the whole extent of our continent, is narrowing the phenomenon of a whole epoch to one of its merely local results. The deposits of this *Champlain epoch* follow the rivers of the valley of the Mississippi to their highest affluents and their sources.[3] I have seen them overlie the Drift along the banks of the Big Waraju river, in the Indian Territory, at an elevation of more than two hundred feet above the bed of the river. This epoch of subsidence has been followed by an epoch of slow upheaval and slow drainage whose action is recognizable in denudations, deepening of channels, moulding of terraces, along the lakes and the rivers, etc.,[4] and in the formation of prairies over immense, nearly horizontal plains, which were necessarily covered with vast sheets of water, lakes, and swamps. If *the wider expanse of our lakes at former times* is understood in that way, it is indeed undeniable. But, as we have already said, these lakes cannot be considered in the phenomenon as causative or primitive agents. And if it is so, all the deposits of that epoch belong to the same *lacustrine* formation, and as all these surface deposits which were not horizontal are generally wooded, and often densely so, we are already authorized to conclude that the so-called *lacustrine* formation has, by its nature, no direct relation to the prairies.

Before passing to another of the statements of the author, I would like to ask if there is not a contradiction in asserting that there was "little difficulty in discovering the true origin of the so-called 'wet prairies' so common in Ohio and Michigan, and now usually termed 'marshes,' 'swales,' 'bogs,' and in proving that the humidity and sourness of the soil were the real causes of the absence of ordinary upland trees from their surface,"[5] and to say, in considering my opinion identified by the author with the former, that "it is so well known that there is no situation so wet but certain trees will flourish in it," (referring here to the soil of swamps.)[6] Is it not also a contradiction to acknowledge that the wet prairies, along the shores of our lakes, are caused by humidity of the soil, and to assert that the high prairies, *under lacustrine influence*, owe their origin to quite another cause? Does the difference of time modify the action of lacustrine influence?

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[3] Dana: Manual of Geology, p. 547.

[4] The Terrace epoch. Dana, ibid., p. 554.

[5] This Journal, [2], xxxviii, 333.

[6] This Journal, ibid., p. 342.

2nd. *Lacustrine sediments inclose but few living germs*, says Prof. Winchell, and, on the contrary, 3d. *Diluvial deposits are found everywhere replete with living germs.*

Nothing is known of the length of time for which the germs of seeds can be preserved in water. Salt water and fresh water appear to have about the same destructive action on seeds. Though a number of fruits and seeds are brought by currents from the shores of America to those of Scotia and Norway, these seeds do not, or only very rarely, germinate, even under careful cultivation.[7] The marine cocoa-nut (*Lodoicea Seychellarum* Lab.) has been carried for ages by currents from the Praslin Islands to the Maldive, and, nevertheless, it has not been naturalized on these last islands, although similar to the former in climate.[8] In fresh water, the only instance known of seeds preserved for a length of time is that quoted by Dureau de la Malle, who says,[9] that seeds of White Birch, (*Betula alba*, L.), preserved their germinative power after twenty years of immersion. Nothing, or very little, is known, either, of the duration of vitality in different kinds of seeds, under various circumstances. Even for the same species, this duration depends on their degree of maturity, and on the substance in which they have been deposited. When inclosed in a perfectly dry, unalterable matter, in carbon, for example, and sheltered against any kind of atmospheric action, the life of seeds may be, apparently, preserved for a long but not for an indefinite period of time. Isolated and apparently marvelous facts, tending to prove the preservation of the vegetative power of some seeds for centuries, have been quoted by authors. But these peculiar cases have not been carefully enough investigated to be considered as conclusive for science; or, after investigation, have been found to be mere fabrications, published for mercenary purposes. Three cases of this kind are quoted by Prof. Winchell in support of his 4th assertion. One regards the seeds of raspberries taken from the stomach of a skeleton, found 30 feet below the surface of the earth. With the body were found "some coins of the Emperor Hadrian, from which we are justified in assuming that these seeds had retained their vitality for 1600 to 1700 years." I will say nothing of that stomach preserved for 1700 years, though, supposing that the story was true, it would indicate a concurrence of peculiar circumstances, so remarkably favorable to the preservation of organic substances, that we should consider the case of the preserved vitality of the raspberry seeds, as quite an exceptional one. But on this subject, Prof. Ad. DeCandolle, who mentions the same fact, and who has studied more closely than any other botanist this interesting question of the preservation of seeds, says: "Prof. Lindley has quoted a species of Rosaceæ (raspberry) whose seeds were found in a human skeleton, which was believed to be some centuries old; but after verification of the fact, the seeds proved to be more recent."[10] DeCandolle says nothing of that *bulbous root* found in the hands of a mummy and which produced a beautiful dahlia, rightly passing the fact, as one of those apocryphal stories with which science had better not be encumbered. Not that the statement of Prof. Lindley is to be doubted; but that this celebrated botanist, too prone to believe stories in confirmation of his opinion, may have been misled by false accounts. We know of old that the merchants of Egyptian mummies are not more conscientious in the preparation of their *goods* for market, than the manufacturers of Italian or of Syrian antiquities. Manuscripts, seeds, rings, every kind of remarkable utensils and bodies, have been wrapped with the old bones of Egyptian mummies, to raise their price. This explains also the third case of long preservation of seeds of wheat from the pyramids, quoted by Prof. Winchell. I have myself cultivated green peas, duly certified as directly coming from the same source and bought at a high price; and learned to my cost that the most common of our varieties of this vegetable was already cultivated at the time of the Pharaohs.

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[7] Alph. De Candolle Geogr. Bot., p. 616. [8] Hooker's Bot. Mag., t. 2734.

[9] Annales des Sciences Nat., v, 373.

But even accrediting to the seeds a vitality of infinite duration, whenever they are imbedded under favorable conditions, wrapped in a hard and perfectly dry body, sheltered against any kind of atmospheric influence, we must inquire if the diurnal or drift deposits may have afforded to them these conditions of preservation.

Two theories only are admissible for explaining the transportation of the drift materials: 1st, Water and floating Icebergs, 2d, Glaciers. In the first case the transport of seeds would have been made in water, and their vitality destroyed, as it has been remarked already. Admitting, with Prof. Winchell, the transportation by glaciers, we are called to suppose or believe that seeds of all the species of trees now living on the north part of our continent, have been at first buried and then carried in the drift, as in a kind of Noah's ark, especially provided for the preservation of vegetables. But the transportation of the materials carried by glaciers is only partly done upon their surface, and does not proceed in a way very favorable to the preservation of seeds. In their forward advance, this surface of the glaciers is continually broken into innumerable fissures of various size, often broad crevasses of great depth, where heaps of transported materials are constantly engulfed, to be, by the movement of the ice, crushed, ground up, and, by and by, thrown back to the surface to be swallowed again. Hence, all the materials transported by glaciers bear the marks of long grinding. The soft stones have been reduced to sand or to mud, the hard ones are rounded into pebbles, and the few which have casually escaped burial have still their angular corners smoothed away, of course. And though the movement is slow, the materials carried on the back of the glaciers (moraines) are continually mixing and in a confused mass, and thus, though their thickness may be great, they are at repeated times exposed to atmospheric action. How then could it be supposed that mere seeds, especially large ones, like acorns, nuts, beech nuts, etc., could have resisted the crushing action of ice, when hard stones have been ground into sand and mud. For, according to the theory, the seeds ought to have been preserved within the transported materials, as it is positively stated that diluvial materials are full of seeds.

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[10] Alph. de Candolle Geogr. Bot., p. 541.

Prof. Winchell says, "The general effect of the events which ushered in and marked the progress of the reign of ice was to destroy the vegetation flourishing over all the northern portion of the continent, and mingle its forms with cubic miles of *debris* detached from the under-lying rocks," etc. What has become of these forms of vegetation, remains of vast destroyed forests which have produced those seeds *with which the diluvial is replete?* Are trunks and branches of trees more difficult to preserve than seeds? Who has found *those trunks and limbs of trees buried* 50 *and* 100 *feet in this diluvial rubbish?*[11] Although the drift has been cut by canals, railroads, etc., over its vast extent, I do not know that any large deposits of vegetable remains have been found in it. There is, near Cleveland, Ohio, a bed of vegetable matter, a compound of leaves and branches of nearly carbonized Conifers heaped in patches, within sandy clay, and which do not appear to have been transported from the place where the plants grew. They look like the scattered remains of an incipient peat-bog. Here, at Columbus, a few pieces of wood have been dug out, at a depth of twenty feet, from a kind of drifted clay not far from the banks of the Scioto river. They may or may not belong to the Drift; in any case they were, when taken out, as soft as clay and decayed throughout. These are the only Drift deposits of vegetable remains which have come under my examination.

And if the diluvial deposits are replete with living germs, how is it that wherever these deposits are deeply cut, and their inmost recesses exposed to view, we do not see this new ground invaded at once by a peculiar kind of vegetation, especially by trees? Along the Ohio canals, the exposed Drift remains absolutely sterile in the first years after its denudation. By and by, some seeds of the plants growing in the neighborhood, those of the Mullen especially, invade this new soil, and become for a time its predominant vegetation. Afterward, a few shrubs, also the most common in the vicinity, appear on it; and it is only long years after, and when the surface of the original ground is already covered by a coat of vegetable mould, that a few trees, the Black Locust, the Elm, etc., are seen here and there growing up among the bushes.

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[11] This Journal, l. c., p. 334.

Prof. Winchell again says: "4th, *The living germs of the diluvial deposits were buried during the glacial epoch;*" and, "5th, *In proportion as the diluvial surface became exposed, the flora of the pre-glacial epoch was reproduced.*

The author supposes that the vegetation which characterized the close of the Tertiary period was probably nearly identical with that existing at the present day under the same climatic conditions.[12] The climate of the Tertiary period at our latitude was evidently warmer than it is now, and, in the supposition of the author, we must expect of course to find the seeds of northern species, brought with the Diluvium and established with us, taking the place of those destroyed by the influence of the glacial period. Considering only the general character of the flora of the Tertiary at different latitudes, it agrees well enough with this idea, and we may give here some details which tend to strengthen the hypothesis, more, perhaps, than any reason advanced by the author himself.

The Tertiary flora of Iceland, at least what is known of it, is more closely related to our present flora than that of the Tertiary of the Mississippi. Among twenty-seven species of Phenogamous plants,[13] it has eight Conifers, one of which, the most common, is an *Araucaria;* two Pines, one of them related to *Pinus serotina* Michx. of ours; five Firs, one also related to our *Abies balsamea* Marsh., or *Abies Fraseri* Pursh. Among the Monocotyledonous plants, this flora of Iceland has a *Sparganium,* a species of the *Naiadaceæ* and the seeds of a sedge; and among the Dicotyledonous it has three species of Birch, one of which is related to our *Betula excelsa* Ait; one Alder, one Willow, one Hazelnut, one Oak, whose nervation resembles that of our *Quercus montana* Willd.; one Buttonwood (*Platanus*), one *Dombeyopsis,* a genus of the Linden family, represented only by broken, scarcely determinable leaves; one Maple, apparently the most predominant species in that Tertiary flora of Iceland, and remarkable by its large leaves and large fruits; one Grape, related to *Vitis vulpina* L. of ours; one Tulip tree, one Buckthorn, one Sumach, and one Walnut. If we consider the genera only, it is evident that this flora has the greatest analogy with ours; and if we could account for the transportation of seeds, it would perhaps seem reasonable to suppose that the change of character of our vegetation, since the Tertiary period, might be explained by Prof. Winchell's theory. Before conceding the value of this supposition, however, we must look further to what we know of the characters of the vegetation in our recent geological ages. The materials collected till now from the flora of Iceland as well as from our own are not sufficient to form a solid basis for argument, but we must use them as they are.

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[12] This Journal, l. c., p. 387.

[13] O. Heer: Flora Tert. Helvet., iii, 117.

In the lower Miocene (may be the Eocene?) of the State of Mississippi, the most abundant remains of fossil plants belong to a species of *Calamopsis*, an extinct genus of Palms. With this, and in about the same abundance, there are leaves of a *Sabal* resembling our Palmetto, and of Fig and Cinnamon trees. At a higher stage, in the red shales, which I consider as true Miocene, we find still some leaves of Fig, Cinnamon, and Sabal, with already those of Oak, Maple, Hickory, Laurel, Olive, Magnolia and even Beech, whose species are related to, but not identical with, species of our time. Higher still, in the chalk banks of Columbus, Kentucky, we have leaves and fruits, whose relation is still nearer to our present species. Some of them so much so, that I have considered them as identical with ours.[14] Passing higher up in the formations, we have, along the banks of the lower Ohio river, thick strata of a compound of leaves and fruits mixed with alluvial clay and formed in terraces. All the species of this formation are of our time and of our latitude.

In the difference of forms between the plants of the lower Tertiary and those of our epoch, we recognize a constant modification of specific characters, but one so uniform and so slow in its progress that we cannot consider it as resulting from any great geological cataclysm. These changes are nevertheless evident; as evident after the glacial epoch as at the different stages of the Tertiary, but no more marked. Therefore, I do not see that we can ascribe the last change to a peculiar glacial agency, especially when, to sustain the hypothesis, we should have now, in our arborescent vegetation, exactly the same species as those of the Northern Tertiary. This is certainly not the case. For out of the twenty-seven species of the Tertiary of Iceland, no one is perfectly identical with ours; four only are related to species of our time, and the most remarkable and prominent, *Araucaria* and *Acer otopterix* Gp., that maple noted for its very large fruits, are types of which no trace appears, in our actual vegetation.

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[14] This Journal, [2], xxvii, 364. Specimens of these fossil plants were sent to Prof. Heer, who denied the identity of some of them with species of our time, and doubted that of others.

Prof. Winchell's next statement is, "6th, *The vegetation which finally appeared on the drained lacustrine area was extra-limital, and was more likely to be herbaceous than arboreal.*

What is an extra-limital vegetation? Prof. Winchell supposes that after the Drift epoch the whole continent passed *under the action of the ocean's surges.* That the highest points of the drift emerging from this general flood were immediately covered with forests, from the seeds preserved in it. That, at the time when this portion of the emerged continent "was again clothed with those forms of verdure which had adorned it at the close of the Tertiary period," "the retreating waters paused to brood over the wide region destined to become the garden of the West; perpetual dilution converted them into a vast inland sea of fresh water, upon whose bottom gathered the lifeless sediments that were to be the soil of the prairies."

A few simple measurements have already contradicted the essential idea in this paragraph. We have quoted it only to show that by *extra-limital* species, the author understands apparently species which either did not belong to the Tertiary, or, did not inhabit the forests bordering the lacustrine prairies, and were brought from somewhere far away. For the vegetation of the prairies is of a type quite different from that of the forests, and cannot have originated from it. How, then, can we account for this extra-limital origin of species and the transportation of seeds across the forests surrounding the lacustrine deposits? Can we suppose that, for hundreds of centuries, the seeds of trees never progressed by dispersion, never trespassed upon that limit marked by the borders of ancient lakes, *though the soil was well adapted for their germination and their growth;*[15] and that the lacustrine soil was invaded by herbaceous plants brought from far away across the forests, we know not whence? It is not necessary to attempt to refute such a supposition. Are the seeds of Maple, Box-Elder, Linden, Hornbeam, Ironwood, Birch, Alder, Willow, Cottonwood, etc., which are either winged or surrounded with down or with hairs to facilitate their dispersion, more difficult to transport by natural known agencies, such as winds, currents, etc., than those of Pentalostemon, Astragalus, Baptisia, and other Leguminosæ so common on the prairies? The vegetation of the prairies has the same general character as that of the swamps and marsh lands. In both there is especially an abundance of large, coarse Compositæ, "plants which continually pump water by their roots from the soil and send it through their leaves into the atmosphere as vapors."[16] These and most of the other species of the prairies have generally deep-creeping or tap conical and tuberous roots, penetrating the soil and thus escaping destruction by conflagration.[17] And with these, there are the sedges, and the hard grasses, like the *Andropogons*, which seek a siliceous soil, and whose tissue is so hardened by silica that their culms are not even consumed by the autumnal fires. These, and indeed many of the species of the high prairies of the Mississippi, are found in our swamps along the canals of Ohio.

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[15] According to Prof. Winchell's opinion. This Journal, [2], xxxviii, 344.

[16] De Candolle Physiol. Veget., p. 1210.

A simple remark on the whole theory will close this examination, already too long. An hypothesis, or a theory, to be acceptable to the mind, should account for all the appearances of the phenomenon which it proposes to explain; and its explanations should be sustained by what we know of natural laws still in activity, and by action so evident that its effects cannot be denied. Neither of these conditions is fulfilled, I think, by the new theory. It takes into consideration a very small part of the whole system of prairies, explaining neither the low lacustrine nor the fluvial prairies, neither those of the sea nor those of the mountains, etc. And it refuses to acknowledge an evident operation constantly at work under our eyes, the result of a simple law of nature.

ART. IV.—*Preliminary notice of a small collection of Fossils found by Dr. Hays, on the west shore of Kennedy Channel, at the highest northern localities ever explored;* by F. B. MEEK.

SOME time after Dr. Hays' return from his Arctic expedition, he sent on to the Smithsonian Institution several boxes of mineral and rock specimens collected by him while in the north, to be examined by Prof. Thomas Egleston. On opening these, Prof. Egleston noticed, amongst other specimens, a mass of gray limestone containing a few fossils, to which he called the attention of the writer. Finding these to be of much interest, considering the distant northern locality from which they were obtained, the other specimens were then carefully examined, and fragments of a few other fossils found amongst them. When Dr. Hays subsequently visited Washington, he stated that the best specimens of fossils collected by him were then in the possession of a friend at Philadelphia, and that those we had seen were merely fragments that had been packed up with the rock specimens. At the request of Dr. Hays, the writer agreed to examine and report upon these fossils, so soon as the other specimens could be sent on from Philadelphia. After the lapse of five or six months, however, without their arrival, inquiries were made in regard to them, when it was ascertained from Dr. Hays that his friend, with whom he had left the specimens, had sent them on some time previous. Unfortunately, however, up to this time they have not been received, and, as it is quite probable they may never be recovered, it has been thought desirable in the interests of science, as well as in justice to the intrepid Arctic explorer, Dr. Hays, that such conclusions as can be deduced from the meager collection of imperfect fossils found amongst the rock specimens collected by him, should be placed on record.

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[17] Fire extends sometimes its ravages beyond the natural limits of the prairies and destroys the forests of the borders, where the trees are not only scattered but of feeble growth. It is only on this intermediate ground that the contest of progression and receding of the forest is in constant activity.

Before expressing an opinion, however, in regard to the age of the rock from which these specimens were obtained, the following list of them, with brief descriptions of some of those believed to be new to science, are given:[1]

1. ZAPHRENTIS HAYSII, Meek.

Corallum obconical, distinctly curved, rapidly expanding from a pointed base; length, about two inches; breadth, near the summit, 1·40 inches; sometimes showing, on the convex side, two broad, distant, shallow, longitudinal furrows, extending the whole length, so as to give that side a trilobate appearance. Epitheca, thick, and, where not worn, concealing the septa within; surface showing small wrinkles of growth, which are most distinct near the summit. Calice, apparently rather deep, (filled with stony matter in all the specimens examined); principal radial septa about sixty, rather stout and rigid, as seen around the margins of the calice, where about ten of them may be counted in a space of half an inch; alternating with these there is a shorter and weaker secondary series.

The trilobate appearance of the outer or convex side in the type of this species, together with its small wrinkles of growth, give it much the aspect of some of the merely arched species of *Platyceras*, for which it might be mistaken, when the calice is filled with stony matter. As some of the other specimens, however, apparently not differing in other respects, do not present this trilobate appearance, it may not be constant.

The specific name of this coral is given in honor of Dr. Hays, its discoverer.

Locality, Cape Frazier. Between lat. 80° and 81° N., long. 70° W.

2. SYRINGOPORA, (*sp. undt.*)

A mere fragment. The tubes are crowded so as to be nearly always less than their own breadth apart, and sometimes nearly in contact. They are uniformly 0·10 inch in diameter, and apparently nearly straight and parallel, while the connecting tubes are small.

Locality, Leidy. Between lat. 80° and 81°, long. 70° W.

3. FAVOSITES, (*sp. undt.*)

A small flat fragment showing regular hexagonal calices 0·10 inch in diameter. Tabulæ apparently thin and closely arranged; mural pores consisting (as seen on one wall only) of four or five alternating series.

Locality, same as last.

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[1] It is the intention of the writer, when more at leisure, to prepare drawings and fuller descriptions of these fossils, as well as of the others now lost, should they be recovered, for publication in a work Dr. Hays has in progress on the results of his expedition.

4. STROPHOMENA RHOMBOIDALIS, Wahlb. (=*Leptaena depressa* of authors). Presenting its usual characters.

5. STROPHODONTA HEADLEYANA, Hall?

The specimen of this shell consists of about the half of a ventral valve, embedded in the matrix so as to show the inner side with its coarse irregular striate, subcordate visceral cavity, and granulose surface. So far as can be determined from this, it agrees well with the New York species.

Locality, same as last.

7. STROPHODONTA BECKII, Hall?

The specimen referred with doubt to the above species, is imperfect, but presents the same general outline, and flatness, as well as the characteristic small curving concentric wrinkles, fine striæ, and even traces of the flabelliform visceral scar, of the New York shell.

Locality, same as last.

8. RHYNCHONELLA, (*sp. undet.*)

A single specimen of a ventral valve partly embedded in the matrix. Breadth, 0·42 inch; length, 0·36 inch. Mesial sinus broad and shallow; surface with only twenty small radiating costæ, five of which occupy the mesial sinus.

Locality, same as last.

9. CŒLOSPIRA CONCAVA, Hall.

Several specimens, showing both sides, agree well with the New York species.

Locality, same as foregoing.

10. SPIRIFER, (*sp. undet.*)

Specimens, partly embedded, closely resemble *S. perlamellosus*, Hall, of New York Catskill Shaly limestone.

Locality, same as last.

11. LOXONEMA? KANEI, Meek.

An internal cast, from which it is not possible to determine, beyond doubt, whether it is a *Loxonema*, or a *Murchisonia*. Length, about 2·07 inches; breadth, 0·75; apical angle, 20°. Form conoid-subfusiform; consisting of about six convex whorls, separated by a distinct suture. Aperture subovate; last turn comparatively rather large; surface unknown.

Named in honor of Dr. Kane, the Arctic explorer.

Locality, Cape Frazier; between lat. 80° and 81°, long. 70° W.

12. ORTHOCERAS (*undetermined*).

The specimen is too imperfect for identification with any known species, or be characterized as new. It is incomplete at both extremities, and partly embedded in a mass of limestone. Entire length of the fragment, 2 inches; section circular, at the larger end, 0·43 inch in diameter; at smaller end, 0·15 inch. Septate throughout; septa numbering five in the space of 0·27 inch at larger end. Siphon and surface unknown.

Locality, same as last.

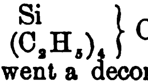
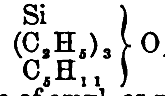
13. ILLÆNUS, (*sp. undet.*)

Fragments of the glabella, and a movable cheek, apparently of a species of this genus.

From the foregoing list, it is believed that geologists will agree, that the rocks at this highest northern locality at which fossils have ever been collected, belong to the Upper Silurian era. The most remarkable fact, however, is, that they are nearly all very closely allied to, and some of them apparently in all respects undistinguishable from, species found in the Catskill Shaly limestone of the New York Lower Helderberg Group. Indeed, these shells resemble so closely Shaly limestone forms, that we could scarcely dismiss the impression that the mass of limestone in which they are imbedded might be a New York specimen that had in some way been accidentally mixed with those from the north after Dr. Hays's return, until assured by him that he distinctly remembered collecting it at the Arctic locality.

We are aware that great caution is necessary in pronouncing upon the identity of fossils found at localities separated by forty degrees of latitude, without a good series of specimens for comparison; but, whether or not the absolute specific identity of any of these Arctic shells with New York species be admitted, in their unquestionably close affinities to the New York forms alluded to, they certainly present another striking evidence of the apparent wonderful uniformity of climatic and other physical conditions during these early periods of our earth's history, over the whole globe.

ART. V.—*On the Replacement of one Alcoholic Radical by another in compounds of the Ether Class;* by C. FRIEDEL and J. M. CRAFTS.

WHILE engaged in the study of the ethers of silicic acid, we noticed that the normal silicate of ethyl,[1]  $\left.\begin{matrix}\mathrm{Si}\\(C_2H_5)_4\end{matrix}\right\}O_4$, when it was heated with amylic alcohol, underwent a decomposition similar to that which would have been effected by an alkaline base; ethylic alcohol was set free, and ethers containing the radical amyl were formed; we also observed a decomposition, at the temperature of its boiling point, of a mixed silicate of ethyl and amyl,  $\left.\begin{matrix}\mathrm{Si}\\(C_2H_5)_3\\C_5H_{11}\end{matrix}\right\}O_4$, with formation of silicate of ethyl and silicate of amyl, as well as of the mixed ethers intermediate to these two. We considered it of interest to determine the conditions under which reactions of this nature take place, especially as they have much analogy with those which most often occur in inorganic chemistry, namely, the action of a base upon a salt, and in general the replacement of one element by another, and there was reason to hope that the study of the reactions in which these organic radicals were concerned might throw some light upon those in which their types, the inorganic elements, play a part.

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[1] The atomic weights used in this paper are $H=1$, $O=16$, $C=12$, $Si=28$.

In the present research we have confined ourselves to the study of a few compounds, which may be taken as representatives of others which are analogous, and have endeavored to solve by experiment the following problems.

1. Is the replacement of the alcoholic radicals by each other one that takes place readily in all the combinations into which they enter; or is it peculiar to their combinations with acids?

2. Is one alcoholic radical to be considered as playing the part of a stronger base than another, and as displacing it from its combination by virtue of a stronger affinity for the body with which it was combined? Or, on the contrary, is the change of composition, produced by heating a mixture of compounds of several radicals, due to the tendency of each one to enter into all the combinations possible to it?

In order to answer the first question, we have examined—1st, the action of alcohols on the ethers of various acids; 2d, the action of these ethers on each other: and 3d, the action on each other of the ethers, properly so called. The second question has been, at least partially, answered by conducting two series of experiments, in which the conditions of the reaction have been reversed; as for example, first by the study of the action of amylic alcohol on the acetate of ethyl, and then that of ethylic alcohol on the acetate of amyl. Great care has been taken to ascertain the purity of the preparations employed, and particularly that the alcohols were anhydrous.[2]

Action of amylic alcohol on acetate of ethyl.—A mixture of equal parts, by weight, of the two bodies was sealed in a stout glass tube by means of the blowpipe lamp, and heated during forty-eight hours at 240°–260° C. Common alcohol was set free, and could easily be isolated and recognized in the portion of the contents of the tube which distilled at the lowest temperature. The portion which distilled last contained acetate of amyl, together with the excess of amylic alcohol employed. To separate the two, advantage was taken of the property of amylic alcohol to unite readily with an excess of sulphuric acid to form amyl-sulphuric acid soluble in water, while acetate of amyl is dissolved, undecomposed by the acid, and is precipitated by diluting the solution with water. [If care is taken not to allow the temperature of a mixture of amylic alcohol, with several times its weight of concentrated sulphuric acid, to rise above 60°–70° C., the product is entirely soluble in water. At a higher temperature insoluble products, sulphate of amyl and amylic ether, are formed.] The portion, therefore, of the contents of the tube, whose boiling point was above 125° C., was treated with sulphuric acid, and the acetate of amyl, precipitated by the addition of water, was again treated with sulphuric acid, and again precipitated by water. This operation was repeated three times. In this way acetate of amyl was obtained, distilling 136°–138°, which contained:

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[2] The best and most economical method of obtaining an anhydrous alcohol is, after previous rectification over quick lime, to distil it with a quantity of sodium sufficient to destroy the small amount of water still retained by it. The excess of sodium employed forms an alcoholate of sodium, which is not decomposed at the point of ebullition of the alcohol.

		Theory.
C =	64·71	64·62
H =	10·93	10·77

The quantity of acetate of amyl formed was considerable, but still a certain portion of the acetate of ethyl remained unchanged, although amylic alcohol had been employed in excess.

Action of common alcohol on acetate of amyl.[1]—Equal parts of each were heated forty hours at 240° C. The liquid which distilled at 75°–90° C., was washed eight times with a saturated solution of common salt, to free it from alcohol, and dried over chlorid of calcium. After being thus purified, it distilled at 74°–76° C., and had all the properties of acetate of ethyl. An analysis gave:

		Theory.
C =	53·81	54·54
H =	9·50	9·09

Thus acetate of ethyl was formed in the same way from a mixture of acetate of amyl and common alcohol, that acetate of amyl was from a mixture of acetate of ethyl and amylic alcohol.

Action of amylic alcohol on benzoate of ethyl.—Equal parts of each, heated sixty hours at 210°–240° C., gave a small quantity of an ether, distilling at 251°–253°, which answered in all its properties to benzoate of amyl. It contained:

		Theory.
C =	74·47	75·00
H =	8·25	8·33

We conclude from this and another experiment, which was

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[1] The acetate of amyl, obtained at different times, always distilled at 136°–138° C. The boiling point, given in Gerhardt, about 125°, and that determined by Kopp, 133°, differ so much from that observed by us that we thought it necessary to analyze the product used in these experiments. A portion, prepared by treating one part amylic alcohol with one part crystallized acetic acid and two parts concentrated sulphuric acid, at a temperature below 70° C., after having been thoroughly washed with water, distilled at 127°–136°, and contained:

		Theory.
C =	64·81	64·62
H =	11·06	10·77

The amylic alcohol used in the preparation distilled at 130°–131°, and contained:

		Theory.
C =	68·02	68·18
H =	13·51	13·64

made at a somewhat lower temperature, and where only traces of benzoate of amyl were produced, that the benzoate of ethyl is decomposed by an alcohol with more difficulty than the acetic ethers. It will be remembered that this body is also less easily decomposed by water and alkaline bases than they; the reaction, however, takes place, and the extent of the decomposition effected is without doubt only a question of time and temperature.

We next studied the action of an alcohol on the ether of a bibasic acid, a reaction which is capable, evidently, of giving rise to a greater number of products than those which were formed in the preceding cases; for, by the successive replacement, by another radical, of each equivalent of the alcoholic radical combined with the acid, first a mixed ether, and then an ether containing two equivalents of the other radical would be formed. Oxalic ether, which is easily decomposed by water and alkalies, is well adapted to the study of this reaction.

Action of amylic alcohol on oxalate of ethyl.—One part amylic alcohol and two parts oxalic ether were heated thirty-six hours at 220°–250° C. On opening the tube, carbonic acid and carbonic oxyd, unmixed with any hydrocarbon, were given off, and ordinary ether was found in the most volatile part of the liquid; thus a small part of the oxalate of ethyl had been decomposed by heat into carbonic acid, carbonic oxyd and ether; but the contents of the tube were not acid, and this decomposition could have had no influence on the reaction we were studying. After the liquid, taken from the tube, had undergone several fractionated distillations, the part which passed below 90° C. consisted of a mixture of ordinary ether and alcohol. The alcohol had evidently been set free from the oxalic ether by the action of amylic alcohol. It was isolated from the common ether, with which it was mixed, by treating the mixture with an aqueous solution of salt, and distilling to regain the alcohol which was dissolved in the saline solution. The alcohol, distilled, was treated again several times in the same way. After having been rectified over anhydrous baryta, it boiled at 78°, and presented the characteristic properties of ordinary alcohol.

The portion of the liquid boiling at a temperature higher than 90° was separated by fractionated distillation into four products: amylic alcohol, oxalate of ethyl, distilling near 180°, oxalate of ethyl and amyl, distilling at 225°–233°, and oxalate of amyl, distilling at 259°–261°. An analysis of the latter gave:

		Theory.
C =	62·24	62·61
H =	9·49	9·56

The mixed ether, $\left.\begin{matrix}C_2O_2\\C_2H_5\\C_5H_{11}\end{matrix}\right\}O_2$, boiling at 225°–233°, which was obtained in a state of approximate purity, could not be farther purified by repeated distillations, but on the contrary, when distilled by itself, the limits of temperature within which it passed became wider with each operation. We suppose this circumstance to arise from a decomposition of the body by heat, and our supposition was verified by the result of an experiment, where we heated a portion of this product, which had distilled at 230°–242°, during twenty-four hours, at 220°–250° C., in a sealed tube, and found that the decomposition had advanced so far that oxalate of ethyl (boiling at 180°), and oxalate of amyl (boiling at 260°), could be obtained from it. We shall publish the details of experiments with the ethers of a quadribasic acid in a paper on silicic ethers.

It appears from the experiment with the mixed oxalate of ethyl and amyl, that, when two alcoholic radicals are combined with a single bibasic acid, they have a tendency, at a high temperature, to displace each other, so that, besides the portion of the mixed ether which remains unchanged, compounds of the acid with two equivalents of each of the radicals are formed. When two ethers containing different acids and different alcoholic radicals are subjected together to the action of heat, a decomposition might be expected with production of two new ethers, each acid having entered into combination with each alcohol. This reaction actually takes place.

Action of benzoate of ethyl on acetate of amyl.—The two ethers were heated several hours at 300°–310° C., and the acetate of ethyl and benzoate of amyl formed were isolated and analyzed.

Acetate of ethyl, boiling point about 74°:

		Theory.
C =	55·84	54·54
H =	10·09	9·09

Benzoate of amyl, boiling point about 250°:

		Theory.
C =	74·06	75·00
H =	8·14	8·33

In another experiment, when the tube was heated sixty hours at 200°–240°, the quantity of these ethers formed was too small to admit of their being separated from the other products. A very high temperature or a great length of time therefore is necessary to the accomplishment of this reaction. The ethers properly so called, the oxyds of alcoholic radicals, do not undergo a similar decomposition except perhaps at a very high temperature.

Common ether and amylic ether were heated seventy-five hours at 200°–250° without formation of a mixed ether.

The presence of water in the products employed would have taken away entirely from the value of the preceding experiments, for the water would have acted upon the ether, setting free an equivalent of acid, which would have combined with the alcohol, setting water free again, so that the transfer of the acid from one alcoholic radical to another might have been wholly independent of the reaction we were studying; each of the tubes, after having been heated, was therefore examined with very sensitive litmus paper, which was left in the liquid several minutes. In no case was there a trace of acid reaction observed.

If we refer to these experiments for an answer to the questions proposed at the beginning of the memoir, we find: 1st. That the reaction which we have considered takes place, at a temperature in the neighborhood of 250° C., between an alcohol and the ether of an acid, or between two ethers of acids, and also, that a similar decomposition takes place when the mixed ether of a polybasic acid is heated by itself. That it does not take place at all, or only with great difficulty, when the ethers (oxyds) of alcoholic radicals are heated with one another. That the reaction takes place most readily with those ethers that are most easily decomposed by water and other reagents. 2d. That the character of the reaction indicates that it is not dependent upon *elective* affinity, but simply on a tendency of each radical to unite with all the others present, and thus to form the greatest number of compounds possible.

It has long been a point, upon which the opinion of chemists has been undecided, whether two salts in solution undergo mutual decomposition, so that a portion of each acid combines with a portion of each base, or whether the salts remain in their primitive condition. Late researches[4] seem to render probable the first view of the subject, so that, for example, a solution of nitrate of potash and sulphate of magnesia would be found to contain the sulphates of potash and magnesia and the nitrates of these bases if there were any means of directly separating all these salts; the separation is however impossible, and the state of combination of each element in the solution must be deduced from an examination of its physical properties. With many compounds of organic chemistry, however, when it is sought to separate products formed under similar circumstances, and belonging to the same class as salts in inorganic chemistry, advantage may be taken of the differences in the boiling points of the products to isolate each one by fractionated distillation; and also, a peculiar and characteristic property of organic bodies frequently aids in rendering a separation of this nature easy, namely, the slowness with which they experience intermolecular changes. Thus, the mixed oxalate of ethyl and amyl can be obtained, by a single distillation, in a state of approximative purity, from the products of the reaction of oxalate of amyl on oxalate of ethyl, although, when it is isolated and heated alone for *a certain length of time*, it is resolved in part into the bodies from which it was formed. It is this last mentioned property of compounds of the ether class together with their volatility, which render them peculiarly fitted as material for the study of reactions like those which we have been considering; and the results, obtained by such a study, are not without weight in questions concerning inorganic compounds, for the analogies between the elementary bodies and the organic radicals, which play the same part as elements in a large number of reactions, are becoming every day more numerous with the advance of chemical science, and the barrier which separates the two branches of chemistry is now nothing more than a line of division in an artificial classification, which, notwithstanding its many advantages, has the demerit of separating too widely phenomena which are dependant on the same general laws.

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[4] Gladstone, Journ. Chem. Soc., xv, 302.

ART. VI.—*On Etherification;* by C. FRIEDEL and J. M. CRAFTS.

WITH the purpose of extending our observations on the class of reactions described in the preceding paper, we undertook similar experiments with the chlorids and iodids of alcoholic radicals; but we soon recognized that, although these bodies are easily decomposed by an alcohol at a comparatively low temperature, the reaction is quite different from the one before described.

Two tubes were heated twenty-four hours at 160°–180° C. The first contained iodid of amyl with one-half its weight of common alcohol; the second contained iodid of ethyl with an equal weight of amylic alcohol. The contents of the tubes became strongly acid, and a layer of water, containing iodhydric acid in solution, separated on the surface of the liquid in each of them.

The liquid was washed with water to free it from iodhydric acid, and, as the iodids are not acted upon by concentrated sulphuric acid, this agent was employed to separate them from the other products. The iodids, separated by decantation from the sulphuric acid, underwent several fractionated distillations. The contents of the first tube gave a small quantity of a liquid, boiling at 72°–75°, which possessed the properties of iodid of ethyl [true boiling point =72°·2], and from the second tube iodid of amyl, boiling at 146° [true boiling point =146°], was obtained. Analyses were made of these products, which corresponded sufficiently with the true composition to leave no doubt as to the identity of the bodies. The quantity of these iodids which was obtained was, however, comparatively small; in each of the tubes the principal portion of the reaction was an ether, which was regained from the sulphuric acid employed in purifying the iodids, by the addition of water.

The ether obtained in this manner was not pure, being mixed with a small quantity of the iodids, and with traces of the alcohols, but it distilled in greater part at 100°–120°, and after a few fractionated distillations a large quantity of a product whose boiling point was near 110° was obtained. In order to rid this product of the iodids with which it was mixed, it was heated some time with sodium; the iodids were destroyed with formation of iodid of sodium and the radicals ethyl and amyl. From these latter the ether was freed by repeated fractionated distillation as far as possible, but the analyses below show that the body was not perfectly pure; the traces of these hydrocarbons which it contained was not, however, sufficient to leave any doubt as to its identity with the mixed oxyd of ethyl and amyl, $\left.\begin{matrix}C_2H_5\\C_5H_{11}\end{matrix}\right\}O$. It distilled at 110°–113° (true boiling point =112°).

Analysis I is of the product obtained from the tube containing iodid of amyl and common alcohol; analysis II, of that obtained from iodid of ethyl and amylic alcohol.

		I.	II.	Theory.
C	=	73·53	72·55	72·41
H	=	14·30	14·22	13·79

The ether could only have contained a trace of an alcohol, but, as the action of sodium of a mixture of an alcohol with the iodid of the radical of another alcohol gives rise to a mixed ether, we repeated the experiment, taking care to destroy the alcohol more completely by repeated treatments with concentrated sulphuric acid before acting upon the iodid with sodium, but we did not observe that the quantity of mixed ether formed was diminished after this precaution had been taken.

In the foregoing experiments common ether as well as amylic ether were formed, but in much smaller quantity than the mixed ether. The principal reaction which took place in the tubes is therefore expressed by the equation:

$$\left.\begin{matrix}R\\H\end{matrix}\right\}\Theta \;+\; R'I \;=\; \left.\begin{matrix}R\\R'\end{matrix}\right\}O \;+\; HI.$$

The iodhydric acid in presence of an excess of alcohol would give rise to the formation of water and of the iodid of the alcoholic radical,

$$\left.\begin{matrix}R\\H\end{matrix}\right\}O \;+\; HI \;=\; RI \;+\; H_2O,$$

and the iodid would in this manner be continually decomposed and reformed as long as any alcohol remained. The iodhydric acid would also decompose a certain portion of the ether formed, in the same manner as it decomposes an alcohol, but less readily.

It is not necessary to suppose that the decomposition and recomposition of the iodid are dependent upon successive reactions; it is on the contrary certain, that at any given moment all the above mentioned reactions take place simultaneously, and that the mass of each body entering into them at that moment determines the relative amount of their products.

The fact that the principal product of the reaction of the iodid of an alcoholic radical upon the alcohol of another radical is a mixed ether can only be interpreted by the first of the preceding equations, and this manner of viewing the reaction is of some importance in its bearing on the theory of etherification, as affording evidence in support of a theory, already advanced by Reynoso[1] and others, which refers to a simple chemical reaction, the etherification by so-called catalytic action of chlorids, bromids, and iodids on alcohols. We have been able to demonstrate in the preceding experiments, that the first stage of the reaction, the replacement of the hydrogen of the alcohol by the radical combined with the iodid, actually takes place in a manner which accords with the theory of Reynoso. The property, which the chlorids, bromids and iodids of alcoholic radicals, as well as of metals, possess, of transforming an unlimited quantity of an alcohol into an ether is comparable to that of ferments, in so far that a change of composition is wrought in a large quantity of matter by a body, which seems in itself to be inactive, and the explanation of phenomena of this class was sought in a mysterious influence, exerted by the presence of the body, which brought about a change without seeming to suffer one itself; but the remarkable researches of Pasteur have led to the discovery of an explanation of the process of fermentation in the physiological action exercised by the fermenting matter on one another; the etherification of alcohols by bodies which are not destroyed at the end of the process, also, admits of a simple explanation, founded on known chemical laws, and there is reason to hope that the name catalysis will eventually be banished from scientific language by a closer study of the reactions, which have been designated by this term in default of a knowledge of their true character.

These researches have been made in the laboratory of Professor Wurtz.

Paris, March 1st, 1865.

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[1] Ann. de Chim. et Phys., 1856.

ART. VII.—*Contributions to the Chemistry of Natural Waters;* by T. STERRY HUNT, A.M., F.R.S.: of the Geological Survey of Canada.

II.

Analyses of various Natural Waters.

CONTENTS OF SECTIONS.—35, mode of analysis, date of collection; 36, waters of the first class; 37, their probable origin, the elimination of sulphates; 38, separation of lime-salts from waters; 39, earthy chlorids in saliferous formations; brines of New York, Michigan, and England; foot-note on errors in water-analyses; 40, brines of western Pennsylvania; waters in which chlorid of calcium predominates; 41, origin of such waters; separation of magnesia as an insoluble silicate; 42, waters of the second class; 43, waters of the third class; 44, waters of the fourth class; Chambly; 45, other waters of the same class; Ottawa River; 46, waters of Highgate and Alburg; 47, changes in the Caledonian waters; comparative analyses; 48, waters of the fifth class; sulphuric-acid springs of New York and Canada; 49, changes in composition, action on calcareous strata; 50, waters of the sixth class, their various sources; 51, neutral sulphated waters.

§ 35. The analyses of the various mineral waters to be given in the second part of the present paper, were made according to the modes laid down in the treatise of Fresenius on Quantitative Analysis. The carbonate of soda in the alkaline waters was determined by the excess of the alkaline bases over the chlorine and sulphuric acid present. This was generally controlled by the amount of the carbonate of baryta thrown down from a solution of chlorid of barium by a solution of the soluble salts obtained by the evaporation of the mineral water; and in some cases, to be specified farther on, this latter process was relied on as the only means of determining the amount of carbonate of soda. For remarks on the earthy carbonates of the waters, and their relation to the results of analysis, see Part III of this paper.

The date at which the various waters were collected for analysis is in each case appended to the notice of the spring. This is of the greater importance, inasmuch as it will be shown that, in the course of years, some of the springs here described have suffered considerable changes in their composition.

§ 36. In the following table are given the analyses of several waters belonging to the first class as defined in § 34.[1]

1. This water is from a well thirty feet in depth, near the village of Ancaster, on the western shore of Lake Ontario. It is sunk in the Niagara formation; but like the other waters of this class, probably has its source in the Lower Silurian limestones. The water rises nearly to the surface, but there is no perceptible discharge. Its temperature was found to be 48° F. when collected for analysis in September, 1847.

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[1] Of the thirty-seven analyses of waters here given, ten have already appeared in this Journal, [2], viii, ix, xi; but for the purposes of comparison it is thought well to reproduce them in the present connection. Of the others the greater part have appeared in the *Geology of Canada*, but several are now for the first time in print.

TABLE I.—WATERS OF THE FIRST CLASS.

	1.	2.	3.	4.	5.	6.	7.	8.
Chlorid of sodium...	17·8280	18·9158	38·7815	17·4000	29·8034	19·94	29·864	7·227
" potassium...	·0920	*traces.*	*traces.*	undet.	·3555	und't.	undet.	undet.
" calcium.....	12·8027	17·5315	15·9230	9·2050	14·8544	6·49	12·439	2·102
" magnesium..	5·0787	9·5437	12·9060	9·4843	3·3977	1·95	7· 33	1·763
Bromid of sodium...	·1178	·2482	·4685	undet.	undet.	und't.	undet.	undet.
Iodid of sodium.....	......	·0008	·0183	"	·0042	....	"	"
Sulphate of lime....	·7767	......	......	......	2·1923	1·77	·954	2·388
Carbonate of lime...	traces.	·0411	......	......	......	....	·370	·400
" magnesia.	......	·0227	......	......	......	....	1·287	....
" { baryta & strontia .	......	undet.	......	......	......	....	....	....
In 1000 parts	36·6909	46·3038	68·0423	36·0893	50·6075	30·15	52·247	13·880

2. This water is from a copious spring which issues from the limestones of the Trenton group at Whitby, on the north shore of Lake Ontario. It contained small portions of baryta and strontia, and was collected in October, 1853.

3, 4. Several wells have been sunk in the Trenton limestone in the township of Hallowell, on the Bay of Quinté, Lake Ontario, in search of brine for salt-making, and have yielded bitter saline waters, of which the two here noticed are examples. No. 3 was obtained from a well twenty-seven feet deep, in October, 1853. No. 4 was taken in the summer of 1854 from a well a mile or two distant from the last. Neither of these waters was examined for baryta or strontia.

5, 6. At St. Catherines, near Niagara Falls, a boring of five inches in diameter was carried to a depth of about 500 feet, and, after traversing the Medina formation, is said to have penetrated fifty or sixty feet into the Hudson River shales. It yields about twenty gallons a minute of a saline water, whose analysis by Professor Croft of the University of Toronto, a few years since, afforded the results given under 5. This water, which was first sought for the manufacture of salt, is now much used for medicinal purposes. Its strength seems subject to some variation, since a specimen from the same well in December, 1861, gave me, by a partial analysis, chlorid of sodium 23·00, chlorid of calcium 9·66, chlorid of magnesium 2·40, sulphate of lime 1·75=36·81 parts in 1000. No. 6, examined at the same time, is from a second well sunk in 1861, not far from the last.

7, 8. These are analyses of the waters from two borings in the Trenton limestone at Morton's distillery in Kingston. The analyses are by Dr. Williamson of Queen's College in that city, and were made probably ten or twelve years since. They have been recalculated so as to represent the whole of the sulphuric acid as combined with calcium. The first of these waters gave to Dr. Williamson both bromine and iodine, and the second was found to be sulphurous. These waters differ from the preceding in containing considerable amounts of earthy carbonates, and in this respect are related to those of the second class, while they still show a large predominance of earthy chlorids.

§ 37. The waters of the above table contain, besides chlorid of sodium and a little chlorid of potassium, large quantities of the chlorids of calcium and magnesium, amounting together, in several cases, to more than one half the solid contents of the water. Sulphates either are absent, or occur only in small quantities, and the same is true of earthy carbonates. Salts of baryta and strontia are sometimes present, while the proportions of bromids and iodids, though variable, are often considerable.

In the large amount of magnesian chlorid which they contain these waters resemble the bittern or mother-liquor which remains after the greater part of the chlorid of sodium has been removed from sea-water by evaporation. The bitterns from modern seas, however, differ in the presence of sulphates, and in containing, when sufficiently concentrated, only traces of lime. The reason of this, as already pointed out in § 22, is to be found in the fact that in the waters of the present ocean the sulphates are much more than equivalent to the lime, so that this base separates during evaporation as gypsum.[1] But, as shown in § 23 and § 24, the waters of the ancient seas, which held in the form of the chlorid of calcium the greater part of the lime since deposited as carbonate, must have yielded by evaporation bitterns containing a large proportion of chlorid of calcium. Such is the nature of the brines whose analyses are given in the above table, and such we suppose to have been their origin. The complete absence of sulphates from many of these waters points to the separation of large quantities of earthy sulphates in the Lower Silurian strata from which these saline springs issue; and the presence in many of the dolomitic beds of the Calciferous sand-rock of abundantly disseminated small masses of gypsum, is an evidence of the elimination of the sulphates by evaporation. The frequent occurrence of crystalline masses of sulphate of strontian in the Chazy and Black River limestones of this region, is also to be noted as another means by which the sulphates were separated from the waters of the Lower Silurian seas. From the proportions of chlorid of sodium, varying from about one-third to more than two-thirds of the solid contents of the above waters, it is apparent that in most cases the process of evaporation had gone so far as to separate a part of the common salt; and thus successive strata of this ancient saliferous formation must be impregnated with solid or dissolved salts of unlike composition. The mingling of these in varying proportions affords the only apparent explanation of the differences in

[1] See farther on this point, Bischof, Chem. Geology, i, 413.

the relative amounts of the several chlorids in waters from the same region, and even from adjacent sources. These differences are seen on comparing the waters from the different wells of St. Catherines, Hallowell and Kingston, with each other.

§ 38. The great solubility of chlorid of calcium renders it difficult to suppose its separation from the mother-liquors so as to be deposited in a solid state in the strata. The same remark applies to chlorid of magnesium. It is, however, to be remarked that the double chlorid of potassium and magnesium (carnallite) is decomposed by deliquesence into solid chlorid of potassium and a solution of chlorid of magnesium; and thus strata like those which at Stassfurth contain large quantities of carnallite, (§ 22,) might give rise to solutions of magnesian chlorid. This, however, would require the presence of a large amount of chlorid of potassium in the early seas. It will be observed, by referring to the analyses above given, that the chlorid of magnesium sometimes surpasses in amount the chlorid of calcium, and sometimes, on the contrary, is equal to only one-half or one-fourth of the latter salt. While it is not impossible that the predominance of the magnesian chlorid in some waters may be traced to the decomposition of carnallite, it is undoubtedly in most cases connected with the action of solutions of carbonate of soda, the effect of which, as already pointed out, is to first separate the soluble lime-salt as carbonate, leaving to a subsequent stage the magnesia chlorid. (§ 18.) As this reaction replaces the lime-salts by chlorid of sodium, it might be expected that there would be an increase in the amount of the latter salt in the water wherever the magnesian chlorid predominates, did we not remember that evaporation separates it from the water in a solid form; and that the two processes, one of which replaces the chlorid of calcium by chlorid of sodium, while the other eliminates the latter salt from the solution, might have been going on simultaneously or alternately. As the nature of the waters now under consideration shows that the process of evaporation had been carried so far as to separate the sulphate in the form of gypsum, and probably also a portion of the chlorid of sodium in a solid state, it is evident that we have not yet the data necessary for determining the composition of the water of the Lower Silurian ocean, as regards the proportions of the sodium, calcium and magnesium which it held in solution; and we can only conclude, from these mother-liquors, that the amount of the earthy bases was relatively very large.

§ 39. As already remarked in § 22, the mother-liquor from modern sea-water contains no chlorid of calcium, but, on the contrary, large quantities of sulphate of magnesia; the lime in the modern ocean being less than one-half that required to combine with the sulphate present. If, however, we examine the numerous analyses of rock-salt and of brines from various saliferous formations, we shall find that chlorid of calcium is very frequently present in both of them; thus supporting the conclusions already announced in § 24 with regard to the composition of the seas of former geological periods. The oldest saliferous formation which has been hitherto investigated is the Onondaga salt-group of the New York geologists, which belongs to the upper part of the Silurian series, and supplies the almost saturated brines of Syracuse and Salina in New York. These, notwithstanding their great purity, contain small proportions of chlorids of calcium and magnesium, as shown by the analyses of Beck, and the recent and careful examinations of Goessmann. In the brines of that region the solid matters are equal to from 14·3 to 16·7 per cent, and contain on an average, according to the latter chemist, 1·54 sulphate of lime, 0·93 chlorid of calcium, and 0·88 chlorid of magnesium in 100·00; the remainder being chlorid of sodium.[3]

The nearly saturated brines from the Saginaw valley in Michigan, which have their source at the base of the Carboniferous series, contain, according to my calculation from an analysis by Prof. Dubois, in 100·00 parts of solid matters, chlorid of calcium 9·81; chlorid of magnesium 7·61; sulphate of lime 2·20; the remainder being chiefly chlorid of sodium. Another well in the same vicinity gave to Chilton an amount of chlorid of calcium equal to 3·76 per cent.[4] In a specimen of salt manufactured in this region Goessmann found 1·09 of chlorid of calcium; and in two specimens of Ohio salt, 0·61 and 1·43 per cent of the same chlorid. The rock-salt from the Lias of Cheshire, according to Nichol, contains small cavities, partly filled with air, and partly with a concentrated solution of chlorid of magnesium, with some chlorid of calcium.[5]

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[3] Goessmann. Report on the brines of Onondaga: Syracuse, 1862 and 1864. Also Report on the Onondaga Salt Co.: Syracuse, 1862.

[4] Winchell: this Journal, [2], xxxiv, 311.

[5] Cited by Bischof, Lehrbuch ii. 1671. The results of the analyses by Mr. Northcote of the brines of Droitwich and Stoke in the same region (L. E. & D. Philos, Mag. [4], ix, 32,) as calculated by him, show no earthy chlorids whatever, and no carbonate of lime, but carbonates of soda and magnesia, and sulphates of soda and lime. He regarded the whole of the lime present in the water as being in the form of sulphate. If, however, we replace, in calculating these analyses, the carbonate of soda and sulphate of lime by sulphate of soda and carbonate of lime, we shall have, for the contents of these brines, chlorid of sodium, with notable quantities of sulphate of soda, some sulphate of lime, and carbonates both of lime and magnesia; a composition which is more in accordance with the admitted laws of chemical combinations. From these results it would appear that the earthy chlorids, which according to Nichol are present in the rock-salt of this formation, are decomposed by sulphates in the waters which, by dissolving the salt, give rise to the brines.

It is to be regretted that in many water analyses by chemists of note the results are so calculated as to represent the co-existence of incompatible salts. Of the association of carbonates of soda and magnesia with sulphate of lime, as in the analysis just noted, it might be said that I have shown that it may occur in the presence

§ 40. The brines from the valley of the Alleghany River, obtained from borings in the Coal formation, are remarkable for containing large proportions of chlorids of calcium and magnesium, though the sum of these, according to the analyses of Lenny, is never equal to more than about one-fourth of the chlorid of sodium. The presence of salts of barium and strontium in these brines, and the consequent absence of sulphates is, according to Lenny, a constant character in this region over an area of two thousand square miles. (See Bischof, Chem. Geol. i, 377). A later analysis of another one of these waters from the same region, by Steiner, is cited by Will and Kopp, Jahresbericht, 1861, p. 1112. His results agree closely with those of Lenny. See also the analysis of a bittern from this region by Boyé (this Journal, [2], vii, 74).

These remarkable waters approach in character to those of Whitby and Hallowell; but in these the chlorid of sodium forms only about one-half the solid contents, and the proportion of the chlorid of magnesium to the chlorid of calcium is relatively much greater than in the waters from western Pennsylvania, where the magnesian chlorid is equal only to from one-third to one-fifth of the chlorid of calcium; the proportions of the two being subject in both regions to considerable variations.

In this connection may be cited a water from Bras d'Or, in the island of Cape Breton, lately analyzed by Prof. How, which contains, in 1000 parts, chlorid of sodium 4·901, chlorid of potassium 0·650, chlorid of calcium 4·413, and chlorid of magnesium only 0·638, besides sulphate of lime 0·134, carbonates of lime and magnesia 0·085, with traces of iron-oxyd and phosphates =10·821. (Can. Naturalist, viii, 370.) The analyses of European waters furnish comparatively few examples of the predominance of earthy chlorids.[*]

§ 41. We have already shown in § 38 how the action of carbonate of soda upon sea-water or bittern will destroy the nor-

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of an excess of carbonic acid. (This Jour., [2], xxviii, 174). By evaporation, however, such solutions regenerate carbonate of lime and sulphates of soda and magnesia; and by the consent of the best chemists these elements are to be represented as thus combined. But what shall be said when chlorid of magnesium, carbonate of soda, and silicate of soda are given as the constituents of a water whose recent analysis may be found in a late number of the *Chemical News;* or when bi-carbonates of soda, magnesia and lime are represented as co-existing in a water with sulphates and chlorids of magnesium and aluminum? These errors probably arise from determining in the recent water, or in water not sufficiently boiled, the lime and magnesia which would by prolonged ebulition be separated as carbonates, together with portions of alumina, silica, etc. In the subsequent calculation of the analyses these dissolved earthy bases being regarded as sulphates or chlorids, instead of carbonates, there remains an excess of soda, which is wrongly represented as carbonate, instead of chlorid or sulphate of sodium.

[*] Lersch, Hydro-Chemie, zweite Auflage: Berlin, 1864; *vide* p. 207. This excellent work, which is a treatise on the chemistry of natural waters, in one volume 8vo of 700 pages, was unknown to me when I prepared the first part of this essay.

mal proportion between the chlorids of magnesium and calcium by converting the latter into an insoluble carbonate, and leaving at last only salts of sodium and magnesium in solution. A process the reverse of this has evidently intervened for the production of waters like that from Cape Breton and some others noticed by Lersch, in which chlorid of calcium abounds, with little or no sulphate or chlorid of magnesium. This process is probably one connected with the formation of a silicate of magnesia. Bischof has already insisted upon the sparing solubility of this silicate, and he observed that silicates of alumina, both artificial and natural, when digested with a solution of magnesian chlorid, exchange a portion of their base for magnesia, thus giving rise to solutions of alumina; which, being decomposed by carbonates, may have been the source of many of the aluminous deposits referred to in § 9. He also observed a similar decomposition between the solution of an artificial silicate of lime and soluble magnesian salts. (Bischof, Chem. Geology, i, 13, also chap. xxiv.) In repeating and extending his experiments, I have confirmed his observation that a solution of silicate of lime precipitates silicate of magnesia from the sulphate and the chlorid of magnesium; and have found, moreover, that by digestion at ordinary temperatures with an excess of freshly precipitated silicate of lime, chlorid of magnesium is completely decomposed; an insoluble silicate of magnesia being formed, while nothing but chlorid of calcium remains in solution. It is clear that the greater insolubility of the magnesian silicate, as compared with silicate of lime, determines a result the very reverse of that produced by carbonates with solutions of the two earthy bases. In the one case, the lime is separated as carbonate, the magnesia remaining in solution; while in the other, by the action of silicate of soda (or of lime), the magnesia is removed and the lime remains. Hence, carbonate of lime and silicate of magnesia are everywhere found in nature; while carbonate of magnesia and silicate of lime are produced only under local and exceptional conditions. The detailed results of some experiments on this subject are reserved for another place. It is evident that the production from the waters of the early seas of beds of sepiolite, talc, serpentine, and other rocks in which a magnesian silicate abounds, must, in closed basins, have given rise to waters in which chlorid of calcium would predominate.

§ 42. Of the waters of the second class whose analyses are here given, the first three occur, with many others of similar character, on the south side of the Ottawa river, below the city of that name. The remaining four are on the north side of the St. Lawrence, between Montreal and Quebec, where also similar waters abound. All of these springs rise from the Lower Silurian limestones of the region.

TABLE II.—WATERS OF THE SECOND CLASS.

	1.	2.	3.	4.	5.	6.	7.	8.	9.
Chlorid of sodium..	11·6660	9·4600	12·2500	11·1400	8·0454	11·7750	11·4968	17·2671	11·5094
" potassium..	·1040	·1040	·0305	·1460	undet.	·0800	·1832	·2409	undet.
" barium.....	......	......	......	·0303	.....	......	·0019	.....	.....
" strontium ..	......	......	......	·0185	.....	......	·0019	.....	.....
" calcium....	·1364	·0443	·2870	·2420	·0466	·0503	·0718	·6038	·2264
" magnesium.	·2452	·4942	1·0338	·2790	·0856	·3743	·6636	2·0523	·8942
Bromid of "	·0080	·0029	·0238	·0283	undet.	·0342	·0091	·0587	·0278
Iodid of "	·0052	·0017	·0021	·0052	*traces.*	·0039	·0046	·0138	·0183
Sulphate of lime ..	......	·1929	......	.....	.....	.....	.....	.....	.....
Carbonate of baryta.	......	......	......	·0106	.....	.....	.....	.....	.....
" strontia .	......	......	......	·0137	.....	.....	.....	.....	.....
" lime.....	·0330	·2980	·1264	·4520	·0470	·2160	·3493	·0120	·0180
" magnesia.	·8904	·3629	·8632	·4622	·8354	1·0593	·9388	·7506	·4464
" iron.....	·0096	*traces.*	*traces.*	*traces.*	.....	·0054	·0145	*traces.*	*traces.*
Silica............	·0700	·0205	·0225	·0552	.....	·0479	·0865	undet.	undet.
Alumina.........	*traces.*	undet.	*traces.*	undet.	.....	·0050	·0145	"	"
In 1000 parts.....	13·1678	10·9814	14·6393	12·8830	9·0600	18 6513	13·8365	20·9987	13·1400
Specific gravity ...	1009·39	1008·78	1010·9	1009·42	.....	1010·36	1011·28	.....	.....

1, 2. These two waters are from the township of Plantagenet. The first is known as Larocque's, and the second as the Georgian Spring. These waters were examined in 1849 and 1851. Two other springs have been observed in the same vicinity, one resembling Larocque's spring and containing borates, with a notable proportion of strontia, while the other is an alkaline-saline water of the third class.

3. Caledonia Intermittent Spring. This spring owes its name to the intermitting discharge of carburetted hydrogen which takes place from its waters. It is in the township of Caledonia, not far from Plantagenet, and near three other waters from the same township, to be mentioned in the next class. The water was collected in September, 1847.

4. Lanoraie. This is from the seigniory of Lanoraie. It contains both baryta and strontia, and evolves an abundance of carburetted hydrogen. The water was collected in March, 1851.

5. Is from a copious spring in the seigniory of Berthier, and was collected in July, 1853.

6. Is from the township of Caxton, and yields six or eight gallons of water a minute, besides a great abundance of inflammable gas. The carbonic acid was found to equal 1·126 parts, of which ·651, or more than one half, is required for the neutral carbonates present. The water was taken from the spring in October, 1848.

7. Is from the seigniory of St. Léon, and is a copious spring which, like the last, disengages inflammable gas. The carbonic acid was equal to 1·224 parts, of which ·651, or not quite one half, is required for the neutral carbonates found by analysis. The water was collected in October, 1848.

8, 9. These are from two springs in the parish of Ste-Geneviève on the Batiscan River, and are remarkable for the large proportion of iodids which they contain. The first is known as Trudel's spring, and the second is at the ferry opposite to the church. The waters were collected in August, 1853. Several other saline springs occur in the same neighborhood.

§ 43. Of the waters of the third class which follow, the first four rise from the Trenton limestone, and occur on the south side of the Ottawa River, in the vicinity of the first three of the preceding section. The others are from the south side of the St. Lawrence below Montreal.

TABLE III.—WATERS OF THE THIRD CLASS.

	1.	2.	3.	4.	5.	6.	7.	8.
Chlorid of sodium....	6·9675	6·4409	3·8480	6·5325	9·4231	8·4286	4·8234	5·9662
" potassium .	·0309	·0296	·0230	·1160	·1234	·0382	·0610	undet.
Bromid of sodium....	·0150	·0169	·0100	·0217	·0126	·0046	undet.	"
Iodid of sodium......	·0005	·0014	*traces.*	·0032	·0054	·0085	"	"
Sulphate of potash...	·0053	·0048	·0183	....	....	....	....	....
Phosphate of soda....	....	....	....	·0124	....	....	....	....
Carbonate of soda....	·0485	·1762	·4558	·5885	·1705	·3260	1·5416	·6082
" baryta...	....	....	....	*traces.*	·0226	·0123	*traces.*	....
" strontia..	....	....	....	"	·0140	·0096	"	·0250
" lime.....	·1480	·1175	·2100	·1500	·3540	·3490	·2180	·1440
" magnesia.	·5262	·5172	·2940	·7860	·5433	·3559	·4263	·4756
" iron.....	*traces.*	*traces.*	*traces.*	*traces.*	·0048	*traces.*	....	*traces.*
Alumina............	·0044	undet.	·0026	·0040	*traces.*	"	undet.	undet.
Silica...............	·0310	·0425	·0840	·1330	·0465	·0540	·2120	·1140
In 1000 parts........	7·7773	7·3470	4·9407	8·3473	10·7202	9·5867	7·2823	7·3330
Specific gravity......	1006·2	1005·8	1003·7	1006·24	1008·15	1007·7	....	....

1, 2 and 3 are waters from Caledonia, and rise about two miles from the spring 1, of the second table. These waters were examined in September, 1847. The first, which is known as the Gas spring, then yielded about four gallons of water a minute, and discharged in the same time about 300 cubic inches of carburetted hydrogen gas, whence its name. At a distance of four or five rods from this, are the second and third springs, known as the Saline and White Sulphur waters, yielding each about ten gallons a minute. The former affords a few bubbles of carburetted hydrogen gas, and is not at all sulphurous, while the latter contained a little sulphuretted hydrogen, equal to somewhat less than a cubic inch to the gallon. The temperature of the three waters was found to be respectively 44°·4, 45° and 46° F. The carbonic acid in 1000 parts of the Gas spring was equal to ·705, of which ·356, or a little more than one-half, is required for the neutral carbonates present. In the Saline spring there was found ·648 of carbonic acid, being an excess of ·292 over that required to form neutral carbonates; while in the Sulphur Spring, which contained in 1000 parts only ·590 of carbonic acid, ·349 are contained in the neutral carbonates, leaving only ·141 towards the formation of bi-carbonates. For later analyses of these waters see § 46.

4. This, which is known as Gillan's spring, is from the township of Fitzroy, not very far from the last. Its waters were collected in July, 1850.

5, 6. These two waters are from Varennes, and are about one hundred rods apart. The first is known as the Saline, and the second is called the Gas spring from the large volumes of carburetted hydrogen gas which it disengages. The Saline spring contained in 1000 parts ·920 of carbonic acid, of which ·451, or nearly one half, is required to form neutral carbonates present. In the Gas spring was found ·792 of carbonic acid, leaving thus ·312 over that required to form neutral carbonates. The waters were collected in October, 1848.

7. This is from Labaie du Febvre, and is known as Courchêne's spring. It evolves small quantities of carburetted hydrogen gas. The water was collected in September, 1852. Several other mineral springs occur in this vicinity, one of them belonging to this class, and others to the second and fourth classes.

8. This water, from the seigniory of Belœil, was collected in 1851.

§ 44. We shall now proceed to the springs which, in § 34, have been referred to the fourth class—and begin with three analyses of a mineral water from Chambly. Here, on a plateau, over an area of about two acres, the clayey soil is destitute of vegetation and impregnated with alkaline waters, which in the dry season give rise to a saline efflorescence on the partially dried-up and fissured surface. A well sunk here to a depth of eight or ten feet in the clay, which overlies the Hudson River formation, affords at all times an abundant supply of water, which generally flows in a small stream from the top of the well. Small bubbles of carburetted hydrogen are sometimes seen to escape from the water. The temperature at the bottom of the well was found, in October, 1861, to be 53° F., and in August, 1864, to be nearly 54° F. The mean temperature of Chambly can differ but little from that of Montreal, which is 44°·6 F., so that this is a thermal water. Another alkaline and saline spring in the same parish has also a temperature of 53° F. The water of the spring here described has a sweetish saline taste, and is much relished by the cattle of the neighborhood. Three analyses have been made of its waters, the results of which are here given side by side. The first was collected in October, 1851; the second in October, 1852; and the third in August, 1864, during a very dry season.

	1851.	1852.	1864.
Chlorid of potassium	undet.	·0324	·0182
“ sodium	·8689	·8387	·8846
Carbonate of sodium	1·0295	1·0004	·9820
“ lime	·0540	·0380	·0258
“ magnesia	·0908	·0765	·0650
“ strontia	undet.	·0045	undet.
“ iron	“	·0024	“
Alumina and phosphate	“	·0063	“
Silica	·1220	·0730	·0166
Borates, iodids and bromids	undet.	undet.	undet.
In 1000 parts	2·1652	2·1822	1·9917

A portion of barium is included with the strontium salt. The water contains moreover a portion of an organic acid, which causes it to assume a bright brown color when reduced by evaporation. Acetic acid gave no precipitate with the concentrated and filtered water; but the subsequent addition of acetate of copper yielded a brown precipitate of what was regarded as apocrenate of copper. The organic matter of this and of many other mineral springs has probably a superficial origin. The carbonic acid was determined in the third analysis, and was equal in two trials to ·903 and ·905. The neutral carbonates in this water require ·452 parts of carbonic acid.

§ 45. In the following table are given the results of the analyses of several other waters, which belong like the last to the fourth class.

TABLE IV.—WATERS OF THE FOURTH CLASS.

	1.	2.	3.	4.	5.
Chlorid of sodium	·0207	·0347	·3818	·3920	....
“ potassium	·0496	·0076	·0067	·0318	·0169
Sulphate of soda	....	*traces.*	·0215	*traces.*	·0188
“ potash	·0081	....	....	....	·0122
Carbonate of soda	·1340	·1952	·2301	1·1353	·0410
“ lime	·1740	·0710	·0620	undet.	·2480
“ magnesia	·1287	·0278	·0257	“	·0690
Iron, alumina, phosphates	*traces.*	....	....	“	*traces.*
Silica	·0161	·0110	·0245	“	·2060
In 1000 parts	·5312	·3473	·7523	1·5591	....
In 10,000 parts	....	....	....	....	·6119

1. This spring was met with some years since in constructing a lock on the Richelieu River at St. Ours, and was enclosed in such a way that it is only accessible through a pump; so that it is impossible to determine the amount of water furnished by the spring, or its freedom from admixture. The water was obtained in November, 1852, and is remarkable for the large proportion of potassium salts. 1000 parts of the water gave of alkalies determined as chlorids, 0·2250; of which 0·0565 parts, or 25·11 per cent, were chlorid of potassium. Another trial gave 24·52 per

cent; while a portion of the water taken from the spring three weeks earlier gave a large proportion of alkalies, equal to 0·3400 of chlorids; of which 0·0596, or 17·53 per cent, were chlorid of potassium.

2. This spring occurs on the bank of the Jacques Cartier river, a little above Quebec. It is strongly impregnated with sulphuretted hydrogen, and appears to contain a considerable proportion of borates. It was collected for analysis in the summer of 1852.

3. This water is from a spring in the township of Joly on the opposite side of the St. Lawrence, a few miles south from the last, and like it is sulphurous, and affords a strong reaction of boric acid. It was collected for analysis in July, 1853.

4. A small area of marshy ground in the seigniory of Nicolet, near the line of St. Gregoire, is, like the similar tract in Chambly, so impregnated with mineral water as to be destitute of vegetation. The water collected in a small pit, dug in this locality in the autumn of 1853, was yellowish colored, and alkaline to the taste, and gave by analysis the above results. Several other alkaline springs occur in this vicinity. All of the preceding waters, with the exception of No. 2, which comes out from the Utica slates, rise, like that of Chambly, from the Hudson River formation.

5. This water, unlike the preceding, is that of a large river, the Ottawa, which drains a region occupied chiefly by ancient crystalline rocks, covered by extensive forests and marshes. The soluble matters which it contains are therefore derived in part from the superficial decomposition of these rocks, and in part from the decaying vegetation. The water, which was taken at the head of the St. Anne's rapids, on the 9th of March, 1854, before the melting of the winter's snows had begun, had a pale amber-yellow color from dissolved organic matter, which gave a dark brown hue to the residue after evaporation. The weight of this residue from 10,000 parts dried at 300° F., was ·6975, which after ignition was reduced to ·5340 parts. As seen in the above table, one-half of the solid matters in this water were earthy carbonates, and more than one-third was silica, so that the whole amount of salts of alkaline bases was ·088, (of which nearly one half is carbonate of soda); while the St. Ours water, which resembles that of the Ottawa in its alkaline salts, contains in the same quantity 4·248, or more than forty-eight times as much. The alkalies of the Ottawa water, equalled as chlorids, ·0900, of which ·0293 or 32·5 per cent were chlorid of potassium. The results of some observations on the silica and organic matters of this river water will be given in part III. It will be observed that in the above table the figures given for the first five waters are for 1000 parts, while those of the Ottawa are for 10,000 parts.

§ 46. In this connection may be given the analyses of two similar springs from Vermont, the Highgate and Alburg springs. The waters were sent me in October and November 1861, and the results have already appeared in the Geology of Vermont, vol. ii, p. 926. Both of these waters, when examined, were slightly sulphurous, and yielded the reactions of boric acid. The amount of carbonate of soda was estimated from the carbonate of baryta obtained by the process already mentioned in § 35.

	Highgate.	Alburg.
Chlorid of sodium	·402	·140
Sulphate of soda	·042	·024
Carbonate of soda	·235	·230
" lime	·024	·036
" magnesia	·010	·022
Potash and borates	undet.	undet.
In 1000 parts	·713	·452

§ 47. On the 5th January, 1865, after a lapse of more than seventeen years, I again visited the three springs of Caledonia, whose analyses have been given in the table § 43, and collected their waters for a second examination. The results of my recent analyses show that considerable changes have occurred in the composition of each of these springs, and tend to confirm in an unexpected manner, the theory which I long since put forward, —that the waters of the second and third classes owe their origin to the mingling of saline waters of the first class with alkaline waters of the fourth class. It will be observed that the three Caledonia waters in 1847 were all alkaline, though the proportions of the carbonate of soda were unlike. Sulphates were also present in all of them, though most abundant in the Sulphur spring, which, although holding the smallest amount of solid matters, was the most alkaline. In January, 1865, however, the first and second of these waters had ceased to be alkaline, and contained, instead of carbonate of soda, small quantities of earthy chlorid, causing them to enter into the second class. They no longer contained any sulphates, but, on the contrary, portions of baryta and strontia. Only the Sulphur spring, which in 1847 contained the largest proportion of carbonate of soda and of sulphates, still retained these elements, though in diminished amounts, and was feebly impregnated with sulphuretted hydrogen. If we suppose these waters to arise from the commingling of saline waters like those of Whitby and Lanoraie, containing earthy chlorids and salts of baryta and strontia, with waters of the fourth class holding carbonate and sulphate of soda, it is evident that a sufficient quantity of the latter water would decompose the earthy chlorids and precipitate the salts of baryta and strontia present; while an excess would give rise to alkaline-saline waters containing sulphate and carbonate of soda, such as were the three springs of Caledonia in 1847. A falling-off in the supply of the sulphated alkaline water has however taken place, and the result is seen in the appearance of chlorid of magnesium and of baryta and strontia in two of the springs, and in a diminished proportion of carbonate of soda in the Sulphur spring.

These later analyses being directed chiefly to the determination of these changes, no attempt was made to determine potassium, iodine and bromine. For the purposes of comparison, the two series of analyses are here put in juxtaposition; the elements just mentioned being included with the chlorid of sodium, and the figures reduced to three places of decimals. The precipitate by a solution of gypsum from the concentrated and acidulated water was regarded as sulphate of strontia, and calculated as such, but was in part sulphate of baryta.

TABLE V.—SHOWING THE CHANGES IN THE CALEDONIAN SPRINGS.

	1. Gas Spring.		2. Saline Spring.		3. Sulphur Spring.	
	1847.	1865.	1847.	1865.	1846.	1865.
Chlorid of sodium	7·014	6·570	6·488	6·930	3·876	3·685
" magnesium	....	·024	....	·026	....	....
Sulphate of potash	·005	....	·005	....	·018	·021
Carbonate of soda	·048	....	·176	....	·456	·091
" lime	·148	·096	·117	·095	·210	·077
" magnesia	·526	·455	·517	·469	·294	·228
" strontia	....	·009	....	·012	....	....
Silica	·021	·020	·042	·015	·084	·021
In 1000 parts	7·762	7·174	7·345	7·547	4·938	4·123

In the recent analyses of these waters, the carbonic acid in the Gas spring was found to equal, for 1,000 parts, ·671; of which ·278 were required for the neutral carbonates. The Saline spring contained ·664 of carbonic acid, of which ·290 go to make up the neutral carbonates. The Sulphur spring, in like manner, gave of carbonic acid ·573, while the neutral carbonates of the water require only ·191. All of these waters in January, 1865, thus contained an excess of carbonic acid above that required to form bicarbonates with the carbonated bases present; while the analyses of the same springs in 1847, showed, as we have seen in § 43, a quantity of carbonic acid insufficient for the formation of bicarbonates. The questions of this deficiency, and of the variation in the amount of carbonic acid in these and other waters will be considered in the third part of this paper.

§ 48. The waters of our fifth and sixth classes, as defined in § 34, are distinguished by the presence of sulphates; the former being acid and the latter being neutral waters. In the fifth class the principal element is sulphuric acid, associated with variable and accidental amounts of sulphates of alkalies, lime, magnesia, alumina, and iron. Apart from the springs of this kind which occur in regions where volcanic agencies are evidently active, the only ones hitherto studied are those of New York and western Canada; which issue from unaltered, and almost horizontal Upper Silurian rocks. (§ 31.) The first account of these remarkable waters was given in this Journal in 1829 (vol. xv, p. 238), by the late Prof. Eaton, who described two acid springs in Byron, Genesee Co., N. Y.; one yielding a stream of distinctly acid water sufficient to turn a mill-wheel, and the other affording in smaller quantities a much more acid water. The latter was afterward examined by Dr. Lewis Beck (Mineralogy of New York, p. 150). He found it to be colorless, transparent and intensely acid, with a specific gravity of 1·113; which corresponds to a solution holding seventeen per cent of oil of vitriol. No chlorids, and only traces of lime and iron, were found in this water, which was nearly pure dilute sulphuric acid. Prof. Hall (Geology of New York, 4th District, p. 134), has noticed in addition to these, several other springs and wells of acid water in the adjacent town of Bergen. Farther westward, in the town of Alabama is a similar water, whose analysis by Erni and Craw will be found in this Journal, [2], ix, 450. It contained in 1000 parts about 2·5 of sulphuric acid, and 4·6 parts of sulphates, chiefly of lime, magnesia, iron and alumina. In this, as in the succeeding analyses, hydrated sulphuric acid, SO_3HO, is meant.

The earliest quantitative analyses of any of these waters were those by Croft and myself of a spring at Tuscarora, in 1845 and 1847, of which the detailed results appear in this Journal, [2], viii, 364. This, at the time of my analysis in September, 1847, contained, in 1000 parts, 4·29 of sulphuric acid, and only 1·87 of sulphates; while the previous analysis by Prof. Croft gave approximatively 3·00 of neutral sulphates, and only about 1·37 of sulphuric acid. Similar acid waters occur on Grand Island above Niagara Falls, and at Chippewa.

All of these springs, along a line of more than 100 miles from east to west, rise from the outcrop of the Onondaga salt-group; but in the township of Niagara, not far from Queenston, are two similar waters which issue from the Medina sandstone. One of these is in the southwest part of the township, and fills a small basin in yellow clay, which, at a depth of three or four feet, is underlaid by red and green sandstones. The water, which like those of Tuscarora and Chippewa, is slightly impregnated with sulphuretted hydrogen, is kept in constant agitation from the escape of inflammable gas. It contained, in 1000 parts, about two parts of free sulphuric acid, and less than one part of neutral sulphates. This water was collected in October, 1849, and at that time another half dried-up pool in the vicinity contained a still more acid water. Another similar spring occurs near St. Davids in the same township.

In connection with the suggestion made in § 31 as to their probable origin at great depths, it would be very desirable to have careful observations as to the temperature of these acid springs. When, on the 19th October, 1847, I visited the Tuscarora spring, the water in two of the small pools had a temperature of 56° F.; but on plunging the thermometer in the mud at the bottom of one of these it rose to 60°·5.

§ 49. It appears from a comparison of the analysis of Croft with my own, that the waters of the Tuscarora spring underwent a considerable change in composition in the space of two years; the proportion of the bases to the acid at the time of the second analysis being little more than one-third of that in the analysis of Croft. This change was indeed to be expected, since waters of this kind must soon remove the soluble constituents from the rocks through which they flow, and eventually become like the water from Byron, little more than a solution of sulphuric acid. The observations of Eaton at Byron, and my own at Tuscarora, show that half-decayed trees are still standing on the soil which is now so impregnated with acid waters as to be unfit to support vegetation. Reasoning from the changes in composition, it may be supposed that these waters were at first neutral, the whole of the acid being saturated by the calcareous rocks through which they must rise. It was from this consideration that I was formerly led to ascribe to the action of these waters, the formation of some of the masses of gypsum which appear along the outcrop of the Onondaga salt-group. (This Journal, [2], vii, 175.) That waters like those just mentioned must give rise to sulphate of lime by their action on calcareous rocks is evident; and some of the deposits of gypsum in this region, as described by good observers, would appear to be thus formed. So far, however, as my personal observations of the gypsums of western Canada have extended, they appear to be in all cases cotemporaneous with the shales and dolomites with which they are interstratified, and to have no connection with the sulphuric-acid springs which are so common throughout that region. (This Journal, [2], xxviii, 365, and Geology of Canada, 352.)

§ 50. We have included in a sixth class the various neutral saline waters in which sulphates predominate, sometimes to the exclusion of chlorids. The bases of these waters are soda, potash, lime, and magnesia; which are usually found together, though in varying proportions. For the better understanding of the relations of these sulphated waters, it may be well to recapitulate what has been said about their origin; and to consider them, from this point of view, under two heads.

First, those formed from the solution of neutral sulphates previously existing in a solid form in the earth. Strata enclosing natural deposits of sulphates of soda and magnesia, sometimes with sulphate of potash (§ 17, § 19), afford the most obvious source of these waters. The frequent occurrence of gypsum, however, points to this salt as a more abundant source of sulphated waters. Solutions of gypsum may in some case exchange their lime for the soda of insoluble silicates, or this salt may be decomposed by solutions of carbonate of soda (§ 7, § 19). The decomposition of the sulphate of lime by hydrous carbonate of magnesia, as explained in § 21, is doubtless in many cases the source of sulphate of magnesia, which is, more frequently than sulphate of soda, a predominant element in mineral waters. In connection with a suggestion made in the section last cited, it may be remarked that I have since found that predazzite, in virtue of the hydrate of magnesia which it contains, readily decomposes solutions of gypsum holding carbonic acid in solution, and gives rise to sulphate of magnesia.

In the second place, sulphuric-acid waters, like those described in § 47, by their action upon calcareous and magnesian rocks, or by the intervention of carbonate of soda, may, as already suggested, give rise to neutral sulphated waters of the sixth class. It is evident also that waters impregnated with sulphates of alumina and iron from oxydizing sulphates, as mentioned in § 28, may be decomposed in a similar manner, and with like results.

Neutral sulphated waters generated by any of the above processes, are evidently subject to admixtures of saline matters from other sources, and may thus become impregnated with chlorids and carbonates. Indeed it is rare to find waters of the sixth class without some portion of chlorids, and a transition is thus presented to the waters of the first four classes; in which also portions of sulphates are of frequent occurrence. The presence of sulphates being one of the conditions required for the generation of sulphuretted hydrogen (§ 10), we find that the waters of the sixth class are very often sulphurous.

§ 51. Waters of the sixth class are very frequently met with in the Paleozoic rocks of New York and western Canada, and are probably derived from the gypsum which is found in greater or less abundance at various horizons, from the Calciferous sand-rock to the Onondaga salt-group. It is however not improbable that the sulphuric-acid waters which abound in this region (§ 48) may, by their neutralization, give rise to similar springs. In the waters of the district under consideration, the sulphate of lime generally predominates over the sulphates of the other bases, and chlorids are frequently present in considerable quantities. For numerous analyses of these waters, see Beck, Mineralogy of New York. The results of an examination of the Charlotteville spring, remarkable for the amount of sulphuretted hydrogen which it contains, will be found in this Journal, [2], viii, 369. A very copious sulphur spring which issues from a mound of calcareous tufa in Brant, C. W., overlying the Corniferous limestone, is distinguished by the absence of any trace of chlorids; in which respect it resembles the acid waters of the fifth class from the adjacent region. A partial analysis of a portion of it collected in 1861, gave, for 1000 parts, sulphate of lime 1·240, sulphate of magnesia ·207, and carbonate of lime ·198. From a slight excess in the amount of sulphuric acid, it is probable that a little sulphate of soda was also present.

Of waters of this class, in which sulphate of magnesia predominates, but few have yet been observed in this country. A remarkable example of this kind from Hamilton, C. W., was examined by Prof. Croft of Toronto, and described by him in the Canadian Journal for 1853 (page 153). It had a specific gravity of 1006·4, and gave for 1000 parts,

Chlorid of sodium,	·5098
Sulphate of soda,	1·6985
" lime,	1·1246
" magnesia,	4·7799
	8·1128

The rocks exposed at Hamilton include the Medina sandstone, and the Niagara limestone, with the intermediate Clinton group. Along the outcrop of the latter, crystalline crusts of nearly pure sulphate of magnesia are observed to form in many localities, during the dry season of the year. (Geology of Canada, p. 460.)

According to Emmons, the Post-tertiary clays near Crown Point, on the western shore of Lake Champlain, are during dry weather covered with efflorescences of sulphate of magnesia, which impregnates several springs in the vicinity. The water of one of these, according to Emmons, had a specific gravity of 1014·0, and contained, in 1000 parts, 18·78 of saline matter, which was chiefly sulphate of magnesia, with some sulphate of lime. (Cited by Beck, Mineralogy of New York, p. 252.) The strata underlying the clays of this region belong, according to the State geological map, to the Potsdam, Calciferous and Trenton formations, but the source of the magnesian salt is not improbably to be found in the clays themselves.

In the third and concluding part of this paper, it is proposed to notice briefly some of the more important points in the chemistry of the various waters which have been here described, and to inquire into their geological relations.

Art. VIII.—*On Molecular Physics;* by Prof. W. A. Norton.

[Continued from vol. xxxix, p. 254.]

Thermo-Electricity.—The key to thermo-electric phenomena should be found in the effect produced by heat on the electric condition of molecules. Now when heat is applied to a surface, the molecules at the surface first receive the ethereal pulses of which the force of heat consists. These pulses passing on to the central atom of each molecule, or the condensed universal ether at the center of the molecule, are there partially expended in expanding its electric atmosphere on the outer side, and are partially propagated on. Upon reaching the inner side of the atom they will again be partially consumed in expanding the atmosphere on that side, while a certain portion will be transmitted to the next atom. It is easy to see that this second expansion should be less than the first. Under these circumstances two important electrical effects will be produced. (1.) By reason of the greater expansion of the atmosphere on the outer than on the inner side, its density will be diminished on the outer side, and hence electric ether will flow around to that side. *The molecular atmosphere will therefore become polarized positively on the outer side.* (2.) As a consequence of this polarization the molecular atmosphere *will urge away from it a portion of the electric ether posited near its surface, and tend to develop a negative polarization in the particles of contiguous surfaces.* The surface receiving the heat will then become positively polarized, and there will be at the same time an electric movement outward from the surface. Cold, or the abstraction of heat, will have precisely the opposite tendency; that is, a surface in the act of cooling will become negatively polarized, and this change will be attended with a flow of electricity toward the surface. Such movements of the electric ether will be in waves of translation, or in currents of free electricity, or both combined, according to the conducting power of the medium exterior to the surface (p. 252). Substances may differ in the effects thus produced, under similar circumstances, from two causes; a difference in their conducting power for heat, and a difference in the degree of expansion, or in the effect of the expansion produced by the same amount of heat.

Now let a plate of bismuth be placed in contact with a plate of antimony, and let the junction be heated and the other ends be brought into good conducting communication. If the above mentioned effects of heat be different for these two metals, a current should set, at the junction, from the one which experiences the greatest effect to that which experiences the least, and pass through the circuit. Bismuth is a poorer conductor of heat than antimony, and in fact than most other metals, and hence its surface molecules should imbibe and retain more heat than those of the antimony. The diamagnetic properties of bismuth also indicate, as will be seen hereafter, that its molecular atmospheres are remarkably expansible. If these peculiarities of bismuth be admitted, we have an explanation of the fact that bismuth is positive[1] to other metals in its thermo-electric relations. The bismuth and antimony in the thermo-electric pair, it will be observed, hold the same relation to each other as the zinc and copper in the galvanic pair, and the heat does the same electrical duty in the one, that the oxygen does in the other.

If the other ends of the two metals be brought together and cooled, the current will be reënforced, since the molecular atmospheres of the bismuth will contract more than those of the antimony. (See effect of cold, p. 61.)

It has been ascertained as the result of numerous experiments, that "increasing the temperature of the negative metal generally increases the amount of deflection of the galvanometer needle produced by heating the junction; while if the higher heat is applied to the metal which is positive at moderate temperatures, a current in the opposite direction is established." To get at the explanation of these curious effects we must observe that the "higher heat" spoken of is applied at a certain distance from the junction, and hence it is the inner sides of the surface molecules which first receive the heat from this second source, and become positively polarized. Accordingly, the current should be strengthened in the first case, above mentioned, and weakened in the second. It has also been observed that a current may be excited with two wires of the same metal, by heating the end of one and bringing it in contact with the other; and that the direction of the current at the junction is from the cold to the hot wire." In this case it is to be remarked that the hot wire is in the act of cooling, and hence there should be an electric movement toward its surface (p. 61), or from the cold to the hot wire, through the junction.

Pyro-electric Crystals.—Tourmaline is the most conspicuous crystal belonging to this class. "A prism of tourmaline has different secondary planes at its two extremities, or, as it is expressed, is hemihedrally modified." This peculiarity of crystallization, which also obtains in the other pyro-electric crystals, indicates that in the direction of the axis, the molecules of the crystal have different mechanical properties on opposite sides. In this condition of things we may reasonably suppose that the molecular atmospheres would expand unequally on opposite sides, under the influence of the same amount of heat. If this be admitted we have a complete explanation of the electric phenomena exhibited by the tourmaline when heated, in accordance with the principles already laid down. Thus, let a tourmaline be heated regularly, that is, so that all points of its surface shall receive equal increments of heat; at all points of the surface the unequal expansive action of the heat upon the two sides of the molecular atmospheres in the axial direction, will determine their polarization, and an attendant electric movement from the positively polarized side of one molecule to the negatively polarized side of the next. There should accordingly be opposite electrical states manifested at the ends of the crystal. This state of things should continue so long as the temperature is rising. But it is to be observed that the effective polarization determined in each molecule by the heat is weakened by the discharge that takes place from one molecule to the next, and that from this cause the signs of electrical excitement at the ends of the crystal will be much feebler when the temperature becomes uniform than they would otherwise be. Now if the heated tourmaline be cooled regularly, the process that attended upon the heating will be reversed, and the electrical states, or effective poles, will be reversed. This reversal of the poles may occur soon after the temperature begins to fall, because of the above mentioned weakening of the effective polarization; since this remaining polarization, when the temperature becomes uniform, which is really *effective* upon bodies placed near the ends of the tourmaline, may be no greater than the opposite *transient* polarization that may arise from a small loss of temperature.

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[1] By the positive metal is here meant that which imparts, at the heated junction, positive electricity to the other metal. This is often termed the negative metal. Its polarization is positive, but its electrical state is negative (p. 245).

Mutual Attractions and Repulsions of Electric Currents.—We have seen that in an electric current there is a flow, or onward movement of electric ether throughout the entire route of the current. Now the rapidly moving molecules of electric ether must act impulsively against the universal ether in their path; and every such impulse must be propagated indefinitely outward through this ether. The innumerable impulses thus originating in a current are parallel to the direction of the current, and must take effect upon all surrounding molecules in this direction. They constitute what may be called the *external impulsive force of the current.* The well known phenomena under immediate consideration, as well as all the other phenomena of the external action of currents to be discussed, are to be ascribed to the operation of this external impulsive force. Let us first take the case of two parallel wires conveying currents in the same direction. The impulses propagated from these wires will tend to weaken the tension of the universal ether lying between the wires. A similar effect, but less in amount, will be produced upon the ether lying on the outer sides of the wires. The ethereal tension should therefore be in excess upon the outer sides of the wires, and hence they should be urged toward each other, or there should be an apparent attraction between the wires conveying the currents. The excess of ethereal tension here alluded to is attributable to the fact that the impulses proceeding from the one wire, in being propagated through the other, are materially reduced in intensity. This effect results mainly from the dispersion produced by the interstitial ether, which is brought into a very disturbed state of density by the swiftly moving atoms of the electric ether in the current. When only one of the wires conveys a current, no attraction or repulsion is observed, because the dispersion just mentioned is wanting.

If the currents be supposed to traverse the wires in opposite directions, then the same operative cause, the external impulsive forces of the currents, will compress the ether between the wires to a greater degree than beyond them, and thus there will be an effective force urging them farther apart.

Let *a*, fig. 8, be a point of one of the currents, from which an impulse is propagated, and *ab*, *ac*, *ad*, lines radiating from it and crossing the second wire, *v*, under different degrees of obliquity. The dotted lines drawn between *bd* and *eg* represent the lines of moving electrical atoms. Each of the lines *ab*, *ac*, *ad*, will cross the same number of such lines, and therefore impulses propagated along them will encounter the same number of moving atoms, and experience the same proportional diminution. This diminution should be a certain constant fractional part of the impulse that reaches any point of the nearer side, *eg*, of the wire. The diminution of the propagated impulse, in traversing the wire, simply by reason of the increased length of the passage, as the line is more oblique, should also be a constant fractional part of the impulse; since *fc* is the same proportional part of *af*, or *an* for each point of the wire. Hence the action of any point *a* of the first wire, upon any point *f* of the second, should be inversely proportional to the square of the distance; and the entire force of action of one indefinite wire upon another should be inversely proportional to the distance between the two. (See Lamé, Cours de Physique, vol. iii, p. 236.)

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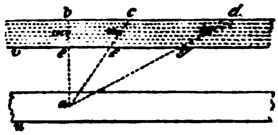

If the currents cross each other under a certain angle instead of being parallel, it may be seen by attending to the mutual actions of the separate points of the two currents, that there will be attractions or repulsions according to the relative directions of the currents at the points; and that the entire action will tend to bring the two currents into the same direction, in which the attraction will be a maximum.

External Action of an Electric Current upon bodies in their natural state.—In undertaking to deduce from our fundamental principles the varied phenomena of the action of a current upon bodies in its vicinity, we have to consider that there are *two modes* in which the external impulsive force of a current may act upon such bodies and develop currents; the one *direct*, and the other *indirect.* (1.) The propagated impulses may take effect directly upon the atoms of the electric atmospheres of the molecules, impelling them in the same direction that the primary current is moving, and so tend to generate a current similar to the primary. (2.) Or these same ethereal impulses may fall upon the central atoms of the molecules, force up the atmospheres on the side of the atoms upon which they fall, and so develop a current opposite to the original one. We have to consider, also, that the external current-force may operate, theoretically, in both these ways, either upon the *simple molecules* which are grouped together into compound molecules, or upon *compound molecules* as a whole. The action upon the constituents of the compound molecules, tends to develop currents within the mass of these molecules. Among the variety of especial currents which may thus be excited, we have particularly to note those which may be developed in the surface of each group, and circulate around it, from particle to particle. If the direct mode of operation of the primary current predominates, such circular currents are *magnetic;* if the indirect prevail, they are *diamagnetic.* If the two tendencies countervail each other, the substance is in a *neutral* magnetic condition. The neutral magnetic state may also result from the absence of groups of particles in the substance, within which circular currents can be established.

To the action of the current upon each compound molecule, with its own proper atmosphere (p. 241), as a whole, are to be ascribed the remarkable phenomena of *Induced Currents*, especially so-called. Experiment has hitherto failed to detect the existence of any current, from one such molecule to another, or through the mass of a body, produced by the uniform action of an established current. All such induced currents result from either an increase or a decrease in the effective action of the inducing current, and are wholly due to the indirect mode of operation of this current. This fact indicates, in accordance with the views that have been advanced upon electric polarization, (p. 242), that the current develops a polarization in the compound molecules of bodies, which increases or decreases as the external impulsive action of the current increases or decreases, but remains constant so long as this action continues the same. This polarization is Faraday's "Electro-tonic state" of bodies.

Magnetism.—The general nature of the magnetic currents, as distinguished from other electric currents, has just been indicated. The existence of such currents in the surface of a compound molecule seems to imply that there is a virtual chain of particles extending circularly around it, which there would be if the number of particles in each group be large. Ampére's researches have completely established that the idea of circular molecular currents is the key to all magnetic phenomena. It suffices, therefore, for our present purpose, to show, as has been done, that such currents are legitimately deducible from the fundamental conceptions laid down at the outset; and that the mutually attractive and repulsive actions of currents may also be derived from the same mechanical ideas, (p. 63). We adopt, then, definitively, Ampére's theory of the constitution of a magnet.

The immediate cause of the development of magnetic currents is the direct impulsive action of an established current, taking effect upon the particles in the surface of compound molecules.

In respect to magnetic properties, we have specially to distinguish soft iron and steel. The cause of their difference of property seems to lie simply in a difference in the conducting power of the groups of particles into which they are aggregated. If in soft iron these groups are good conductors, the electric ether set in motion should pass freely around them, unattended with any material polarization of the particles, and unresisted by the force that results from such polarization, (p. 245). When the exciting cause ceases to operate, there is no force remaining to counteract the resistance of the universal ether to the flow of the electricity, and the retarding operation of contiguous opposing currents. But if we suppose that, in the case of steel, the molecular groups are imperfect conductors, the magnetizing force will develop a polarization of the separate molecules; and, as a consequence, the currents may pass chiefly through their atmospheres, as in the case of electric induction, (p. 243). In this state of things, when the force is withdrawn, it does not follow that the magnetic currents will entirely pass off; for there would apparently be a force in operation which would tend to retain currents of a certain intensity in circulation. This is the mutual action of the contiguous positive and negative sides of two molecules, throughout the chain. It would seem that such a circular chain of particles would not, of necessity, attain to a condition of statical polarization, but might reach a dynamical equilibrium; in which the impulsive force, from each discharge brought about by the action of contiguous molecules, should restore the polarization lost by the discharge to the next preceding molecule.[1]

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[1] It is worthy of consideration, whether certain phenomena of luminosity, as phosphorescence, heat lightning, &c., may not have a similar origin, viz: in recurring discharges resulting from a previous molecular polarization, established by an electric discharge through the mass; or from a similar effect produced in a feebler degree by heat or light.

According to this view, permanent magnetization consists in polarizing the molecules, and it is this induced state which determines and maintains, in opposition to all resistances, currents of a certain intensity. A certain amount of force ("coercive force"), and a certain interval of time, are expended in developing this polarization. At the same time it is only when this force is exerted with this result that permanent magnetism can exist. Heat, or a blow, or an electric discharge, by destroying the molecular polarization, may demagnetize the bar. It is also to be observed that intense heat may demagnetize a bar, and destroy the susceptibility to magnetism in iron or steel, by breaking up the groups of particles into which the mass is aggregated.

Diamagnetism.—It has already been intimated that the diamagnetic state into which certain substances, as bismuth, phosphorus, antimony, &c., are brought by the action of a powerful magnet, probably consists in the circulation of currents around the compound molecules of the mass, in the opposite direction to the inducing current; and that these currents result from the second mode of operation of the external force of the primary current, (p. 65). Accordingly, the susceptibility to diamagnetism must arise from a special liability to expansion on the part of the electric atmospheres of the simple molecules, when urged upward by the ethereal pulses that fall upon their central atoms.

Electro-Magnetism.—The essential theory of the developement of magnetism by electric currents, and of electro-magnetic phenomena generally, is embraced in what has already been stated, (pp. 63–4–5).

Magneto-Electricity.—The excitation of electric currents by magnetic action is a phenomenon of pure induction, and will be included under the next head.

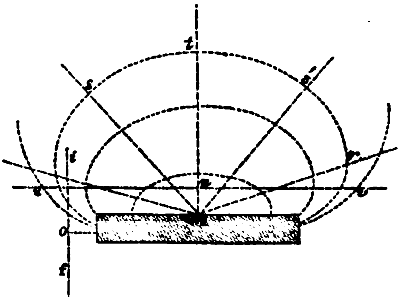
Induction of Electric Currents.—The term induction, as ordinarily used in Dynamical Electricity, has reference only to the development of currents, through wires, or upon the surfaces, or within the mass of bodies. It will suffice to confine our attention to the origination of currents in wires forming a closed circuit. The general idea of the process, as contemplated from our theoretical point of view, has already been indicated. The fundamental principle which covers the whole ground is, that whenever the action of the impulsive force of any current begins, or is on the increase, the induced current sets in the opposite direction to the inducing current; and when the same action terminates, or is on the decrease, the induced flows in the same direction as the inducing current. The rationale is, that the ethereal pulses of the originating force fall upon the condensed ether at the central portion of each group of molecules, and urge outward the interstitial electric ether on that side of the group.

This determines a flow of a certain portion of the electric ether around to that side; and a consequent positive polarization on that side of the group, or compound molecule. While this process of polarization is going on, there must then be a flow of electricity from one compound molecule to the next, in the same direction in which the transfer of ether occurs within the molecules themselves, as an inevitable attendant upon the increasing polarization (p. 242). When the primary action becomes constant, there simply abides a state of static polarization,—an "electro-tonic state,"—in which an equilibrium prevails throughout the circuit. But when this action falls off, the polarization maintained by it must decline, and a current arise setting in the opposite direction to the former one (p. 67). The inducing current may be either electric, or magnetic, so-called. The increase of its action upon the adjacent wire may result either from the closing of the circuit in the case of a galvanic current, or the magnetization of the iron when a magnet is employed; or from the approach of the current to the wire; or from a change in the position of the wire in the magnetic field, attended with an augmentation in the action of the external impulsive force exerted by all the circular currents of the magnet. A decrease in the polarizing effect of the inducing current may result from an interruption of the circuit, or a diminution in the force of the magnet, or a recess of the wire from the current, or a movement of the wire in the magnetic field attended with a diminished action of the entire impulsive force of the magnet.

The oppositely directed currents induced by alternately closing and breaking the galvanic circuit, when the two contiguous wires are formed into a hollow coil, are greatly augmented by inserting rods, or bundles of iron wire within the coil. The explanation of this is, doubtless, that the direct action of the galvanic current is reënforced by the magnetic currents which it develops in the iron. The other cases of induction above referred to need not be dwelt upon, with the exception of that resulting from the movement of a wire to different points of the magnetic field.

This case has been subjected to a rigorous experimental analysis by Faraday, who has deduced from his researches the following general results. If a wire, forming part of a closed circuit, be held in a direction transverse to a magnet, and moved in the magnetic field, across the "lines of force," or magnetic curves, a current will be induced in the wire. If the wire be moved in one direction across these lines of force, a current in a certain direction will arise, and if it be moved across them in the other direction, a current in the opposite direction will be induced. The quantity of electricity set in motion will depend upon the number of lines of force crossed by the moving wire, and not upon the obliquity of the direction of the motion to these lines. Thus, if the wire be placed over the middle of the magnet, (*m*, fig. 9), and moved in any direction from this position, across the lines of force as shown in the figure, a current will be induced in it, running in the same direction as the circular currents on the upper side of the magnet; and if the wire be moved across these lines toward the middle of the magnet, a current will traverse the wire in the opposite direction. In each case the comparative quantity of electricity set in motion, will depend solely upon the comparative number of magnetic curves crossed, and not upon the line, *mr*, *ms*, or *mt*, along which the movement takes place.

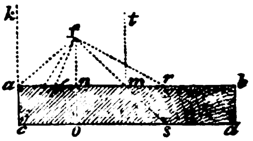
9.



These facts are in entire accordance with the fundamental principle of induction, stated on page 67. For, (1.) the impulsive force of the magnet, at any point of the field, is the resultant of the special impulsive forces due to all the circular currents of the magnet, and has the same direction as these currents in the upper face of the magnet. (2.) This resultant will have the greatest value at the middle of the magnet, *m*, and decrease in all directions from this point. (3.) The curves of equal impulsive force correspond very closely to Faraday's "lines of force." For it will be seen on a little reflection, that the force will, beyond a certain distance from the middle, if not from the very middle, decrease most rapidly parallel to the surface of the magnet; and that the curves of equal impulsive force, proceeding from points on the ends of the magnet, will recede, over the middle of the magnet, to a greater distance from it than half its length. This is strikingly true of the curves that originate from near the center of each end; for at the very center, the force in question vanishes entirely, and therefore the curves for that point would be thrown to an infinite distance from the middle of the magnet.

In making a comparative estimate of the impulsive force of the magnet in different parts of the field, it should be observed that in receding from the magnet the force that results from any one molecular current is the resultant of the opposing impulses propagated from the oppositely directed currents on the nearer and farther sides of the molecule, and that this not only depends upon the distance of the point from the molecule, but also upon the obliquity of the line connecting the two, to the plane of the circular current. For the same distance the resultant will be greatest when the point lies in this plane, and zero when the point is opposite the center of the current. Bearing this in mind, it may readily be seen that if a line, *uv*, be drawn parallel to the axis of the magnet, at any distance from it, the impulsive force of the magnet, along this line, will decrease from the point *n*, opposite the middle of the magnet, indefinitely in both directions. Also that the force will decrease along a line, as *if*, parallel to the end, and vanish at *o* in the prolongation of the axis.

10.



To obtain a general expression for the intensity of the external impulsive force of the magnet, let *abcd*, fig. 10, be a magnet, and let us regard its effective action upon any molecule, at *f*, as the result of the joint action of two sets of opposing currents, the one lying in the upper face *ab*, and the other in the lower face *cd*. Let $nf=y$, $nr=z$, $nr'=z'$, $nb=u$, $na=v$, $ac=d$, and m=coefficient of the impulsive force of an individual current. Then for the action of *r* upon *f* we have $\frac{mdz}{y^2+z^2}$; and for the action of *nr*, $\int\frac{mdz}{y^2+z^2}=\frac{m}{y}\,\text{tang}^{-1}\frac{z}{y}+\text{C}$. For the entire action of *nb*, we have the definite integral $\frac{m}{y}\,\text{tang}^{-1}\frac{u}{y}$. In a similar manner we obtain, for the opposing action of *od*, $\frac{m}{y+d}\,\text{tang}^{-1}\frac{u}{y+d}$. The effective impulsive action of the portion *nodb* of the magnet, will then be

$$\frac{m}{y}\,\text{tang}^{-1}\frac{u}{y}-\frac{m}{y+d}\,\text{tang}^{-1}\frac{u}{y+d}.$$

The effective action of the other portion, *naoc*, of the magnet, will be

$$\frac{m}{y}\,\text{tang}^{-1}\frac{v}{y}-\frac{m}{y+d}\,\text{tang}^{-1}\frac{v}{y+d}.$$

We therefore have, for the entire action of the magnet,

$$w=\frac{m}{y}\left(\text{tang}^{-1}\frac{u}{y}+\text{tang}^{-1}\frac{v}{y}\right)-\frac{m}{y+d}\left(\text{tang}^{-1}\frac{u}{y+d}+\text{tang}^{-1}\frac{v}{y+d}\right),$$

$$\text{or, } w=\frac{m}{y}\left(\text{arc } afb\right)-\frac{m}{y+d}\left(\text{arc } cfd\right) \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad \text{(a.)}$$

When y is large as compared with d, we have approximately

$$w=\frac{m}{y}(\text{arc } afc+\text{arc } bfd) \quad . \; . \; . \; . \; . \; . \; . \; . \quad \text{(b.)}$$

To obtain the equation of the curve of equal impulsive force, let $mn=x$, and $rm=t$. Then $nr=z=x+t$, and $nb=u=x+{}+mb=x+a$. Also $na=v=a-x$. Hence,

$$\frac{m}{y}\left(\text{tang}^{-1}\frac{x+a}{y}+\text{tang}^{-1}\frac{a-x}{y}\right)-\frac{m}{y+d}\left(\text{tang}^{-1}\frac{x+a}{y+d}+\text{tang}^{-1}\frac{a-x}{y+d}\right)=\text{C}\ldots\text{(c.)}$$

C here represents the constant intensity of the impulsive force of the magnet, for one curve. The value of C decreases as the distance of the curve from m, fig. 9, increases. The equation (b) shows that for the larger curves, except near the magnet, $afc+bfd$, must vary in nearly the same ratio with the ordinate y, from one point to another of the curve. To the left of the line ak the arc afc becomes negative in equation (b). Equations (a) and (c) fail for $y=0$.

The investigation here made proceeds upon the supposition that the breadth of the magnet is indefinitely small. If we suppose it to be indefinitely great, the action of each individual transverse current upon any point, f, fig. 10, would be inversely proportional to the distance of the current from this point,[*] and it will be readily seen that the amount of force propagated to f, within any angle, as mfr', will be the same whatever may be the value of fn.

The equation for the value of the effective impulsive force will be approximately of the form

$$w=k\left(\frac{l}{n}+\frac{l'}{n'}\right);$$

k being a constant coefficient dependent upon the strength of the magnet,—l and l' the parts of the length, ab, of the magnet, comprised between the angles afc and bfd subtended by the two ends,—and n, n' the mean distances of these parts from f. We approximate to this state of things in proportion as the magnet is supposed to be broader, and shorter and thinner; or in proportion as, with a magnet of given dimensions, the point f is taken nearer to the magnet.

Let us now replace Faraday's lines of force by the curves of equal impulsive force of the magnet, and consider what should be the effect of moving a wire across them, along any line mr, ms, mt, &c., (fig. 9).

It is obvious that if the movement be outward, the impulsive force taking effect upon the wire will decrease; and that if it be inward, the force will increase. Hence, agreeably to the fundamental principle before alluded to, (p. 67,) in the first case there should be an induced current having the same direction as the currents of the upper face of the magnet; and in the second case a current pursuing the opposite direction. Again, the amount of change of force which results from the displacement of the wire, and therefore the quantity of electricity which this charge sets in motion, should depend solely upon the number of curves traversed. We may add that in whatever part of the magnetic field, and in whatever direction the wire be supposed to move, the theoretical result is in perfect accordance with the facts as experimentally established by Faraday.

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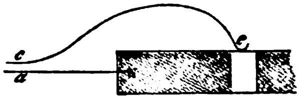
[*] The individual molecular currents lying in a transverse section of the magnet, are here supposed to be replaced by two linear currents transverse to the magnet, one in the upper and the other in the lower surface.

11.



In the foregoing we have supposed the wire, transverse to the magnet, to be moved parallel to itself to various points of the magnetic field; but Faraday has shown that a current may also be induced in the wire, by bending it into a curve and causing it to revolve around the magnet, after one end has been brought into contact with the equatorial part of the magnet, and the other with a wire or rod leading out from the pole, as shown in fig. 11. "A copper ring was fixed round and in contact with the equatorial part, and the wire, *e*, made to bear by spring pressure against this ring, and also against a ring on the axis." The direction of the current changed with the direction of revolution. Corresponding currents were also obtained by rotating the magnet in the opposite directions, the wire remaining fixed. To explain these currents upon the principles now developed, we must first observe that the impulsive force of the magnet will impart a transverse polarization to the molecules of the wire. Now let a motion of revolution be imparted to the wire, in a direction opposite to that of the circulation of the magnetic currents, and the relative velocity with which the ethereal impulses will fall upon the molecules will be the sum of the velocity due to the impulse, and that of the molecules themselves in the opposite direction. The molecules at the end, *e*, of the wire, will therefore take on a higher polarization than is induced in the copper ring by the magnet simply. This polarization should be attended with a disturbance of the electric condition of the molecules in the direction of the length of the wire. There should, then, be an inequality in this disturbance at the point of contact, *e*. This inequality should originate a current that would pass around the circuit. Let v denote the velocity answering to the magnetic impulse, and v' the velocity of revolution of the molecule at *e*. Then the effect due to the polarizing force at the end, *e*, of the wire, may be represented by $m\ (v+v')^2$, and that induced in the contiguous particles of the copper ring by mv^2. The difference is $mv'(2v+v')$, which represents the electro-motive force of the current. If the wire were made to revolve around an unmagnetized bar, the originating force of the current would be mv'^2. The electro-motive force just mentioned would exceed this nearly in the ratio of $2v$ to v'. A very high velocity of revolution of the wire would therefore be required to develop a sensible current, if the bar were unmagnetized. The above expression for the electro-motive force, viz, $mv'(2v+v')$, which is nearly equal to $2mvv'$, shows that this force is proportional to the velocity, v', of revolution of the wire. The entire force developed in ten revolutions of the wire should then remain the same, if the velocity of revolution should be changed, (as determined by Faraday). If the magnet rotates in the opposite direction its impulses against the ether will be correspondingly increased, and the result will be the same.

Faraday, in certain papers originally published in the Philosophical Magazine, and the Philosophical Transactions, has indulged in ingenious speculations upon the probable physical character of the lines of magnetic force, and distinctly intimates that he inclines to the opinion that they have in reality a physical existence, correspondent to their analogues the electric lines," instead of being simply "representants of magnetic power," or lines of resultant magnetic action. In speculating upon the question in what this physical existence may consist, he remarks that "it may be a vibration of the hypothetical æther," (along the lines), or a state or tension of that æther equivalent to either a dynamic or a static condition, or it may be some other state." The results arrived at, in the present paper, are opposed to these speculative ideas of the great English physicist, for our conclusions are that the lines upon which the phenomena of induction by a magnet depend are merely lines of equal magnetic action; but the action is that of a force whose existence has not heretofore been recognized, viz, the so-called impulsive force of the magnet.

(To be concluded.)

ART. IX.—*On the Spectra of some of the Fixed Stars;* by WILLIAM HUGGINS, F.R.A.S., and Prof. W. A. MILLER, V.P.R.S., and *On the Spectra of some of the Nebulæ;* by WILLIAM HUGGINS, F.R.A.S.

Mr. HUGGINS and Prof. MILLER presented to the Royal Society of London, on the 26th of May, 1864, an important paper on the spectra of some of the fixed stars, and Mr. Huggins presented one on the 8th of September on the spectra of some of the nebulæ. By a peculiar adaptation of the spectroscope to a telescope of 10 feet focal length and 8 inches aperture, they were able to make a direct comparison of the spectra of the moon, planets, fixed stars, and nebulæ, with the spectra of the several chemical elements. The following are some of the more important points of the two papers.

The result of the analysis of the light of the moon is wholly negative as to the existence of any considerable lunar atmossphere. The spectra of the planets Jupiter, Saturn, Mars, and Venus, agree essentially with the solar spectrum. Differences however exist which cannot be due to the earth's atmosphere. The evidence afforded by the prism of the existence of atmospheres around these planets is imperfect. This may be explained by supposing the light to be reflected from masses of clouds in the upper strata of thin atmospheres, and not from the surface of the planets.

Observations on the Fixed Stars.—The number of fixed stars which we have, to a greater or less extent, examined amounts to nearly 50. We have, however, concentrated our efforts upon three or four of the brighter stars, and two only of these have been mapped with any degree of completeness. These spectra are, indeed, as rich in lines as that of the sun, and even with these it may be advantageous to compare the spectra of additional metals when the season is again favorable.

α Tauri (Aldebaran).—The light of this star is of a pale red. When viewed in the spectroscope, numerous strong lines are at once evident, particularly in the orange, the green, and the blue portions. The positions of about seventy of these lines have been measured.

We have compared the spectra of sixteen of the terrestrial elements by simultaneous observation with the spectrum of Aldebaran, of course selecting those in which we had reason, from the observations, to believe coincidence was most likely to occur. Nine of these spectra exhibited lines coincident with certain lines in the spectrum of the star. They are as follows:—*sodium, magnesium, hydrogen, calcium, iron, bismuth, tellurium, antimony,* and *mercury.*

Seven other elements were compared with this star, viz. nitrogen, cobalt, tin, lead, cadmium, lithium, and barium. No coincidence was observed. With *nitrogen* three strong double lines were compared, with *cobalt* one strong single line and a double line, with *tin* five lines, with *lead* two strong lines, with *cadmium* three lines, with *barium* two of the strongest in the green, and with *lithium* the line in the orange, but were found to be without any strong lines in the star-spectrum corresponding with them.

α Orionis.—The light of this star has a decided orange tinge. None of the stars which we have examined exhibits a more complex or remarkable spectrum than this.

The spectra obtained from sixteen elementary bodies were observed simultaneously with it. In five of these, viz. *sodium, magnesium, calcium, iron,* and *bismuth,* lines corresponding with certain stellar lines were found to exist.

The bright green line so characteristic of *thallium* appears to coincide with one of the lines seen in the star-spectrum; but this line may be due to calcium, since the small difference between the position of the thallium line and that of one of the calcium lines very close to it, would not be distinguishable with the dispersive power of the apparatus employed.

In the spectra of the other elements which we compared with that of the star, no coincidences occur. There is no line coincident with the red line C of *hydrogen;* but in the star are two strong lines, one on either side of the position of C: there is also no line coincident with F. It is strikingly confirmatory of this methed of analysis, that in all the stars hitherto examined by us in which a line corresponding to C exists, that corresponding to F is also found. When F is absent, C is also wanting. In *nitrogen* three strong double lines were compared. In *tin* five lines, and in *lead* two bright lines were compared, but no coincidence was found.

The strongest of the gold lines approximates closely in position to one in the spectrum of the star, but it is probably not coincident. Three of the strong lines of *cadmium*, two of *silver*, four of *mercury*, two of *barium*, and one (the orange line) of *lithium*, were observed to be not coincident with any of the lines visible in the star. In these comparisons, when barium was used, it was employed in the form of a nearly solid amalgam.

β Pegasi.—The color of this star is a fine yellow. In the general arrangement of the groups, in the gradation of the strength of the lines composing the groups, and in the absence of the hydrogen lines, this spectrum, though much fainter, is closely analogous with the spectrum of α Orionis. Nine of the elements were compared with it. Two of these, viz. *sodium*, and *magnesium*, and perhaps a third, viz. *barium*, furnish spectra in which there are lines which coinicide with lines in the spectrum of the star.

The spectra of *iron* and *manganese* were also compared with that of the star, but the state of the atmosphere prevented any certain conclusion. The lines in the spectra of *nitrogen*, *tin*, and *mercury*, were not coincident with any definite lines in the star-spectrum. Neither of the *hydrogen* lines corresponding to C and F was present.

The absence in the spectrum of α Orionis, and also in the spectrum of β Pegasi which so closely resembles it in character, of lines corresponding to those of hydrogen, is an observation of considerable interest. It is of the more importance since the lines C and F are highly characteristic of the solar spectrum and and of the spectra of by far the larger number of the fixed stars to which our observations have been extended. These exceptions are further interesting as they seem to prove that the lines C and F are due to the luminous bodies themselves. Of this some doubt might be entertained, and it might be suspected that they are in some way due to our own atmosphere, if these lines were present in the spectra of *all* the stars without exception. This absence of the lines corresponding to hydrogen is also the more entitled to consideration, since it is so rare to find them wanting, amongst the considerable number of stellar spectra which we have observed.

Sirius.—Three if not four elementary bodies have been found to furnish spectra in which lines coincide with those of Sirius. viz. *sodium*, *magnesium*, *hydrogen*, and probably *iron*.

The whole spectrum of Sirius is crossed by a very large number of faint and fine lines. It is worthy of notice that in the case of Sirius, and a large number of the white stars, at the same time that the hydrogen lines are abnormally strong as compared with the solar spectrum, all the metallic lines are remarkably faint.

α Lyræ (Vega).—This is a white star having a spectrum of the same class as Sirius, and as full of fine lines as the solar spectrum.

General Observations.—Probably in the constitution of the stars as revealed by spectrum analysis, we shall find the origin of the differences in the color of stellar light. Since spectrum analysis shows that certain of the laws of terrestrial physics prevail in the sun and stars, there can be little doubt that the immediate source of solar and stellar light must be solid or liquid matter maintained in an intensely incandescent state, the result of an exceedingly high temperature. For it is from such a source alone that we can produce light even in a feeble degree comparable with that of the sun.

The light from incandescent solid and liquid bodies affords an unbroken spectrum containing rays of light of every refrangibility within the portion of the spectrum which is visible. As this condition of the light is connected with the state of solidity or liquidity, and not with the *chemical* nature of the body, it is highly probable that the light when first emitted from the photosphere, or light-giving surface of the sun and of the stars, would be in all cases identical.

The source of the difference of color, therefore, is to be sought in the difference of the constituents of the investing atmospheres. The atmosphere of each star must vary in nature as the constituents of the star vary; and observation has shown that the stars do differ from the sun and from each other in respect of the elements of which they consist. The light of each star therefore will be diminished by the loss of those rays which correspond in refrangibility to the bright lines which the constituents of each atmosphere would, in the incandescent state, be capable of emitting. In proportion as these dark lines preponderate in particular parts of the spectrum, so will the colors in which they occur be weaker, and consequently the colors of other refrangibilities will predominate.

Observations on the Planetary Nebulæ.—These wondrous objects present in the telescope slightly oval disks. For this reason they were placed by Sir William Herschel in a class by themselves. They present but little indication of resolvability. The color of their light, which in the case of several is blue tinted with green, is remarkable, since this is a color extremely rare amongst single stars. These nebulæ, too, agree in showing no indication of central condensation. By these appearances the planetary nebulæ are specially marked as objects which probably present phenomena of an order altogether different from those which characterize the sun and the fixed stars. On this account, as well as because of their brightness, I selected these nebulæ as the most suitable for examination with the prism.

The numbers and description of the nebulæ, are taken from the last Catalogue of Sir John Herschel.

[37 H. IV. A planetary nebula in Draco, very bright; pretty small; suddenly brighter in the middle, very small nucleus.]

On August 29, 1864, I directed the telescope armed with the spectrum apparatus to this nebula. At first I suspected some derangement of the instrument had taken place; for no spectrum was seen, but only a short line of light perpendicular to the direction of dispersion. I then found that the light of this nebula, unlike any other ex-terrestrial light which had yet been subjected by me to prismatic analysis, was not composed of light of different refrangibilities, and therefore could not form a spectrum. A great part of the light from this nebula is monochromatic, and after passing through the prisms remains concentrated in a bright line occupying in the instrument the position of that part of the spectrum to which its light corresponds in refrangibility. A more careful examination with a narrower slit, however, showed that, a little more refrangible than the bright line, and separated from it by a dark interval, a narrower and much fainter line occurs. Beyond this, again, at about three times the distance of the second line, a third, exceedingly faint line was seen. The positions of these lines in the spectrum were determined by a simultaneous comparison of them in the instrument with the spectrum of the induction spark taken between electrodes of magnesium. The strongest line coincides in position with the brightest of the air lines. This line is due to nitrogen, and occurs in the spectrum about midway between *b* and F of the solar spectrum.

The faintest of the lines of the nebula agrees in position with the line of hydrogen corresponding to Fraunhofer's F. The other bright line was compared with the strong line of barium; this line is a little more refrangible than that belonging to the nebula.

Besides these lines, an exceedingly faint spectrum was just perceived for a short distance on both sides of the group of bright lines. I suspect this is not uniform, but is crossed with dark spaces. Subsequent observations on other nebulæ induce me to regard this faint spectrum as due to the solid or liquid matter of the nucleus, and as quite distinct from the bright lines into which nearly the whole of the light from the nebulæ is concentrated. The color of this nebula is greenish blue.

[6 Σ. A planetary nebula in Taurus Poniatowskii, very bright; very small; round; little hazy.] The spectrum is essentially the same as that of 37 H. IV. The three bright lines occupy the same positions in the spectrum, which was determined by direct comparison with the spectrum of the induction spark. These lines have also the same relative intensity. They are exceedingly sharp and well defined. The presence of an extremely faint spectrum was suspected. In connexion with this it is important to remark that this nebula does not possess a distinct nucleus. The color of this nebula is greenish blue.

[73 H. IV. A planetary nebula in Cygnus with a central star. Bright; pretty large; round; star of the 11th magnitude in the middle.] The same three bright lines were seen. Their positions in the spectrum were verified by direct comparison with the induction spark. In addition to these a spectrum could be traced from about D to about G of the solar spectrum. This spectrum is much stronger than the corresponding spectrum of 37 H. IV. This agrees with the greater brightness of the central star, or nucleus. The opinion that the faint continuous spectrum is formed alone by the light from the bright central point was confirmed by the following observation. When the cylindrical lens was removed, the three bright lines remained of considerable length, corresponding to the diameter of the telescopic image of the nebula; but the faint spectrum became as narrow as a line, showing that this spectrum is formed by light which comes from an object of which the image in the telescope is a point. Lord Rosse remarks of this nebula, "A very remarkable object, perhaps analogous to H. 450." The color of this nebula is greenish blue.

[51 H. IV. A planetary nebula in Sagittarius. Bright; very small; round.] This nebula is less bright than those which have been described. The two brighter of the lines were well defined, and were directly compared with the induction spark. The third line was seen only by glimpses. I had a suspicion of an exceedingly faint spectrum. The color of this nebula is greenish blue. Lord Rosse remarks, "Center rather dark. The dark part is a little north preceding the middle.

[1 H. IV. An exceedingly interesting object in Aquarius. Planetary; very bright; small; elliptic.] The three bright lines very sharp and distinct. They were compared for position with the induction spark. Though this object is bright, an indication only of the faint spectrum was suspected. This nebula contains probably a very small quantity of matter condensed into the liquid or solid state. The color of the light of this nebula is greenish blue. Lord Rosse has not detected any central star, nor any perforation, as seen in some of the other planetary nebulæ. He represents it with ansæ, which probably indicate a nebulous ring seen edgeways.

[57 M. An annular nebula in Lyra; bright; pretty large; considerably elongated.]—The apparent brightness of this nebula, as seen in the telescope, is probably due to its large extent, for the faintness of its spectrum indicates that it has a smaller intrinsic brightness than the nebulæ already examined. The brightest of the three lines was well seen. I suspected also the presence of the next in brightness. No indication whatever of a faint spectrum. The bright line looks remarkable, since it consists of two bright dots corresponding to sections of the ring, and between these there was not darkness, but an excessively faint line joining them. This observation makes it probable that the faint nebulous matter occupying the central portion is similar in constitution to that of the ring. The bright line was compared with the induction-spark.

[18 H. IV. Planetary; very bright; pretty small, round, blue.]—With a power of 600 this nebula appears distinctly annular. The color of its light is greenish blue. The spectrum formed by the light from this nebula corresponds with that of 37 H. IV. In the spectrum of this nebula, however, in addition to the three bright lines, a fourth bright line, excessively faint, was seen. This line is about as much more refrangible than the line agreeing in position with F as this line is more refrangible than the brightest of the lines, which coincides with a line of nitrogen.

[27 M. Very bright; very large; irregularly extended. Dumb-bell. In Vulpecula.]—The light of this nebula, after passing through the prisms, remained concentrated in a bright line corresponding to the brightest of the three lines. This line appeared nebulous at the edges. No trace of the other lines was perceived, nor was a faint continuous spectrum detected. The bright line was ascertained, by a simultaneous comparison with the spectrum of the induction spark, to agree in position with the brightest of the lines of nitrogen. Minute points of light have been observed in this nebula by Lord Rosse, Otto Struve, and others; the spectra of these bright points, especially if continuous like those of stars, are doubtless invisible from excessive faintness. By suitable movements given to the telescope, different portions of the image of the nebula formed in the telescope were caused successively to fall upon the opening of the slit, which was about $\frac{1}{16}$ inch by $\frac{1}{300}$ inch. This method of observation showed that the light from different parts of the nebula is identical in refrangibility, and varies alone in degree of intensity.

In addition to these objects the following were also observed:

[92 M. Very bright globular clusters of stars in Hercules.] The bright central portion was brought upon the slit. A faint spectrum similar to that of a star. The light could be traced from between C and D to about G. Too faint for the observation of lines of absorption.

[50 H. IV. Very bright; large; round. In Hercules.] The spectrum similar to that of a faint star. No indication of bright lines.

[31 M.] The brightest part of the great nebula in Andromeda was brought upon the slit. The spectrum could be traced from about D to F. The light appeared to cease very abruptly in the orange; this may be due to the smaller luminosity of this part of the spectrum. No indication of the bright lines.

[32 M. Very bright; large; round; pretty suddenly much brighter in the middle.] This small but very bright companion of the great nebula in Andromeda presents a spectrum apparently exactly similar to that of 31 M. The spectrum appears to end abruptly in the orange; and throughout its length is not uniform, but is evidently crossed either by lines of absorption or by bright lines.

[55 Androm. Fine nebulous star with strong atmosphere.] The spectrum apparently similar to that of an ordinary star.

[26 IV. Very bright cluster in Eridanus.] The spectrum could be traced from the orange to about the blue. No indication of the bright lines. Several other nebulæ were observed, but of these the light was found to be too faint to admit of satisfactory examination with the spectrum apparatus.

It is obvious that the nebulæ 37 H. IV, 6 Σ., 73 H. IV, 51 H. IV, 1 H. IV, 57 M, 18 H. IV, and 27 M, can no longer be regarded as aggregations of suns after the order to which our own sun and the fixed stars belong. We have in these objects to do no longer with a special modification only of our own type of suns, but find ourselves in the presence of objects possessing a distinct and peculiar plan of structure.

In place of an incandescent solid or liquid body transmitting light of all refrangibilities through an atmosphere which intercepts by absorption a certain number of them, such as our sun appears to be, we must probably regard these objects, or at least their photo-surfaces, as enormous masses of luminous gas or vapor. For it is alone from matter in the gaseous state that light consisting of certain definite refrangibilities only, as is the case with the light of these nebulæ, is known to be emitted.

Such gaseous masses would be doubtless, from many causes, unequally dense in different portions; and if matter condensed into the liquid or solid state were also present, it would, from its superior splendor, be visible as a bright point or points within the disk of the nebula. These suggestions are in close accordance with the observations of Lord Rosse.

Another consideration which opposes the notion that these nebulæ are clusters of stars is found in the extreme simplicity of constitution which the three bright lines suggest, whether or not we regard these lines as indicating the presence of nitrogen, hydrogen and a substance unknown.

It is perhaps of importance to state that, except nitrogen, no one of thirty of the chemical elements the spectra of which I have measured has a strong line very near the bright line of the nebulæ. If, however, this line were due to nitrogen, we ought to see other lines as well; for there are specially two strong double lines in the spectrum of nitrogen, one at least of which, if they existed in the light of the nebulæ, would be easily visible. In my experiments on the spectrum of nitrogen, I found that the character of the brightest of the lines of nitrogen, that with which the line in the nebulæ coincides, differs from that of the two double lines next in brilliancy. This line is more nebulous at the edges, even when the slit is narrow and the outer lines are thin and sharp. The same phenomenon was observed with some of the other elements. We do not yet know the origin of this difference of character observable among lines of the same element. May it not indicate a physical difference in the atoms, in connexion with the vibrations of which the lines are probably produced? The speculation presents itself, whether the occurrence of this one line only in the nebulæ may not indicate a form of matter more elementary than nitrogen, and which our analysis has not yet enabled us to detect.

ART. X.—*Reactions of Gelatine;* by M. CAREY LEA, Philadelphia.

I HAVE been occupied at times for some years past with the study of this very interesting substance, and propose here to describe a new reaction which I have observed, and which constitutes, I believe, the first colored reaction described as produced between pure gelatine and a perfectly colorless reagent. It is true that the precipitate produced in gelatine solutions by gallotannic acid is much deeper in color than the precipitant. But the straw-yellow color of gallotannic acid naturally leads to the expectation of colored combinations, whereas in the case I am about to mention, the precipitant is colorless, and the production of a marked color seems to point to a more complete action than that of simple combination.

When a piece of gelatine is dropped into an acid solution of pernitrate of mercury, it gradually assumes a strong red coloration, and after a time, dissolves in it completely at ordinary temperatures, to a fine red solution. This solution deepens a little if boiled for some minutes. By chlorate of potash, the hot solution is quickly decolorized, and passes to pale dirty yellow.

This red coloration seems to require a certain amount of time for its production, which cannot be replaced by heat. If a piece of gelatine be immersed in the solution of protonitrate and boiled for some minutes it is dissolved but the solution thus obtained is not red, but yellowish.

It is to be regretted that the reaction here described is not more delicate. It is only striking when tolerably strong solutions of gelatine are employed. When the solution is very weak, as for example, if the gelatine constitutes only one half of one per cent of the mixed liquids, the limit of the delicacy of the test is reached. Such a solution, by standing twenty-four hours, exhibits a light but distinct pink color. Although this delicacy is not what may be desired, still, colloid organic substances are so comparatively difficult of qualitative detection as a general thing, that the method is not without value.

The experiment was next extended to *metagelatine.* A neutral metagelatine was prepared in the following manner.

Gelatine was set to swell in cold saturated solution of oxalic acid, and then a moderate heat was applied for a sufficiently long time for the mass to remain quite fluid when cold. It was then agitated with precipitated carbonate of lime until the whole of the oxalic acid was got rid of. Metagelatine prepared in this way was kept for months in a corked vial in a warm room without showing any disposition to putrefy. It was almost as fluid as water: perfectly neutral and almost insipid to the taste.

With this metagelatine, the red coloration was produced even more decidedly than with ordinary gelatine. The addition of the acid solution of per-nitrate of mercury produced at first a whitish flocculent precipitate, which, by standing, acquired a strong red color, as did the supernatant liquid.

Philadelphia, May 12, 1865.

ART. XI.—*Influence of Gravity on Magnetic Declination;*[1] by PLINY EARLE CHASE, M.A., S.P.A.S.

IN my first communication on the diurnal variation of the barometer, [Proceedings A. P. S., ix, 284], I expressed the belief that a careful investigation would "show a mutual connection through which all the secondary [disturbing] causes may be referred to a single force." In my various subsequent papers, and especially in the one to which the Magellanic Premium was awarded, [op. citat. and Trans. A. P. S., vol. xiii, N. S., Art. VI], I pointed out various reasons for supposing that the primal unitary force is the same that controls the motions of the several stellar systems; in other words, the force of gravitation, or perhaps of simple undulation, which is manifested as heat in one of its subordinate forms, and as attraction in another. The numerical relations which I demonstrated between the disturbances of weight and of total magnetic force were certainly noteworthy, and to my own mind, extremely satisfactory, and as further investigations have afforded additional confirmation of my views, I desire to put upon record a brief notice of the general harmony which mutually characterizes the gravitation currents and the variations of magnetic declination.

Preliminary investigations showed, as might have been reasonably anticipated, that the best quantitative results can be obtained from the observations at stations near the equator, and I therefore based my reasoning in great measure upon the St. Helena records and Maj. Gen. Sabine's discussions, confirming it by such incidental references to other observations, as seemed available for the purpose. At the same time allusion was made [Trans. A. P. S., loc. citat., p. 132,] to researches now in progress, which may probably enable us to discover numerical relations, that will be equally satisfactory, from an examination of the observations in higher latitudes. While patiently and confidently awaiting the completion of those researches it may be well to present some considerations which will serve both as a corroboration of my own views and as a guide to the investigations of others.

The discussions of the magnetic and meteorological observations at Girard College, [Coast Survey Reports, and Smithsonian Contributions], should be specially interesting to all Americans, and they are also among the most recent and valuable publications on terrestrial physics. From them I quote the following references to the most important and best established normal disturbances of declination.

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[1] From the Proceedings of the Amer. Philosoph. Soc., April 21, 1865.

I. "The annual variation depends on the earth's position in its orbit, the diurnal variation being subject to an inequality depending on the sun's declination. The diurnal range is greater when the sun has north declination, and smaller when south declination; the phenomenon passing from one state to the other about the time of the equinoxes." [Part II, p. 10. See also Toronto Obs., 2, xvii; St. Helena Obs., 2, cxviii].

II. "At the hour of 6 or 7 in the morning, the annual variation is a maximum, disappearing at a quarter before 10 A. M., and reaching a second (secondary) maximum value at 1 P. M. It almost disappears soon after 5 P. M., and a third still smaller maximum is reached after 9 P. M. Half an hour before midnight, the annual variation again disappears. At (and before and after) the principal maximum, between 6 and 7 in the morning, the annual variation causes the north end of the magnet to be deflected to the east in summer and to the west in winter; at 1 P. M. the deflections are to the west in summer and to the east in winter. The range of the diurnal motion is thus increased in summer and diminished in winter; the magnet being deflected in summer more to the east in the morning hours, and more to the west in the afternoon hours, or having greater elongations than it would have if the sun moved in the equator. In winter, the converse is the case." [Ibid., p. 12. Compare St. Helena Obs., 2, cxviii; Toronto Obs., I, xiv, and 2, xvi.]

III. "According to the same authority," [Gen. Sabine], "the *annual* variation is the *same* in both hemispheres, the north end of the magnet being deflected to the east in the forenoon, the sun having north declination; when in the *diurnal* variation, the north end of the magnet at that time of the day is deflected to the east in the northern hemisphere and to the west in the southern hemisphere; in other words, in regard to the direction the law of the annual variation is the same, and that of the diurnal variation the opposite in passing from the northern to the southern magnetic hemisphere." [Ibid., p. 13. Comp. St. Hel. Obs., 2, lxxx, cxviii.]

IV. "The regular progression of the monthly values is a feature of the annual variation deserving particular notice. There is no sudden transition from the positive to the negative side, or *vice versâ*, at or near the time of the equinoxes (certainly not at the vernal equinox); on the contrary, the annual variation seems to be regular in its progressive changes. The method here pursued is entirely different from that employed by General Sabine for the same end, but the results are, nevertheless, in close accordance." It has been found that the transition takes place "*ten* days after either equinox, and also that the turning points occur ten days after the solstices." [Ibid., p. 14. Comp. St. Hel. Obs., 2, cxx.]

V. "The general character of the diurnal motion . . . is nearly the same throughout the year; the most eastern deflection is reached a quarter before 8 o'clock in the morning (about a quarter of an hour earlier in summer, and half an hour later in winter), . . . the north end of the magnet then begins to move westward, and reaches its western elongation about a quarter after one o'clock in the afternoon (a few minutes earlier in summer). . . . The diurnal curve presents but a single wave, slightly interrupted by a deviation occurring during the hours near midnight, (from about 10 P. M. to 1 A. M.), when the magnet has a direct or westerly motion; shortly after 1 A.M. the magnet again assumes a retrograde motion and completes the cycle by arriving at its eastern elongation shortly before 8 o'clock in the morning. This nocturnal deflection is well marked in winter, vanishes in the summer months, and is hardly perceptible in the annual curve. According to the investigations of General Sabine, it is probable that if we had the means of entirely obliterating the effect of disturbances, this small oscillation would almost disappear. In summer, when it has no existence, the magnet remains nearly stationary between the hours of 8 P. M. and 3 A. M., a feature which is also shown by the annual type-curve." {Ibid., p. 20. Comp. Hobarton Obs., 2, vi; St. Hel. Obs., 2, cxi, cxix, cxx; Toronto Obs., I, xiv, 2, xvi.]

VI. "The critical hours which vary least during the year are those of the western elongation and those of the morning mean declination. The extreme difference between the value for any month and the mean annual value, is 31 minutes in the former and 28 minutes in the latter." [Ibid., p. 21.]

VII. The curves of lunar-diurnal variation "show two east and two west deflections in a lunar day," the westerly maxima "occurring about the upper and lower culminations," and the easterly maxima "at the intermediate six hours. The total range hardly reaches 0′·5. These results agree generally with those obtained for Toronto and Prague." [Part. III, p. 8. Comp. St. Hel. Obs., 2, xxiii, lxxxii, cxliv; Toronto Obs., 3, lxxxv.]

VIII. "In comparing the easterly and westerly curves, the constant in Bessel's formula comes out zero, and hence it is inferred that the moon has no specific action in deflecting the magnet by a constant quantity." [Ibid., p. 10.]

IX. "If we take the four phases into account, the lunar action seems to be retarded 10 minutes, which quantity may be termed the *lunar-magnetic* interval for the Philadelphia Station. At Toronto the intervals are not so regular." [Ibid., p. 11.]

X. "The characteristic feature of the annual inequality in the lunar-diurnal variation is a much smaller amplitude in winter than in summer. Kreil, indeed, inferred from the ten year series of the Prague observations, that in winter the lunar-diurnal variation either disappears, or is entirely concealed by irregular fluctuations, requiring a long series for their diminution. The method of reduction which he employed was, however, less perfect than that now used. The second characteristic of the inequality consists in the earlier occurrence of the maxima and minima in winter than in summer. The winter curve precedes the summer curve by about one and three-quarter hours." [Ibid., p. 12–13.]

XI. The curves of semi-annual variation at all points where continuous observations have been made, present a striking similarity, the amount of deflection being nearly equal in all parts of the globe. [See diagram, Part II, p. 12. Comp. St. Hel. Obs., 2, cxix; Toronto Obs., 2, xvi, xvii.]

Now it is evident that the mechanically-polarizing currents (Proc. A. P. S., ix, 367 sqq.) must be variously deflected at different periods of the day, in consequence of gravitation-disturbances arising both from the varying distance of the sun and from solar heat. Although it may sometimes be desirable to consider the so-called thermal currents apart from those which would be produced independently of any change of temperature, such a distinction is unnecessary in a preliminary qualitative investigation like the present, because the periods both of maxima and of minima are precisely coincident in the two currents (except as they may be slightly modified by the earth's radiant heat), and because *even the thermal currents are occasioned simply and solely by the varying gravitation* of fluids of varying density.

Regarding, therefore, the air and æther over any given magnetic meridian, during the day hours the intertropical and during the night the extra-tropical portions will be most drawn toward the sun, and the following deflections will be thus produced in the portions nearest the equator:

	6 to 12 A. M.	12 to 6 P. M.	6 to 12 P. M.	12 to 6 A. M.
Northern zones,	S.E.	S.W.	S.E.	S.W.
Southern "	N.E.	N.W.	N.E.	N.W.

The night-disturbances, whether from variations of temperature or from simple fall toward the sun (the distance fallen varying as the square of the time from midnight), will be very slight. The earth's rotation, centrifugal force, and the atmospheric inertia, tend to throw each of the phases forward and to increase the magnitude of the westerly, while they diminish the easterly deflections. If these modifications were sufficient to override the slight easterly tendency at 6 to 12 P. M., and to advance the phases one hour, the disturbances would assume the following forms, the change between 7 P. M. and 1 A. M. being scarcely, if at all, perceptible:

	7 A.M. to 1 P.M.	1 P.M. to 7 A.M.
Northern zones,	S.E.	S.W.
Southern "	N.E.	N.W.

At the equinoxes, the amounts of deflection in the northern and southern magnetic hemispheres should be equal; at other seasons the shortest lines would suffer the greatest displacement, the deflections being greatest in the northern zones from April to September, when the sun is in the northern signs, and in the southern zones from October to March, when the sun is in the southern signs. My experiments have shown that the compass-needle sympathizes with, and is to some extent controlled by, purely mechanical vibrations, and if in obedience to such control it should tend to parallelism with the æthereal currents, a westerly disturbance of declination (the declination being always conventionally referred to the north pole of the needle) would correspond either to an equatorial southeasterly deflection of the southern or a northwesterly deflection of the northern extremity of a half-meridian, and an easterly disturbance to a southwesterly deflection of the southern or a northeasterly deflection of the northern extremity.

Substituting these declination values for the current-deviations to which they correspond, the almost *precise* accordance of theory and observation in the prominent features of the normal variations of declination, may be seen by a reference to the following table:

Daily maximum,[1] Half-yearly " April to Sept.[2] " " " Oct. to Mar.[2]	Easterly. Easterly. Westerly.	Westerly. Westerly. Easterly.	Morning. Mean.	Evening. Mean.	Stationary.
Theoretical, - - - - -	7 A.M.	1 P.M.	10 A.M.	4 P.M.	7 P.M. to 1 A.M.
Observed [I to V],[3] - -	6–8 "	1 "	9¼ "	5 "	8 " " 3 "

Gen. Sabine, in speaking of the opposition of the annual and semi-annual curves (St. Hel. Obs., 2, cxix), says, "these remarkable systematic dissimilarities may be regarded as sufficient indications of a difference in the *mode of operation* of the solar influence in the two cases." I am not aware that any attempt has hitherto been made to explain this apparent difference, or to show that it is only apparent, and may result from the action of a uniform law. I believe that I have now given the needed explanation, and since I have shown experimentally that the phenomena are such as *should* be produced by gravitation, it is reasonable to assume that they probably *are* so produced. The probability is increased by the disappearance of the night oscillation in summer (V), the probability that when it is observed it results from thermal disturbances, and the greater stability of those critical hours which are nearest to the hours of maximum sun-ward gravitation (VI). The precise coincidence both in time and direction of the lunar-diurnal declination and tidal curves (VII), the unavoidable inference that the moon has no constant or specific magnetic action (VIII), the "establishment" of ten minutes at the Philadelphia station (IX), the correspondence of the lunar and solar curves in the diminished winter amplitude (X), and the uniformity at all stations of the semi-annual variation-curves (XI), are all necessary corollaries of my hypotheses.

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[1] In the northern zones.

[2] Over the whole earth.

[3] The bracketed references are to the numbered quotations from the Girard College discussions.

The demonstration of a connection between the daily and annual variations of magnetism and gravitation would be incomplete, if our theory could not be so extended as to explain the decennial and secular changes. For such an extension we are compelled to wait until further study and observation have more precisely defined the character and value of those changes, and suggested all the important gravitation-disturbances of long period to which they may be plausibly referred. We may find, however, in the attraction of Jupiter, one of the possible causes of the 10–11 year period, while nutation, precession, geological upheaval and depression, change of seasons, accumulation or diminution of polar ice, and the shifting position of the center of gravity of our planetary system, must all necessarily contribute to the production of gradual changes in the terrestrial gravitation currents. The belief does not therefore seem unreasonable, that the feeble vibrations of the tremulous needle may not only furnish us with a delicate scale for weighing (as we have already approximately done) the huge mass of the sun, but they may also aid us in the discovery and verification of other important cosmical phenomena, and the assignment of their appropriate laws.

Although it is probable that terrestrial magnetism is mainly owing to currents circulating above the surface, it is well known that there are also earth-currents which exert an appreciable modifying influence. I am confident that they will be found equally obedient to the laws of gravitation, which affect every particle of the earth's body, modifying the crystalline-polarity and cohesion of solids as well as the flow of liquids, and producing internal tides, which may contribute largely to that metamorphism of stratified rocks which has been referred by geologists to the agency of heated fluids and vapors. (See Rogers, Pa. Report, ii, 700; Lyell, this Journal, [2], xxxix, 22.)

The inclination presents some anomalies that are difficult to explain, and whether we compare the solar-diurnal or the annual curves at the principal northern and southern stations, the "indications of a difference in the *mode of operation* of the solar influence in the two cases" seem as striking and perplexing as they did to Hudson and Herschel in their examination of the influence of heat on the barometer (Proc. A. P. S., ix, 283), and to Sabine in his discussions of the semi-annual declination curves (St. Hel. Obs., 2, cxix). But the disappearance, in the progress of our investigations, of these once seemingly insurmountable difficulties,—the wonderful coincidence in the general features of the gravitation and magnetic currents,—and the *a priori* probability that all disturbed forces, of whatever character or variety, will tend constantly to a mutual equilibrium,—encourage the belief that this apparent paradox may be likewise susceptible of a simple interpretation, which will drive it from its latest lurking place.

I can think, at this moment, of no more probable causes of the want of symmetry here spoken of, than the different distribution of land and water in the two hemispheres, and the influence of powerful alternating land and sea breezes. A long series of connected observations at a number of new stations may perhaps be required, before it can be satisfactorily ascertained whether the disturbances thus occasioned are sufficient to account for all the phenomena, but meanwhile it is interesting to observe the degree of accordance that exists, at northern inland stations like Philadelphia and Toronto, between the curves of vertical force and force of wind on the one hand, and those of horizontal force and barometric pressure on the other, as well as the agreement that has been pointed out by Dr. Lloyd and Mr. Homer, between the annual curves of declination and of temperature.

ART. XII.—*Researches on the Volatile Hydrocarbons;* by C. M. WARREN.[1]

Introductory Remarks.—While engaged, a few years since, in attempting to separate some of the constituents of coal-tar naphtha by the common process of fractional distillation, I was forced to the conviction that that process could not be safely relied upon for anything like a complete and accurate analysis of such a complex mixture of liquids; and that, at best, the products thus obtained could not be regarded as anything better than remote approximations to pure substances; leaving reason to fear that there might still be other bodies present, in less quantities perhaps, which had escaped detection.

An examination of the results of previous researches on tars, petroleums, etc., served in general to confirm the impressions induced by my own less extended experiments; and to increase, rather than lessen, the doubts already existing in my mind as to the trustworthiness of the results which had hitherto been published concerning the neutral constituents of such mixtures. Influenced by these considerations, and by the belief that, if I could succeed in finding a process capable of effecting a more complete separation of the constituents of such mixtures, it might probably lead to the discovery of new bodies, lying between those which had already been described,—I was led to undertake the researches, the results of which I am about to record. Even if this chief purpose should fail, I was convinced that the expenditure of labor in isolating those bodies in a state of greater purity, would be amply compensated by the much needed confirmation, or perhaps correction, of the results previously published, in addition to the valuable incidental evidence of the absence of other bodies which would thus be furnished. The results which I have obtained in the pursuit of this object are abundantly sufficient to show that I did not undervalue the work of my predecessors, nor over-estimate the importance of the work before me.

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[1] From the Memoirs of the American Academy, (N. S.) ix, 135.

The success which attended my efforts in search of a better process of separation has already been described in detail, in a memoir "On Fractional Condensation," etc. (Memoirs of the American Academy, 1864, and the last volume of this Journal.)

This new process was first applied, more especially for the purpose of testing its efficiency in the separation of benzole from coal-tar naphtha. This mixture was selected for the test on account of the property which benzole possesses, in contradistinction from its associates, of being crystallizable at a low temperature, thus affording an additional test of the purity of the product which might be obtained by the process of fractioning. Somewhat to my surprise I found that, after only the fifth series of fractionings, I had obtained benzole so nearly pure that the whole of it would distil from a tubulated retort between 80° and 81° C.; and that when congealed, which was effected by placing the containing bottle in pounded ice, not a drop of liquid could be poured from the mass of crystals. From this result,—which, at least, indicated a near approximation to purity,—taken in connection with other favorable indications, I felt confident that I had accomplished my first object, and had found a process that could, in all probability, be successfully applied in the study of the petroleums, which up to that time (1861) had baffled every attempt to resolve them into their proximate constituents.

Being naturally anxious to apply the new process in this seemingly more promising field of inquiry, I at once suspended, for the time being, my operations on coal-tar naphtha, and commenced simultaneously the investigation of Pennsylvanian petroleum, and of the oils distilled from Albert coal (from Hillsboro, New Brunswick) in the process of manufacturing illuminating oil. These two substances, neither of which had ever been made the subject of special scientific investigation, were selected as being fair representative types, on the one hand of the native liquid petroleums, and on the other of the artificial coal oils. The comparative study of these two substances seemed to promise additional interest on account of the close analogies which they present, especially when this circumstance is considered in connection with the fact of their great diversity of origin. This is the limit which at that time was assigned for these researches; my intention being, so soon as the separations and analyses should be completed, and the boiling-points and some of the other more important physical characteristics determined, briefly to publish the results, together with the process of fractioning,—preliminary to a complete memoir at a more advanced stage of the work. Before this work had been accomplished, however, it became evident that the bodies contained in these mixtures could not be studied so satisfactorily by themselves as in comparison with other series of hydrocarbons, especially with reference to certain important questions of more general interest; for example, the question in regard to the increment of boiling-point corresponding to the addition of C_2H_2 in homologous series. It was therefore deemed advisable to extend the inquiry so as to include the naphthas from coal- and wood-tars, and the oil of cumin. And there are still other mixtures of hydrocarbons, that have been made the subject of previous research which must yet be brought into the works, in order to clear up, in a satisfactory manner, the confusion and obscurity that seem to exist in our publications regarding some questions relating to the different series of this class of bodies.

This digression from my original plan having caused much additional labor has necessarily delayed publication longer than was desirable, until now the results of more than three years of work have accumulated. In this connection I may remark, so far as regards petroleum, that I had nearly completed the fractional separations—except of the bodies of high boiling-point—so long ago as June, 1862, having been for a long time occupied with this work before the appearance, in that month, of the first memoir of Pelouze and Cahours on the same subject. At that time my work was considerably in advance of theirs, and their results differed widely from mine in some important particulars; yet after the publication of their memoir I felt reconciled to a continuance of the delay which had been caused by the change of plan above mentioned, considering it due to these chemists that they should have time to complete the publication of the results of their investigations before I should publish mine. Similar remarks might be made respecting the first publication of Schorlemmer, which appeared soon after, on the products of distillation of cannel coal; this substance being so closely analogous to the Albert coal (upon the products of which I had at that time been long engaged) as to induce the belief that, under the same circumstances, either would afford the same products.

I.—ON THE VOLATILE HYDROCARBONS FROM COAL-TAR NAPHTHA, OIL OF CUMIN, AND CUMINIC ACID.

PART I.—*Hydrocarbons from Coal-tar Naphtha.*

In presenting the results of a re-examination of a series of substances upon which so much labor had already been bestowed, and upon the nature and properties of which so little doubt has seemed to exist, it may confer an interest on the subject to state briefly some of the more important results and conclusions that previous investigators have arrived at in the study of these substances.

The discovery by Faraday,[2] in 1825, of benzole ("bicarburetted hydrogen") in the oil compressed from oil-gas, rendered it highly probable, and indeed led this distinguished philosopher to suspect, that this substance might be found in coal-tar naphtha. His search for it, however, proved unsuccessful, it having been first detected by Hofmann in 1845.[3] This chemist, however, did not attempt to isolate this body, and the bare fact of its presence appears to be all that was definitely known of the composition of coal-tar naphtha prior to 1849, in which year Mansfield[4] published his elaborate and valuable research, being the first effort at a proximate analysis of this mixture which appears to have been attended with any considerable measure of success. Although a fatal accident, while engaged in his experiments, prevented Mansfield from completing the investigation which he had so well begun, yet the work that he had already published in an unfinished state must always be regarded as having contributed much towards a clear and definite knowledge of the nature of the neutral pyrogenous oils contained in coal-tar naphtha. Indeed, it may be said that little has since been added to our knowledge on this subject. Notwithstanding the incompleteness of his separations of the hydrocarbons, the extent to which he had carried them with the limited means employed is truly remarkable, and could not have been accomplished without an expenditure of labor, and a degree of patient endurance, which only those who have experienced the tediousness of such operations can appreciate.

Mansfield claimed to show that the light coal-tar naphtha is composed of a mixture of four distinct hydrocarbons, boiling within the range of 80° to 175° C.; and probably having the general formula $C_n H_{n-6}$. The first of these, which he found to boil constant at 80°, was proved to be identical with benzole, $C_{12}H_6$. The second, boiling at about 113°, was determimed, from certain reactions, to be identical with toluole, $C_{14}H_8$. The special study of this body was deferred, however, with the remark that it had not yet been isolated in a state of sufficient purity to claim an analysis. The third body, boiling at about 140° to 145°, was said to present all the characteristics of cumole, $C_{18}H_{12}$; but this view was not founded on a careful study and comparison of the chemical or physical properties of these bodies, but was merely an expression of opinion in advance of anticipated results. Of the fourth body, boiling at about 170° to 175°, Mansfield remarks that it bears so strong a resemblance, in odor and other properties, to cymole, $C_{20}H_{14}$, as to induce the belief that this substance is identical with the hydrocarbon existing in oil of cumin. It thus appears that of the four bodies which Mansfield detected in coal-tar naphtha, benzole is the only one which he had studied in any detail. Indeed he distinctly states that the others had not yet been isolated in such a state of purity as to entitle them to analysis. And yet his conjectures as to the identity of these bodies, thrown out by way of preliminary notice of results which were acknowledged to be incomplete and inaccurate, have nevertheless been extensively quoted, and generally received as established facts. In addition to the bodies already mentioned, Mansfield also detected the presence of a body more volatile than benzole, having an alliaceous odor, which he found to boil between 60° and 70°. Ritthausen[5] made a re-examination of the light coal-tar naphtha, in order to obtain the hydrocarbons in a state of greater purity, and to prove the correctness of Mansfield's view of the composition of this naphtha. In regard to the results which he obtained, he says they fully confirm those of Mansfield. Of the body which Mansfield designated as probably identical with cymole, and of the oil more volatile than benzole, Ritthausen obtained quantities too small to admit of investigation. In regard to the latter, however, he remarks,[6] that to Mansfield's account he can add, that "its nitro-product quite resembles that of benzole, and hence that at all events it belongs to the series $C_n H_{n-6}$, and perhaps has the formula $C_{10}H_4$."[7] It is to be regretted that Ritthausen also omitted to analyze and determine the vapor density of any one of these substances, he having added, therefore, nothing more than a confirmation of the results of Mansfield. He gives the boiling-point of benzole at 80°, of toluole at 109°, and of the so-called cumole at 139°–140°, which will be found to agree very nearly with my own determinations. Church,[*] in the following year, published a paper on the "Determination of Boiling-points" in the "Benzole Series." I cannot better present his results than by quoting the following table:—

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[2] Philosophical Transactions of the Royal Society, 1825, cxv, 465.

[3] Annalen der Chemie und Pharmacie, 1845, lv, 200.

[4] Quarterly Journal of the Chemical Society, 1849, i, 244.

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[5] Journal für praktische Chemie, 1854, lxi, 74.

[6] "Ich kann den Angaben von Mansfield über das letztere nur das hinzufügen, das seine Nitroproducte denen des Benzols, etc. ganz ähnlich sind, daher es jedenfalls der Reihe C_nH_{n-6} angehört und vielleicht die Formel $C_{10}H_4$ besitzt.

[7] On a future occasion I shall show that Ritthausen was in error in placing this body in the benzole series, and indeed in considering it as a hydrocarbon at all. He was evidently deceived by operating on a mixture containing benzole. Furthermore, as Mansfield suggested might be possible, that part of the naphtha more vola-

	Formula.	Boiling-point.	Difference.
Benzole,	$C_{12}H_6 = C_6\ 3(C_2H_2)$	80°·8	22°·9
Toluole,	$C_{14}H_8 = C_6\ 4(C_2H_2)$	103°·7	22°·5
Xylole,	$C_{16}H_{10} = C_6\ 5(C_2H_2)$	126°·2	22°·2
Cumole,	$C_{18}H_{12} = C_6\ 6(C_2H_2)$	148°·4	22°·3
Cymole,	$C_{20}H_{14} = C_6\ 7(C_2H_2)$	170°·7	

Church states that he obtained all of these bodies from coal-naphtha, and also that he obtained benzole from benzoic acid, toluole from toluylic acid, xylole from woodspirit, cumole from cuminic acid, and cymole from oil of cumin; and that he has found the corresponding bodies from these different sources to be identical. It will be observed that Church claims to have found in coal-tar a body boiling at 126°·2, which he calls *xylole*, thus supplying from this source a fifth member of the benzole series; whereas Mansfield and Ritthausen found only four bodies within the range of temperature indicated by the table. It will also be observed that his determination of the boiling-point of toluole is much lower, and that of cumole much higher, than the corresponding determinations of Mansfield and Ritthausen; thus giving room for a middle member between them, and preserving a remarkable uniformity of difference—viz. 22° and a fraction—between the boiling-points of any two contiguous members of the series, for the addition of C_2H_2.

That the earlier investigators had found in coal-tar naphtha only the two lower members ($C_{12}H_6$ and $C_{14}H_8$) and the two upper members ($C_{18}H_{12}$ and $C_{20}H_{14}$), indicating the absence of the middle member ($C_{16}H_{10}$) of the benzole series, was always to me an anomaly which I could not reconcile with any plausible theory in regard to the formation of these bodies; and I was led, therefore, to question whether this body had not been overlooked in making the separations. The alleged discovery of this body in coal-naphtha by Church, together with

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tile than benzole is by no means composed of a single substance. Having had a large quantity of this volatile material at my command, I have been able to obtain the separate constituents apparently in a state of great purity. Of the two bodies separated, one of them boils constant at about 40°, and the other near 0°. Both are compounds containing sulphur, and therefore will more properly form the subject of a separate paper.

[*] Philosophical Magazine, 1855, [4], ix, 256.

the beautiful uniformity of the boiling-point difference throughout the series which he presented, and the apparent care with which the whole research had been conducted, led me to regard his results as being more reliable than those which had previously been published. I remained under this conviction until I had discovered the boiling-point difference of 30° in other series of hydrocarbons,[9] which led me to doubt the accuracy of Church's determinations of boiling-points, and to consider those of Mansfield and Ritthausen as probably more correct.

In the first paragraph of his paper, Church remarks that, "although doubts still remain as to the relations of these bodies to one another, yet their composition has been ascertained with certainty." It does not appear, however, that an analysis or vapor density of any one of the members of this series, as obtained from coal-tar, except benzole, had ever been published.

As already indicated by the title of his paper, it appears to have been the design of Church to treat only of the boiling-points of these bodies; yet finding that his preparations of toluole—prepared both from coal-naphtha and toluylic acid—gave a boiling-point differing considerably from observations previously published, he took occasion to make analyses of his preparations of this substance, which he regards as "perfectly satisfactory;" and adds that "the details and numerical results of these analyses, and of many others which the present inquiry necessitated, the limits and special object of the present paper do not admit of my giving here." As he undertook to correct the work of his predecessors, to do which fairly would seem to require the publication of these "details and numerical results," their omission is to be regretted, the more since he found space and purpose for matter apparently less relevant to his special subject. I am prompted to these remarks from having been led to undertake the tedious task of making a re-examination of coal-tar naphtha mainly on account of the disagreement between Church's determinations, which I have found to be mostly incorrect, and those which had been previously published.

In addition to the bodies mentioned in the foregoing table, Church alludes to the discovery of two other bodies, boiling respectively at 97° and 112°. Subsequently, in a "Note on Parabenzole, a new Hydrocarbon from Coal-Naphtha,"[10] he publishes the details of an investigation of the former of these two bodies, which he finally found to boil "*perfectly constant at* 97°·5," and to be isomeric with benzole.

I think I shall be able to show in the following pages,—

1. That coal-tar naphtha contains only four hydrocarbons within the range of 80° to 170°, as taught by Mansfield, and confirmed by Ritthausen.

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[9] See a following Memoir on this subject.

[10] Philosophical Magazine, 1857, [4], xiii, 415.

2. That the benzole series within that range of temperature is limited to four members, and therefore does not contain five, as has been generally supposed.

3. That these four members have the boiling-points 80°, 110°, 140°, and 170 respectively; and consequently that the boiling-point difference in this series, for an elementary difference of C_2H_2, is 30°, instead of 22° and a fraction as alleged by Church.

4. That the body obtained from coal-tar naphtha, boiling at 140°, is not identical with cumole from cuminic acid, as assumed by Mansfield, nor even isomeric with it; but that it has the formula which has been assigned to xylole, containing C_2H_2 less than that of cumole.

5. That the body obtained from coal-tar naphtha, boiling at 170°, is quite a different body from cymole obtained from oil of cumin,—with which it has been considered identical, as assumed by Mansfield,—these bodies differing from each other by C_2H_2.

6. That cumole from cuminic acid, and cymole from oil of cumin, do not even belong to the benzole series.

7. That the Parabenzole of Church was in all probability only a mixture of benzole and toluole.

Of the Quality of Naphtha employed in this Investigation.—As I have taken occasion to question the existence in coal-tar naphtha of two of the substances which it has been said to contain, —viz. cymole, $C_{20}H_{14}$, and parabenzole, $C_{12}H_6$,—it is a matter of some importance that I should clearly state the kind or quality of the naphtha employed. The tar from which this naphtha was obtained was a mixture of the tar furnished by the following companies, viz. the New York and the Manhattan Gas-Light Companies, of New York; Brooklyn Gas-Light Company, of Brooklyn, N. Y.; Albany Gas-Light Company, of Albany, N. Y.; and the Gas-Light Companies of Newark and Jersey City, in New Jersey. It was mostly made from Cannel and Newcastle caking coals, which were imported from Liverpool, and mixed in the proportions of one-third to five-eighths Cannel, to two-thirds to three-eighths Newcastle. In some of the works a portion of the caking coal was from mines in Pennsylvania. The tar from these different gas-works, as regularly received at the naphtha manufactory, was poured into a large tank provided for this purpose. The stills were uniformly charged with tar directly from this tank; so that there can be no doubt that the naphtha employed was made from a mixture of the tar supplied by the six different companies above enumerated. Most of the gas-works referred to are large, the annual production of tar amounting in the aggregate to upwards of 50,000 barrels. It does not appear, therefore, that the absence of the bodies in question from the naphtha which I have employed, can be attributed to any peculiarity of the tar. The naphtha was prepared in a manufactory in New York over which I had at that time personal control, and was purified under my own direction. The process of purification did not differ essentially from that in common use in England, the reagents employed being oil of vitriol and alkali. One hundred barrels of the purified naphtha were subjected, under my personal superintendence, to repeated fractional distillation from an iron still. The chief object in operating on so large a quantity was to insure the detection of any constituent which might be present in small proportion. The process of fractioning was continued on this large scale until the separations had so far progressed, that at certain temperatures a full barrel of distillate would come off from the ten-barrel still employed, without a variation of more than one or two degrees of the thermometer. Finally, a sample gallon was taken from each of the barrels composing the last series of products, and these samples were set aside for this investigation, which was afterwards conducted in the laboratory.

Of the Results of Fractional Condensation.—Such of the samples above mentioned as promised to yield the different constituents of the naphtha in the largest proportion were subjected to repeated series of fractionings by my process of "Fractional Condensation."[11] As full details of this process have already been given in the memoir referred to, it will be needless to repeat them here. It will suffice to say that the fractioning in this case was conducted in all respects as there described, and continued until the whole of the naphtha taken, boiling between 80° and 170°, had accumulated at the four points already indicated, viz: at 80°, 110°, 140°, and 170°; or so nearly the whole that the intermediate quantities had become too small to admit of being further operated upon. Having, therefore, so thoroughly exhausted the intermediate fractions, I can have no hesitation in asserting that no other body than those alluded to was present in the naphtha,—at least, in appreciable quantity,—hence, that the parabenzole of Church was probably only a mixture of benzole and toluole. I may here remark that each of the sample-gallons employed, when subjected to my process of fractioning, was found to contain, in variable proportion, all of the constituents of the naphtha.

Of some of the Properties of the Bodies obtained by Fractioning.—1. BENZOLE.—Specific gravity, 0·8957 at 0°, and 0·882 at 15°·5.[12]

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[11] Memoirs of the American Academy, 1864, and last volume of this Journal.

[12] It would appear that the specific gravities of liquids are usually determined at the temperature of the air. The result of this is that the determinations made by different observers are not comparable with one another. That these specific gravities are not uniformly taken at 0° C.—the temperature which, on account of

Determination of Boiling-point.—This experiment was conducted in a tubulated retort, operating on 150–200 c. c. of the benzole, containing some pieces of sodium. The benzole employed had previously been repeatedly boiled with sodium, until the latter ceased to have any action. The thermometer bulb extended into the liquid[19] nearly to the bottom of the retort. A second thermometer was attached, by means of flexible bands, to the side of the one in the retort; the bulb being placed, during ebullition, at a point midway between the center of the cork (−5°) and the upper end of the mercurial column, viz: at 85°. A paper screen, closely fitting the thermometer spindle, was placed across at the top of the cork. With the retort neck slightly inclined upward, and cooled to prevent the escape of vapor, ebullition was continued for a considerable time, until the mercury in the thermometer ceased to rise. The lamp being removed for the moment, the neck of the retort was then turned downward, and quickly inserted in a Liebig's condenser. On replacing the lamp, distillation commenced almost immediately at 79°.

Observations.—

Temperature. °		Time. h. m.			Temper. by side thermom.
79·0	at	2.40	5	minutes.	
79·2	"	2.45	15	"	22°.
79·4	"	3.00	12	"	24°.
79·5	"	3.12	20	"	25°.
79·6	"	3.32	18	"	26°.
79·6	"	3.50			26°.

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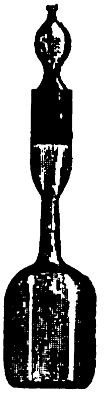
greater convenience, etc., is generally acknowledged to be preferable—is probably due to the fact that the more common specific gravity bottle is not suited to this purpose. Indeed, with a volatile body that bottle cannot serve for an accurate determination at any temperature. A reform in this regard being highly desirable, I would call attention to a specific gravity bottle which I obtained a few years ago from Fastré, in Paris, which is admirably adapted for taking specific gravities, even of volatile liquids, at a low temperature. The accompanying figure represents this bottle one-half its natural size. Who was the author of this particular form I am not informed, although it may have been already noticed in some publication. A bottle analogous to this is figured by Schiel ("Einleitung in das Studium der organischen Chemie," page 76); but his bottle has an oval bottom, which makes it less convenient. The particular advantage of this bottle over the more common one, which advantage Schiel omits to notice, consists in this: that the space or chamber above the line on the capillary neck is large enough to allow for the expansion of the liquid consequent upon the elevation of temperature from 0° to that of the surrounding air; and that the ground stopper fits so closely that no perceptible loss from evaporation can take place during the time occupied by an experiment.



In order to furnish determinations of the specific gravities of the bodies to be treated of in these researches, which shall be comparable with corresponding determinations by other observers, I shall generally record one or more special determinations made for this purpose.

[19] For critical remarks on the question of propriety of placing the thermometer bulb in the liquid, etc.; and for further details of the method of taking boiling-points, especially at low temperatures, see a following memoir, "On the Influence of C_2H_2 on the boiling-points in Homologous Series of Hydrocarbons," etc.

Distillation therefore occupied one hour and ten minutes, during which time the thermometer rose only 0°·6, being fifty minutes in rising 0°·2 from 79°·4 to 79°·6, at which temperature it had distilled nearly to dryness. Height of the barometer during the experiment reduced to 0° =761·9^{mm}. Taking 79°·4, this being the average of the last five observations, and applying the corrections for the upper column of mercury, and for atmospheric pressure, according to the directions given by Kopp,[14] we find the corrected boiling-point of benzole to be 80°·1.

Analysis.—0·2339 gram of benzole gave, by my process[15] of combustion in a stream of oxygen gas, 0·7903 of carbonic acid, and 0·1683 of water.

		Calculated.		Found.
Carbon,	C_{12}	72	92·31	92·15
Hydrogen,	H_6	6	7·69	7·99
		78	100·00	100·14

Determination of Vapor Density.—

Temperature of balance,	15°
Temperature of oil bath,	171°
Height of barometer,	764·1^{mm} at 9°
Increment of balloon,	0·2447
Capacity of balloon,	265 c. c.
Density of vapor found,	2·688
Theory, $C_{12}H_6$=4 volumes,	2·698

2. TOLUOLE.—Specific gravity, 0·8824 at 0°, and 0·872 at 15°.

Determination of Boiling-point.—The preparation employed for this determination had also been repeatedly boiled with sodium until the latter ceased to have any action upon it. Operating in this case also upon a pretty large quantity, the distillation occupied about an hour. The experiment was conducted as detailed under the head of benzole. Distillation commenced at 110°·6; two minutes later, the temperature had fallen to 110°·4, at which point it remained absolutely constant during the lapse of forty-eight minutes. Five minutes later the temperature had risen again to 110°·6; and five minutes later to 110°·8, at which point, having distilled nearly to dryness, the operation was suspended. The corrections for pressure (−0°·16) and for the upper column of mercury—which, with the thermometer used in this experiment, was only 7° in length,—gives 110°·3 as the corrected boiling-point of toluole. Church[16] remarks that toluole, when distilled in the ordinary manner, is liable to become oxydized, and its boiling-point thereby raised, in consequence of the upper part of the retort becoming heated above the boiling-point of toluole. He found that toluole which, by ordinary distillation, had come over between 108° and 109°, would distil eight-tenths between 103° and 104°, after repeated purification with sodium. I would therefore state that my preparation of toluole was never subjected to a temperature above its boiling-point; and that I have never noticed any reduction of the boiling-point of this body by purification with sodium.

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[14] Poggendorff's Annalen, 1847, lxxii, 88.

[15] Proceedings of the American Academy, 1864, p. 251, and this Jour., xxxix, 326.

[16] Philosophical Magazine, 1855, [4], ix, 256.

Analysis.—0·1628 gram of toluole gave, by combustion in a stream of oxygen gas, 0·5447 of carbonic acid, and 0·1315 of water.

		Calculated.		Found.
Carbon,	C_{14}	84	91·3	91·20
Hydrogen,	H_8	8	8·7	8·97
		92	100·0	100·17

Determination of Vapor Density.—

Temperature of balance,	17°
Temperature of oil bath,	209°
Height of barometer,	$760{\cdot}1^{\mathrm{mm}}$ at 15°
Increment of balloon,	0·287
Capacity of balloon,	249·5 c. c.
Density of vapor found,	3·2196
Theory, $C_{14}H_8 = 4$ volumes,	3·1822

3. Xylole (*Cumole of Mansfield and Ritthausen*).—Specific gravity, 0·878 at 0°, and 0·866 at 15°·5.

Determination of Boiling-point.—This determination was made in all respects like that of benzole, the xylole employed having been also subjected to the same treatment. The quantity operated upon was, however, smaller, and the experiment conducted more rapidly. Distillation began at 138°·6, and terminated at 139°, having distilled almost to dryness. The time occupied was seventeen minutes. Taking the average of these observations, viz: 138°·4, and applying the customary corrections, we find 139°·8 to be the corrected boiling-point of xylole.

Analysis.—0·1333 gram of xylole gave, by combustion in a stream of oxygen gas, 0·4413 of carbonic acid, and 0·1185 of water.

		Calculated.		Found.
Carbon,	C_{16}	96	90·57	90·29
Hydrogen,	H_{10}	10	9·43	9·87
		106	100·00	100·16

Determination of Vapor Density.—

Temperature of balance,	16°·5
Temperature of oil bath,	207°·5
Height of barometer,	760^{mm} at 14°
Increment of balloon,	0·3528
Capacity of balloon,	228 c. c.
Density of vapor found,	3·7517
Theory, $C_{16}H_{10}$,	3·6665

These results show clearly that this body has the formula $C_{16}H_{10}$, and that it is doubtless the third member of the benzole series.[17] Although xylole, first discovered by Cahours in the oil separated from wood-spirit, has had a much lower boiling-point assigned to it, I have retained that name for this body, since the results which I have obtained in the study of the light oil from wood-tar indicate that when the corresponding body from this source is in a state of equal purity, its boiling-point will agree with the above determination. I may here mention that in my researches on the light oil from wood-tar I have obtained a body at about 140°, but nothing between that and 110° (these temperatures are not corrected), although special pains were taken to work up the intermediate fractions. So that I am in a position to justify the assertion that no other body was present in appreciable quantity between the temperatures mentioned.

That this body from coal-tar naphtha, boiling at 140°, is not identical with cumole from cuminic acid, will be made apparent on comparison of the results above stated, with those which will be given when treating of cumole.

4. ISOCUMOLE (*Cymole of Mansfield*).—Specific gravity, 0·8643 at 0°, and 0·853 at 15°.

Determination of Boiling-point.—This was conducted with the usual precautions, and under conditions similar to those detailed above. The distillation, as in the foregoing determinations, was continued nearly to dryness, and occupied twenty-five minutes. Before distillation was commenced, the temperature of the boiling liquid was found to be 166°·5, and at the close of distillation 167°. Applying the customary corrections to the average of these observations, viz: 166°·75, we obtain for the corrected boiling-point 169°·8.

Analysis.—0·1944 gram of the substance gave, by combustion in a stream of oxygen, 0.6366 of carbonic acid, and 0·1896 of water.

		Calculated.		Found.
Carbon,	C_{18}	108	90·00	89·31
Hydrogen,	H_{12}	12	10·00	10·84
		120	100·00	100·15

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[17] As this memoir is passing through the press, the receipt of my journals for September calls attention to late publications of Hugo Müller, Béchamp, and Naquet concerning this hydrocarbon. Müller concludes that it is xylole, a result which agrees with my own. (Annalen der Chemie und Pharmacie, 1864, cxxxi, 321.) Béchamp, on the contrary, erroneously regards it as being a *new* hydrocarbon, not belonging to the benzole series. (Bulletin de la Société Chimique, Paris, 1864, 204.) Naquet also calls it a *new* hydrocarbon, and gives it the formula $C_{18}H_{12}$. (Ibid., p. 205.)

Determination of Vapor Density.—

Temperature of balance,	13°·5
Temperature of oil bath,	241°·0
Height of barometer,	769·5^{mm} at 9°
Increment of balloon,	0·4206
Capacity of balloon,	239 c. c.
Density of vapor found,	4·3019
Theory, $C_{18}H_{12}$,	4·151

Hence it appears that the calculated density on the formula $C_{18}H_{12}$ is 0·151 *less* than that found by experiment. The calculated density on the formula $C_{20}H_{14}$, which has previously been assigned to this body,—although, as above stated, without an analysis or determination of vapor density,—is 4·645; which is 0·302 *greater* than that found by experiment. It will be observed that the difference between the density found and that calculated on the formula $C_{20}H_{14}$ is not only more than twice as large as the corresponding difference calculated on the formula $C_{18}H_{12}$, but that the error is reversed; being with $C_{20}H_{14}$ a *deficiency*, while with $C_{18}H_{12}$ it is an *excess*. This circumstance has to my mind a good deal of significance, as it goes strongly to show that the lower formula is the true one. For of the many vapor densities of hydrocarbons which I have determined, I have but rarely met with an instance in which the density found was not greater than the theoretical density. And I have usually observed that the excess of the experimental over the theoretical density is larger in proportion as the boiling-point of the body is higher, a fact which needs explanation. Wurtz[18] observed a similar difference between the determined and calculated vapor densities of bodies of the formulæ C_nH_n and C_nH_{n+2}, which he accounted for on the ground that his preparations contained an admixture of bodies less volatile, the vapors of which would remain in the balloon, and increase the density. But I cannot accept this explanation for the substances here treated of, since they invariably distil without residue within a range of one degree of temperature. I would rather rely upon the supposition that the high temperature employed causes partial decomposition of the substance, which would be the more liable to occur the higher the boiling-point of the body. I do not, however, offer this as an explanation, but merely make the suggestion.

Part II.—*Hydrocarbons from Oil of Cumin and Cuminic Acid.*

The oil of cumin employed in this research was furnished by Messrs. Reed & Cutler, wholesale importers of drugs, etc., of Boston. The package bore the label of Eduard Büttner, manufacturer, of Leipzig, and purported to be a genuine preparation, answering in all of its obvious physical properties—odor, color, etc.—the description given of this oil by Gerhardt and Cahours[19] in their original memoir on this substance, who, it appears, also employed a commercial preparation. Its behavior in distillation left no doubt of its being a genuine article; and this was afterwards confirmed by treatment of the cuminole with fused potash, for the production of cuminic acid, its comportment with this reagent being in all respects identical with that described by Gerhardt and Cahours. Subjected to repeated series of fractionings by my process of fractional condensation already referred to, it gave, in addition to cymole and the residue of cuminole, a body boiling at about 155°, which so closely resembles oil of turpentine in odor, etc., as to be hardly distinguishable from the latter substance. The presence of this body may account for the very low boiling-point which Gerhardt and Cahours assigned to cymole, viz: 165°. The boiling-point of cymole was subsequently found by Gerhardt[20] to be 175°, but my own determination places it still lower by about 5°. It is evident, therefore, from a comparison of their own determinations, that the oil of cumin which they originally operated upon contained an oil boiling below cymole; and hence the finding of such an oil in that which I employed need not raise a doubt as to its being genuine. This lighter body is present in so small a quantity as hardly to admit of its being detected, or at least identified, by the old process of fractioning; and its detection and isolation by the new process is but another illustration of the superior excellence of this method.

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[18] Bulletin de la Société Chimique de Paris, 1863, 309.

1. OF THE BODY RESEMBLING OIL OF TURPENTINE.—Specific gravity, 0·8772 at 0°, and 0·8657 at 15°.

Determination of Boiling-point.—The quantity of material at command was too small to admit of attaining so high a degree of purity for this body as was desirable. The product obtained, however, distilled almost to dryness between 153°·4 and 155°·5. Taking the average of these observations, and applying the usual corrections, we obtain 155°·8 for the boiling-point of this body.

Analysis.—0·2575 gram of the substance gave, by combustion with oxyd of copper, 0·8288 of carbonic acid, and 0·2766 of water.

		Calculated.		Found.
Carbon,	C_{20}	120	88·24	87·73
Hydrogen,	H_{16}	16	11·76	11·94
		136	100·00	99·67

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[19] Annales de Chimie et de Physique, 1841, [3], i, 60.
[20] Ibid., 1845, [3], iv, 111.

Determination of Vapor Density.—

Temperature of balance,	16°
Temperature of oil bath,	211°
Height of barometer,	758°·4^{mm} at 14°
Increment of balloon,	0·4939
Capacity of balloon,	221 c. c.
Density of vapor found,	4·7281
Theory, $C_{20}H_{16}$=4 volumes,	4·7028
Excess found,	·0253

The calculated density on the formula $C_{20}H_{14}$ is 4·635; which, compared with the density found, would increase the excess to 0·093. Although the determination agrees more nearly, indeed almost exactly, with the calculated density on the formula $C_{20}H_{16}$, the calculation on the formula $C_{20}H_{14}$ does not show a greater variation from the density found, than we have observed to be quite frequent with hydrocarbons of so high boiling-point; so that it may be questionable which of these formulæ is the true one. I cannot regard the determination of a vapor density as reliable for fixing the formula nearer than to within two equivalents of hydrogen. In the absence of opposing evidence, it will be wiser, however, to take the formula which agrees best with the results of experiment; at least until it shall be shown that the discrepancy between the calculated and observed vapor densities of bodies of high boiling-point, which appears to be so frequent, is nearly constant, or variable by some fixed law by which the amount of the error, in any given case, may be pretty nearly estimated. I shall therefore regard this body as having the formula $C_{20}H_{16}$, which is also better supported by the results of analysis. On account of its source, and close resemblance to oil of turpentine, I think of no better appellation for this body than cumo-oil of turpentine; thus adding another to the long list of isomers of the former substance, the chemical relations of which stand in so much need of being further studied.

2. CUMOLE.—This body was first obtained by Gerhardt and Cahours,[32] by the dry distillation of a mixture of six parts of crystallized cuminic acid, and twenty-four parts of caustic baryta. Abel[33] obtained the same result by substituting caustic lime for the baryta. His product, however, was found to boil 4° above that of Gerhardt and Cahours. My preparation was also made by the use of lime. Although the results of my experiments confirm the conclusions arrived at by Gerhardt and Cahours as to the composition of this body, yet the numerical results differ considerably from theirs. I have also observed some new facts regarding the formation of this body. They have described the

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[32] Annales de Chimie et de Physique, 1845, [3], iv, 87.

[33] Annalen der Chemie und Pharmacie, 1847, lxiv, 312.

reaction between the baryta and cuminic acid as being much more simple than my experiments seem to indicate. On this point they remark: "The formation of cumene is easily explained. In effect, the cuminic acid being represented by $C_{40}H_{24}O_4$, it appears that C_4O_4, that is to say, 2 equivalents of carbonic acid, are retained by the baryta, while $C_{36}H_{24}$ are set free."[34]

$$C_{20}H_{12}O_2 = C_2O_2 + C_{18}H_{12}.$$

In another place (p. 88) they remark, that "by suitably managing the heat, and employing no more than 6 gr. of cuminic acid at a time, no other products are ever obtained than those which we mention."[35] My experiments show that this reaction is by no means so simple as thus described. The crude product obtained from the mixture of lime and cuminic acid, when subjected to a simqle distillation from a tubulated retort, was found to distil between 155° and 250°, leaving a residue at the latter temperature which became semi-fluid on cooling. The distillate thus obtained gave, by my process of fractional condensation, an oil boiling at 151°·1, and a residue at 170°. It is not improbable that the latter may prove to be mostly cymole, $C_{20}H_{14}$; but the quantity was too small to admit of pursuing this inquiry with the probability of deciding the question. There is evidence, however, that the product obtained by Gerhardt and Cahours was not simply pure cumole, as they described it, but a mixture of different bodies, which would necessitate a more complicated reaction than that which they assigned. Gerhardt and Cahours found the boiling-point of their cumole to be constant at 144°. Four years later, Gerhardt,[36] having occasion to make a very accurate determination of the boiling-point of this body, in connection with his research to find a law governing the boiling-points of the hydrocarbons, found its boiling-point to be 9° higher, viz. 153°, which is but 2° higher than my own determination. The disagreement between their determinations, it being so considerable, may be more reasonably accounted for on the supposition that they operated, in the first instance, upon a mixture of different bodies; and yet I cannot see how they could have obtained the product boiling below 150°. Additional evidence on this point will be found in the discrepancy which appears between their determination of the vapor density, and that calculated upon theory.

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[34] "La formation du cumène s'explique aisément. En effet, l'acide cuminique etant représenté par $C_{40}H_{24}O_4$ on voit que C_4O_4, c'est-a dire 2 equivalents d'acide carbonique sont retenus par la baryte, tandis que $C_{36}H_{24}$ sont dégagés." —Annales de Chimie et de Physique, 1841, [3], i, 89.

[35] "En dirigeant la chaleur convenablement et en n'employant, pas plus de 6gr. d'acide cuminique à la fois, on n'obtient jamais d'autres produits que ceux que nous venons de nommer."

[36] Annales de Chimie et de Physique, 1845, [3], xiv, 107.

The specific gravity of my preparation of cumole was found to be 0·8792 at 0°, and 0·8675 at 15°.

Determination of Boiling-point.—The quantity of material being quite small, this determination was made in a large test tube, with the usual precautions. It had not a perfectly constant boiling-point, the distillation ranging from 148°·4 to 151°·6. Applying the proper corrections to the mean of these observations, gives, for the boiling-point of cumole, 151°·1, which is doubtless a little too high from the impracticability of making a complete separation with the small quantity of material employed. If the boiling-point difference between cumole and cymole, for the difference of C_2H_2 in their elementary formulæ, is 30°, as there is every reason to believe, then the boiling-point of cumole should be 150°, as I have found the boiling-point of cymole to be but a fraction under 180°.

Analysis.—0·1700 gram of cumole gave, by combustion with oxyd of copper, 0·563 of carbonic acid, and 0·1557 of water.

		Calculated.		Found.
Carbon,	C_{18}	108	90·00	90·35
Hydrogen,	H_{12}	12	10·00	10·18
		120	100·00	100·53

Determination of Vapor Density.—

Temperature of balance,	17°
Temperature of oil bath,	203°
Height of barometer,	760·1mm at 15°
Increment of balloon,	0·4428
Capacity of balloon,	232 c. c.
Density of vapor found,	4·2003
Theory, $C_{18}H_{12}$=4 volumes,	4·151

This determination, as well as the results of analysis, confirms, therefore, the formula which Gerhardt and Cahours had assigned to this body. I had anticipated a different result from this, inasmuch as the hydrocarbon from coal-tar naphtha, which I have called *iso-cumole*, boiling at 170°, or nearly 20° higher than cumole from cuminic acid,—had been found, as I have shown above, to have the formula $C_{18}H_{12}$. I am forced to the conclusion, therefore, that these two bodies are isomeric, and belong to different series. A preliminary examination of their behavior with reagents indicates that their chemical properties are also different. These will be treated of on a future occasion, in Part III.

3. CYMOLE.—Notwithstanding that this body is so much more volatile than the cuminole with which it is associated in the oil of cumin,—there being a difference of 40° between their boiling-points,—Gerhardt and Cahours found it necessary to resort to

chemical means, viz: treatment with fused potash, in order to isolate it. Being desirous of testing the efficiency of my process in effecting the separation, the preparation employed in this investigation was obtained by fractional condensation, this process having been found as effective in this as in other cases. This will appear by a comparison of the results obtained in the study of this body before and after treatment with concentrated sulphuric acid, which is also effective to remove cuminole.

Specific gravity.—

At 0°, before treatment with $HO\,SO_3$,				0·8697
At 0°, after	"	"	"	0·8724
At 14°, before	"	"	"	0·8592

Determination of Boiling-point before Treatment with Sulphuric Acid.—The preparation was found to distil to dryness between 175°·8 and 177°. The temperature remained absolutely constant at 176° during the lapse of ten minutes, and occupied fifteen minutes in rising from 176° to 176°·5. Taking the mean of the former numbers, viz: 176°·4, and applying the proper corrections for pressure, etc., we obtain 179°·5 for the boiling-point of cymole.

After Treatment with Sulphuric Acid.—The preparation distilled to dryness between 176° and 177°, the temperature remaining thirteen minutes constant at 176°·3, indicating that no essential change in the boiling-point had been produced by the acid treatment. It was nevertheless evident that some impurity was being removed by the acid, as the first portions of the latter became dark-colored and thickened on being agitated with the oil. Successive portions of acid were therefore employed, until it ceased to produce any marked effect.

Analysis before Treatment with Sulphuric Acid.—0·1589 gram of cymole gave, by combustion in a stream of oxygen gas, 0·5200 of carbonic acid, and 0·1532 of water.

		Calculated.		Found.
Carbon,	C_{20}	120	89·55	89·25
Hydrogen,	H_{14}	14	10·45	10·71
		134	100·00	99·96

After Treatment with Sulphuric Acid, and Distillation in Vacuo.—0·1623 gram of cymole, by combustion in a stream of oxygen gas gave 0·5324 of carbonic acid, and 0·1561 of water.

		Calculated.		Found.
Carbon,	C_{20}	120	89·55	89·46
Hydrogen,	H_{14}	14	10·45	10·68
		134	100·00	100·14

The removal of impurity by treatment with sulphuric acid had therefore hardly a sensible effect on the results of analysis.

Determination of Vapor Density before Treatment with Sulphuric Acid.—

Temperature of balauce,	11°
Temperature of oil bath,	259°
Height of barometer,	740·6$^{\text{mm}}$ at 5°
Increment of balloon,	0·4446
Capacity of balloon,	239 c. c.
Density of vapor found,	4·742
Theory, $C_{20}H_{14}=4$ volumes,	4·6351

After Treatment with Sulphuric Acid.—

Temperature of balance,	25°·5
Temperature of oil bath,	255°
Height of barometter,	760$^{\text{mm}}$ at 26°
Increment of balloon,	0·4647
Capacity of balloon,	232 c. c.
Density of vapor found,	4·7536
Ditto before treatment with $HOSO_3$,	4·742
Difference,	·0116

The results of the two determinations are therefore almost identical.

A comparison of the above results with those obtained in the study of isocumole, the body from coal-tar naphtha boiling at 170°, will show that the two bodies are far from being the same substance, as Mansfield assumed, and that they have a constitutional difference of C_2H_2, and therefore doubtless belong to different series.

NOTE.—I had hoped to be able to present on this occasion the results of the study of some of the more important reactions of the hydrocarbons treated of in the preceding pages,—at least, of those in regard to which I have differed from my predecessors; but as this work is yet incomplete, and as I am at present occupied with the study of other substances of more immediate interest, I will defer this branch of the subject for future consideration, in Part III. I may here remark, however, that the behavior of these bodies with reagents is such as to strengthen the conclusions already expressed in regard to them.

[To be concluded.]

ART. XIII.—*On the nature of the Invisible Photographic Image;* by M. CAREY LEA, Philadelphia. In a letter to the Editors.

SOME experiments in which I have lately been engaged seem to me to finally settle the long-contested question as to the nature of the invisible photographic image, and I hasten to send a very brief description of them, hoping it will be in time for your July number.

The view that the change which takes place in an iodo-bromized plate in the camera, is a purely physical one, that no chemical decomposition takes place, and neither liberation of iodine nor reduction of silver, has obtained a pretty general acceptance. But latterly it has been opposed by two distinguished photographers, Dr. Vogel and Major Russel. The former affirms that iodid of silver is never sensitive unless there is a body present capable of taking iodine from it under the influence of light. And Russel believes that the developed image is chiefly produced at the expense of the silver haloid in the film.

The following experiments seem to me to decisively close this controversy in *favor of the physical theory.*

Experiment 1.—If the iodid or bromid of silver in the film undergoes decomposition in the camera, and still more, if the developed image is formed at its expense, the film of iodo-bromid must necessarily be greatly consumed in the development under the dense portions of the negative, which it has contributed to form.

To settle this point, I exposed and developed an iodo-bromized plate in the ordinary manner. Then, instead of removing the unchanged iodid and bromid by fixing in the ordinary manner, I took measures to *remove the developed image without affecting the iodid and bromid.* This I succeeded in doing with the aid of a very weak solution of acid per-nitrate of mercury. Now, if the iodid, or bromid, or both, had been in any way decomposed, to form, or aid in forming, the developed negative image, when this came to be removed, there should have been left a *more or less distinct positive image,* depending upon varying thicknesses of iodid and bromid in the film, much like a fixed negative that has been completely iodized. *Nothing of this sort* was visible, the film was perfectly uniform, just as dense where an intense sky had been, as in those parts which had scarcely received any actinic impression, and looking exactly as it did when it first left the camera, and before any developer had been applied.

This experiment seems sufficiently decisive. But the following is far stronger.

Experiment 2.—A plate was treated in all respects as in No. 1, except that the application of the nitrate of mercury for removing the developed image was made by yellow light. The plate, now showing nothing but a uniform yellow film, was carefully washed, and an iron developer, to which nitrate of silver and citric acid had been added, was applied. *In this way the original image was reproduced*, and came out quite clearly with all its details.

Now as every trace of a picture and all reduced silver had been removed by the nitrate of mercury, it is by this experiment absolutely demonstrated that the image is a purely physical one, and that after having served to produce one picture, that picture may be dissolved off, and the same physical impression may be made to produce a second picture by a simple application of a developing agent.

Philadelphia, June 14, 1865.

P. S.—Since the above was written, I have repeated the experiment with a pyrogallic development with similar results. Both the first and second developments may be made with an iron developer, or both with a pyrogallic. The experiment succeeds without the least difficulty in either way.

ART. XIV.—*Mineralogical Notices;* by Prof. C. U. SHEPARD.

1. *Syhedrite.*—I have thus named, from its locality, a very pretty green mineral sent me in small quantity by Dr. Thomas Oldham, Geological Surveyor General of India. It has the following properties: Hardness =3·5. Gravity =2·321. Massive; irregularly foliated in much-contorted individuals, resembling common varieties of massive highly crystalline dolomite. Color leek-green,—that of the purest Indian heliotrope. Translucent on the edges only. Luster vitreous. Cleavage in one direction very distinct. Brittle. Liable to alteration by exposure to the weather, when it loses its luster and cleavage, and assumes a pale greenish color, at the same time emitting an earthy odor if moistened.

Before the blowpipe it swells up slightly and undergoes easy fusion into a slightly grayish pearly-white enamel. With borax it gives imperfectly the reaction of iron. Its powder is apple-green, in which state it is but slowly attacked by long boiling in chlorhydric acid. The liquid does not gelatinize on cooling.

As I wished to preserve as much as possible of the material for an analysis by Mr. Tyler, I had but six grains to submit to a qualitative examination. Nevertheless, I kept as accurate an account as I was able of the results obtained. They were, Silica 55·00, alumina 12·00, lime 6·00, protoxyd of iron 4·50, magnesia 2·20, water 13·33=94·15.

The mineral occurs in trap at Thore-Ghat, in the Syhedree Mountains, Bombay.

Mr. W. S. Tyler has sent me the following account of his chemical examination, in a letter dated Göttingen, March 1, 1865.

"I have finished my examination of the *Syhedrite*, with the exhaustion of the material, and send you herewith the results at which I have arrived. Though not so satisfactory as could be desired, some conclusions as to the constitution of the mineral may nevertheless be drawn from them. I devoted most of the material, of course, to a search for the alkalies, but found none present. Of the rest, the larger part was used for the determination of the water and silica, and of the bases as well as possible. The determination of the silica failed, through an accident; and the precipitate of alumina and oxyd of iron was melted with nitrate of potassa to see if a trace of chrome were present. None, however, was found. So only the water, lime and magnesia were determined in this portion. The small remainder was again divided. One portion was heated with concentrated sulphuric acid in an atmosphere of carbonic acid, to discover, if possible, whether the iron was present as protoxyd or peroxyd; but the mineral appeared to be entirely insoluble. The other portion, of 0·095 grams, was devoted to a second determination of the water.

No. 1 (0·8239 grs.) was decomposed with hydrofluoric acid, and gave 0·1241 gr. Al^2O^3, 0·025 gr. Fe^2O^2, 0·0595 gr. CaO.

No. 2 (0·3695 gr.), decomposed in the usual way, with carbonate of soda and potassa, after ignition to determine the water, gave 0·0209 gr. CaO, 0·009 gr. MgO.

No. 3 (0·095 gr.) gave 0·0156 gr. HO.

Analyses.

	1.	2.	3.	Average.	Oxygen.		Ratio.
$\overset{\dots}{\bar{A}}l$,	15·06	——	——	15·06	7·03	7·03	2
$\dot{F}e$,	2·71	——	——	2·71	·60		
$\dot{C}a$,	7·23	5·67	——	6·45	1·84	3·42	1
$\dot{M}g$,	——	2·46	——	2·46	·98		
$\dot{H}$,	——	16·38	16·42	16·40	14·60	14·60	4
$\dddot{S}i$,	(by the loss)		=	56·92	30·36	30·36	9
				100·00			

The simplest formula which corresponds to a ratio 1 : 2 : 9 : 4 would be $(3\dot{R}\dddot{S}i+2\overset{\dots}{\bar{A}}l\dddot{S}i^3)+12$ aq., in which $\overset{\dots}{\bar{R}}$ represents $\overset{\dots}{\bar{A}}l$; and $\dot{R}$, $\dot{F}e$, $\dot{C}$ and $\dot{M}g$, in the proportion $2\dot{F}e$, $6\dot{C}a$ and $3\dot{M}g$. This formula requires SiO^3 58·05, HO 15·47, Al^2O^3 14·73, FeO 2·81, CaO 6·71, and MgO 2·34. If the iron be calculated as peroxyd, we have the following composition:

		Oxygen.		Ratio.
$\overset{\cdots}{\mathrm{Al}}$,	15·06	7·03	7·93	3
$\overset{\cdots}{\mathrm{Fe}}$,	3·03	·90		
$\overset{\cdot}{\mathrm{Ca}}$,	6·45	1·84	2·82	1
$\overset{\cdot}{\mathrm{Mg}}$,	2·46	·98		
$\overset{\cdot}{\mathrm{H}}$,	16·40	14·60	14·60	5
$\overset{\cdots}{\mathrm{Si}}$,	56·60	30·15	30·15	11

$\overset{\cdot}{\mathrm{R}}:\overset{\cdots}{\mathrm{R}}:\overset{\cdots}{\mathrm{Si}}:\overset{\cdot}{\mathrm{H}}=1:3:11:5$

So regarded it has very nearly the composition of stilbite, the oxygen ratio in which is 1 : 3 : 12 : 5, or $(\overset{\cdot}{\mathrm{R}}\overset{\cdots}{\mathrm{Si}}{}^3+\overset{\cdots}{\mathrm{Al}}\overset{\cdots}{\mathrm{Si}}{}^3)+5$ aq., with $\overset{\cdots}{\mathrm{Si}}$ 59·79, $\overset{\cdots}{\mathrm{Al}}$ 16·61, $\overset{\cdot}{\mathrm{Ca}}$ 9·06 and $\overset{\cdot}{\mathrm{H}}$ 14·54. The mineral differs, however, from stilbite in its complete insolubility in acids; and its green color would seem, in the absence of chrome, to indicate that the iron was contained as protoxyd. Want of material prevented any further attempts to solve this not altogether easy question." * * * *

2. *Octahedral Garnet at Middletown, Conn.*—Ever since the china-stone quarry of feldspar has been wrought at Middletown, I have known of the occasional finding there of precious garnet in small drusy masses, either loose in cavities of feldspar or slightly adhering to the walls of such cavities. The color is intermediate between blood-red and hyacinth-red. The largest masses were rarely above half an inch in diameter. Their most ambiguous feature was, that they presented no facets of the rhombic dodecahedron or the trapezohedron, both of which forms were often to be met with in the common red-brown garnet of the same locality. Prof. Johnston of Middletown, however, has lately sent me a small fragment, among some fragments of columbite, which shows distinct octahedral faces, thus evincing the relationship of the substance with the octahedral garnet of Elba.

3. *Corundophilite at Chester, Mass.*[1]—I find this mineral to be most abundant along with the emery of this locality, especially with the coarse granular variety (where the corundum is in rhombohedral crystals of a reddish-brown color); also with the margarite. *Ilmenite*, in thin curved laminæ, is likewise associated with the margarite.

4. *American Sienna.*—A valuable repository of this precious pigment exists in the town of Whately, which will soon be reported upon, and is destined to be brought into extensive use.

5. *Diaspore in the Emery-rock of Chester, Mass.*—I found this mineral on the surfaces of cross-joints of the emery-rock about a month since. It was associated with rose-colored amphodelite. On visiting the locality yesterday, I again discovered it similarly situated, but associated with radiated epidote; and in

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[1] **This is the mineral which Dr. Jackson, in his account of the emery locality, calls *chloritoid*.**

a second instance, in very distinct crystals occupying little clefts, the crystals being implanted on massive or simply foliated diaspore of a delicate pink color, similar to the Hungarian variety. The crystals had the form usually figured for Gœthite, and were of a blood-red color, owing to a very thin film of peroxyd of iron. When broken across, however, they showed the same tint as the massive mineral beneath. The hardness, cleavage, and blowpipe characters fully agreed with the inferences concerning the nature of the mineral derived from crystalline form.

The observation is the more interesting as adding another species to the associates of emery detected at this locality. I have elsewhere had occasion to point out two Connecticut localities of diaspore, one at Litchfield with corundum and kyanite, the other at Trumbull with topaz and fluor.

6. *Dipyre at Canaan, Conn.*—Mr. F. E. Seymour of New York gave me for examination an unknown mineral which I find to be dipyre. It occurs in small crystals disseminated through a white saccharoidal limestone, associated with minute crystals of pyrites and occasional scales of a nearly colorless mica. At first view the mineral seemed to be a glassy pyroxene; but on further examination it was found to disagree with that species both in form and in the absence of cleavage. The crystals are octagonal prisms, or rather right-square prisms, with their lateral edges deeply truncated, so as to possess very nearly equal breadth with the primary planes, to which they incline under angles of 135° (reflec. goniom.). Hardness =6. Gravity =2·6. Semi-transparent. Color gray with a tinge of blue. Fracture conchoidal to uneven. No traces of cleavage. Luster vitreous. Largest crystals one-third of an inch in diameter. Length about three times their diameter. I have seen but one crystal (out of fifty) that showed a tendency toward a regular termination. This exhibited one of the pyramidal planes common in meionite and scapolite. Before the blowpipe it immediately whitens, becomes nearly opake, and fuses easily with ebullition into a vesicular glass, the flame of the blowpipe being deeply colored yellow.

Amherst College, Feb. 15, 1865.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the chemical constitution of the brain.*—Liebreich has discovered in the fresh brain of man and animals a crystalline substance to which he has given the name of *protagon* (from *πρωταγον*). The brain of an animal is most conveniently obtained free from blood by cutting the carotid arteries and injecting water until the liquid which flows from

the veins is colorless. After removal from the skull and adhering membranes, the brain is to be rubbed in a mortar to a fine paste and the mass shaken in a flask with water and ether. Cholesterin and substances soluble in water are thus removed; after filtering, the mass on the filter is treated with alcohol of 85 per cent at 45° C. in a water-bath and then filtered through a water-bath filter. The filtrate is to be cooled to 0° C., when an abundant flocky precipitate settles, which is to be collected upon a filter and washed with cold ether until the filtrate is free from cholesterin. The mass is then to be dried under the air-pump over sulphuric acid, moistened with a little water and dissolved in alcohol at 45° C. The solution, after filtration, is to be allowed to cool gradually upon a water-bath to the mean temperature of the atmosphere, when the liquid will be found filled with microscopic crystals. These differ somewhat in appearance according to the quantity of alcohol employed; they may be purified by repeated crystallization. The pure protagon as obtained from the brains of various animals was found to have the formula

$$C_{232}H_{241}N_4PO_{44}.$$

Dried under the air-pump over sulphuric acid, protagon is a light flocky powder soluble in hot alcohol and ether, but with difficulty soluble in these media when cold. Absolute alcohol at a temperature higher than 55° C. decomposes protagon partially. With water, protagon swells up remarkably and gives an opaque, starch-like mass. More water gives a clear but opalescent solution; when boiled with saline solutions the protagon coagulates, but the coagulum is not a chemical compound, as the salts may be washed out. Glacial acetic acid dissolves protagon to a clear liquid, from which, on cooling, crystals may be obtained similar to those yielded by alcohol. When strongly heated, protagon melts, becoming at the same time brown, and finally leaves a carbonaceous mass which is difficult to burn and which has an acid reaction when moistened.

When protagon is boiled for 24 hours with baryta water, glycerin-phosphoric acid is formed, which unites with the baryta, while a new base which the author terms neurin is set free. Two fatty acids are formed at the same time, of which one appears to be stearic acid. The lead salt of the other acid is soluble in ether, but the acid is not the oleic. Neurin has the formula $C_{10}H_{13}N$ as the simplest expression of the analysis; its platinum salt is $C_{10}H_{14}N.Cl, PtCl_2$. The above investigations are sufficient to show that protagon possesses an extremely complex structure, while the products of its decomposition separate it in a remarkable manner from other known substances. Liebreich considers it certain that the glycerin-phosphoric acid, oleo-phosphoric acid, cerebrin, &c., of certain writers, are all secondary products of the decomposition of protagon. It is to be hoped that the author will pursue the investigation, which promises results of extraordinary importance and interest. —*Ann. der Chemie und Pharm.*, cxxxiv, 29. W. G.

2. *On an advantageous method of preparing oxygen.*—FLEITMANN has given a method of preparing oxygen from bleaching powder, which possesses much interest theoretically, and which appears also to offer some decided advantages over the ordinary processes. The method in question depends upon the fact that a concentrated solution of hypochlorite of lime, when warmed with a trace of freshly prepared moist hyperoxyd of

cobalt, is completely decomposed into oxygen and a solution of chlorid of calcium. No chlorate of lime is formed, but the whole of the active oxygen is given off. The evolution of oxygen takes place at a temperature of 70° to 80° C. in a regular current, with a gentle foaming of the liquid. The author's explanation of the process is that a lower hyperoxyd constantly takes oxygen from the hypochlorite of lime and passes into a higher oxyd, which is decomposed into oxygen and the lower oxyd, and the process is then repeated. The same quantity of hyperoxyd will serve to decompose an indefinite amount of the hypochlorite; the quantity required is very small, from $\frac{1}{2}$ to $\frac{1}{10}$ of one per cent being sufficient. Instead of preparing the hyperoxyd of cobalt separately, a few drops of any soluble cobalt salt may be added to the solution of the hypochlorite, when a corresponding quantity of the hyperoxyd is formed. The advantages of the new mode of preparing oxygen, according to Fleitmann, are as follows. 1. The evolution of the gas is very regular and easily managed, so that the process may be used for lecture experiments in which a gas bladder cannot be employed. After the heat is once applied the lamp may usually be removed, the decomposition going on to the end. 2. All the oxygen of the material is obtained, which is not the case when peroxyd of manganese is heated. 3. The method is much cheaper than that by means of chlorate of potash.

It is to be regretted that the solution of hypochlorite of lime cannot be used in the raw unfiltered state, but must be perfectly clear, since a milky solution foams so that finally the whole contents of the vessel pass over. A clear solution is best obtained by decantation, one portion of of the hypochlorite being heated with water, and then this solution being employed to act upon a fresh portion of the bleaching powder. In this manner it is easy, from a good bleaching powder of 35 per cent, to obtain a solution which evolves 25 to 30 times its volume of oxygen. Upon the small scale, flasks may be employed with advantage, and these may be filled with the liquid to $\frac{7}{8}$ of their volume. Upon the large scale for technical purposes steam boilers would answer well, and would permit the evolution of gas under pressure.—*Ann. der Chemie und Pharm.*, cxxxiv, 64. W. G.

3. *On propyl-phycit.*—CARIUS has given the name of propyl-phycit to a new alcohol, which affords the first instance of a tetratomic organic oxyd in combination with water, and the formula of which, in the ordinary equivalent notation, is $C_8H_4O_4$, 4HO or $\left.\begin{matrix}C_8H_4\\H_4\end{matrix}\right\}O_8$. The new alcohol is an amorphous, solid, tough and colorless substance. It does not crystallize, is soluble in water and alcohol, and has a very sweet taste. In its chemical relations, propyl-phycit resembles the group of sugars in a very remarkable manner. The solution, acidulated with a little chlorhydric, sulphuric, or even acetic acid, and evaporated in a water-bath becomes brown, while a body resembling humus separates, and the greater part of the alcohol is converted into a carbonaceous mass. Humus-like substances are also formed by boiling the alcohol with alkalies. Propyl-phycit prevents the precipitation of oxyd of copper by caustic potash and reduces silver from the ammonia nitrate. Dilute nitric acid oxydizes the alcohol and forms a new acid, $\left.\begin{matrix}C_8H_2O_2\\H_4\end{matrix}\right\}O_8$; by a further oxydation, oxalic acid

is formed. All the four equivalents of hydrogen in the molecule of propyl-phycit may be replaced by other radicals, simple or compound; thus the lead salt has the formula $\left.\begin{matrix}C_6H_4\\H_2Pb_2\end{matrix}\right\}O_8$. The author describes various ethers derived from the new alcohol, the formulæ of which are as follows:

Mono-nitrate,	$\left.\begin{matrix}C_6H_4\\H_3NO_4\end{matrix}\right\}O_8.$
Di-acetate,	$\left.\begin{matrix}C_6H_4\\H_2(C_4H_3O_2)_2\end{matrix}\right\}O_8.$
Tetracetate,	$\left.\begin{matrix}C_6H_4\\(C_4H_3O_2)_4\end{matrix}\right\}O_8.$
Triethyl ether,	$\left.\begin{matrix}C_6H_4\\H(C_4H_5)_3\end{matrix}\right\}O_8.$
Tetrethyl-ether,	$\left.\begin{matrix}C_6H_4\\(C_4H_5)_4\end{matrix}\right\}O_8.$
Diaceto-diethyl ether,	$\left.\begin{matrix}C_6H_4\\(C_4H_5)_2\\(C_4H_3O_2)_2\end{matrix}\right\}O_8.$

These compounds are of special interest as illustrating the tetratomic character of the new radical C_6H_4, and as establishing the existence of 4-acid alcohols. They render it, to say the least, extremely probable that Mannit, and also the ordinary species of sugar, may be prepared artificially, and they confirm the view long since proposed by Berthelot that the sugars are hydrated oxyds analogous to ordinary alcohol but of a higher order. A tetracid alcohol will form four chlorids by the successive replacement of HO_2 by Cl. Thus we should have $\left.\begin{matrix}C_6H_4\\H_4\end{matrix}\right\}O_8$, $\begin{matrix}C_6H_4\\H_3\end{matrix}\left\{\begin{matrix}O_6\\Cl\end{matrix}\right.$, $\begin{matrix}C_6H_4\\H_2\end{matrix}\left\{\begin{matrix}O_4\\Cl_2\end{matrix}\right.$, $\begin{matrix}C_6H_4\\H\end{matrix}\left\{\begin{matrix}O_2\\Cl_3\end{matrix}\right.$, $C_6H_4 . Cl_4$. Of these two chlorids, the second in order, namely, $\begin{matrix}C_6H_4\\H_2\end{matrix}\left\{\begin{matrix}O_4\\Cl_2\end{matrix}\right.$, was obtained by the author from glycerine and formed the starting point of the present investigation.—*Ann. der Chemie und Pharm.*, cxxxiv, p. 71. W. G.

II. MINERALOGY AND GEOLOGY.

1. *Descriptions of New Species of Fossils from the Paleozoic Rocks of the Western States;* by J. H. McChesney. Chicago. Date of text between 1859 and 1861; illustrations, 1865.[1]—The letter-press of this memoir, issued in separate parts some time back, was briefly noticed in this Journal, [2], vol. xxxii, p. 122, 1861. Since that time the author has had most of the fossils described by him carefully lithographed, and now re-issues the whole, consisting of ninety-seven large octavo pages of text, with intercalated wood-cuts, and nine well executed plates containing about one hundred and sixty figures. The fact that these figures were

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[1] The exact date of the *publication* of this memoir is unknown to us, since it bears only the date of the *preparation* of the first part, (Oct. 1859), which was not actually published for some time after; while we did not receive the last part, nor can we learn that it reached others, until about the middle of February, 1861.

drawn on stone by Mr. Salter of London, the well known English paleontologist, is alone a sufficient guaranty for their accuracy; but we can add, from a personal knowledge of many of these forms, our own testimony that they are faithfully represented.

By reference to our former notice of this memoir, alluded to above, as originally published without figures, it will be seen we were then impressed with the remarkable coincidence of the characters of several forms described, with those of old and well known species. But as no illustrations accompanied these descriptions as then issued, we did not consider our means of comparison sufficient to justify an expression of opinion in regard to the validity of the proposed new species. As the author has now, however, added good figures of many of these fossils, we have made careful comparisons of the species with those figured and described by others, and here offer some critical notes embodying our conclusions.

To begin with the first plate, we would remark that figure 1, *Retzia subglobosa* McC. was described by Dr. Shumard in 1858, under the name *Retzia punctulifera*, in the Trans. St. Louis Acad. Sci., vol. i, p. 220. It is also identical with a form described by Marcou during the year 1858, in his N. Am. Geol., p. 51, as *Terebratula Mormonii.* Figure 2, *Terebratula geniculosa* McC. represents a well known species described many years back in this Journal, by Dr. Morton, from Ohio, as *T. bovidens*, and more recently by Prof. Hall under the name *T. millepunctata*, in vol. iii, Pacific Rail Road Reports, p. 101, 1857. Figure 3 of this plate is not mentioned in the explanations of the figures, and as no references were printed with the text to any of the illustrations, some doubts may exist in regard to the species it is intended to represent; but from the fact that it agrees well with the description of a shell he proposes to call *Ambocœlia gemmula* on page 41, and with no other form described in the memoir, we suppose it must have been drawn from that shell. In regard to this form, however, we have only to say, that reliable authorities both in this country and England, including Mr. Davidson of London, after a careful examination of authentic specimens from the Illinois localities, consider it the common *Spirifer Urei* Fleming. If future comparisons, however, should show it to be distinct, Prof. McChesney's name could not stand, since Dr. Shumard had proposed for it the name *Spirifer planoconvexus*, in 1858; see Missouri Geol. Report, page 202. Figure 4, *Spirifer subventricosus* (*subventricosa* in the memoir), seems to be a variety of the same shell described by Prof. Hall in 1858, under the name *S. opimus* (Iowa Report, p. 711), judging from specimens of *S. opimus* from the Iowa localities. Figure 5, *Orthis Richmondi* McC., was described by Meek & Hayden, from the Coal-measures of Kansas, in the Proceed. Acad. Nat. Sci. Philad., 1858, under the name *Orthisina crassa*, and subsequently referred by them to the genus *Streptorhynchus*, to which it properly belongs. Fig. 6, *Orthis Lasallensis* McC. is almost certainly only a variety of the last. Upon such trivial differences, species may be multiplied indefinitely in a genus like this. Fig 7, *Productus asper* McC., fig. 8, *P. Wilberanus* McC., and fig. 9, *P. symmetricus* McC., are all, we strongly suspect, only varieties of one species, apparently very closely allied to the European *P. scabriculus;* if distinct from that shell, however, Norwood and Pratten's name, *P. Rogersii*, (Jour. Phil. Acad. Sci., Aug. 1854) founded upon a variety, or more properly, *condition* of the same, will have to be adopted. Figures 11 and 12, *Productus tubulospinus* McC., seems scarcely distinct from the European species, *P. punctatus;* but if different, our author's name cannot be retained for it, since it was figured by Prof. Shephard in this Journal, vol. xxxiv, p. 153, under the name *P. semipunctatus*, as long back as 1838.

In regard to Plate II, we would merely remark that figure 5, *Bellerophon Blaneyanus* McC. seems to be only a variety of his *B. vittatus.* He is doubtless correct, however, in separating these forms from *B. Urei* Fleming, though here, again, he was too late in proposing a new name, since Mr. Cox named this shell *B. carbonarius* in 1857 (see Kentucky Geol. Report, vol. iii, p. 562). His figures 9, 10 and 13, of this plate, representing forms he calls *Leda Knoxensis*, *L. Owenii*, and *L. Rushensis*, have more the form of the genus *Yoldia.* If they are true Ledas, however, their names must be written *Nuculana Knoxensis*, *N. Rushensis* and *N. Owenii*, since the name *Leda* is only a synonym of the older genus *Nuculana* Link, 1807. Figure 12, *Nucula Mercerensis* McC., has not the form of shells of that genus as restricted by modern conchologists. Figures 14 and 15, *Natica Altonensis* McC. and *N. Shumardi* McC. are not Naticas at all, as will be seen at a glance, but belong to McCoy's genus *Naticopsis*, and hence, if new, must be called *Naticopsis Altonensis* and *Naticopsis Shumardi.* The genus *Natica*, as now restricted, is wholly unknown in the Paleozoic rocks, as all well informed paleontologists must be aware. Figures 19 and 20, *Discina trigonalis* McC., and *D. capuliformis* McC. are doubtless only varieties of *D. nitida* Phillips, which, according to figures given by Mr. Davidson, who referred specimens sent from the Illinois localities to that species, includes a wider range of varieties than the two forms figured by our author.

We pass over Plates III and IV, occupied by figures of shells and crinoids, which, so far as we know, represent good species.

On Plate V, among other Crinoids, he gives (fig. 4) an excellent representation of a noble species under the name *Forbesiocrinus Pratteni*, which shows distinctly *four* basal pieces, supposed by our author to be "probably an accidental feature." It is unnecessary, however, to inform any one who has given the Crinoids more than a superficial study, that this is a typical *Melocrinus*, and, hence, must be called *Melocrinus Pratteni.*

Figure 1 of Plate VI represents his *Productus inflatus*, which may be a small variety of *P. semireticulatus*, supposing that shell to vary to the extent represented by Mr. Davidson. Figures 8 and 9 of this plate illustrate his *Allorisma sinuata* and *A. clavata.* In the explanations of the plate he writes their names *Allorisma* (*Sanguinaria*) *sinuata*, and *Allorisma* (*Sanguinaria*) *clavata.* As *Sanguinaria* is a genus of plants, the Professor doubtless intended to write it *Sanguinolites*, since the first of these two species seems to be congeneric with some of the forms included in the latter genus by McCoy.

On Plate VII, he gives (fig. 4) *Nuculites Vaseyanus*, which is undoubtedly a true *Leda*, or more properly *Nuculana*, and should be called *Nuculana Vaseyana.* The ridge he mentions, descending from the beaks, on the inner side of the valves, is a common feature in such Carboniferous species as *Nuculana bellistriata* (=*Leda bellistriata* of Stevens). His *Nucula rectangula*, of the same plate, is more probably a *Nuculites.*

On Plate IX, fig. 1 represents his *Pentamerus bisinuatus*, which is apparently only a variety of the well known *P. oblongus*. On the same plate, he figures a shell at first described by him under the name *Ambonychia neglecta*, but placed by him now in the genus *Pterinea*. It has not the strong anterior muscular scar, nor apparently the posterior hinge teeth of *Pterinea*, however, and until the characters of its hinge can be clearly determined, it would have been better to leave it in the genus *Ambonychia*.

In conclusion, we would state that we do not mean to assert that all the species described in this memoir are spurious, or placed in wrong genera, for some of them are undoubtedly distinct from all previously described forms, and have been properly referred; while the descriptions are generally sufficiently clear to enable the paleontologist, by the aid of the figures, to identify the forms described. The author also deserves credit for having published accurate figures of most of the species proposed by him, and it is to be hoped that he will publish figures of the others, so that all may form their own conclusions as to which of them should be adopted, and which arranged in the lists of synonyms. We must confess, however, that a careful study of his memoir has not left a very favorable impression in regard to the author's powers of discrimination, nor of his knowledge of the literature of the subject upon which he has essayed to write.

2. *Mont Alto Lignite and Appalachian Erosion;* by J. P. LESLEY. 30 pp., 8vo, with 4 plates. (From the Proc. Amer. Acad. Sci., 1864, pp. 463–482.)—Mr. Lesley here describes a bed of lignite found recently in Southern Central Pennsylvania. He regards it as of the same age with the lignite of Brandon, Vermont, described by Prof. Hitchcock, but not connected in any way with the iron ore (limonite) beds with which the latter associates them.[1]

Mr. Lesley enters at some length into the nature and origin of the ore-bed formation of the Atlantic border, and the position of the lignite, from which we cite the following:

"It consists everywhere of two parts, more or less easily distinguished; the one stratified in the same sense as the Silurian limestones themselves; the other a surface-wash over the basset edges of the first. The date of the formation of this local surface-wash may be Tertiary, and perhaps Post-tertiary. The stratified portions must be, as to their stratification, of Lower Silurian age; while the metamorphism which they have undergone, *in situ*, productive of stratified clays and ores, *may date from any time subsequent to the formation of a surface-topography approximately identical with that which now exists.* The actual change of the original Lower Silurian calcoferriferous sandstones and slates, *in situ*, at their outcrops, into limonite clay beds, *in ipso situ*, stratified as before, but charged with an additional percentage of the oxyds from a former higher surface now eroded, and with this extra charge of iron and manganese carried by percolation down to and crystallized against their foot rock,—this change may have required an immense time to perfect, and no doubt was going on, *pari passu* with the degradation of the surface by slow erosion, from higher to lower levels, until it stands at the level of the present day.

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[1] These limonite beds were long since shown by Percival to be, in part at least, contrary to the views of Prof. Hitchcock, altered beds of pyritiferous, micaceous and argillaceous schist *in place*. (See his Report (1842), p. 182, and also this Journal, [2], ii, 268, 1846.)

"The brown-hematite ore-deposits of Mont Alto follow the outcrop edges of the slates and sandy limestones which form the southeastern edge of the valley. The ore is in fact nothing but the residue of these beds after decomposition and dissolution, the honeycombed and altered edges of the Silurian slates and sand-limes themselves, after their lime has been washed out of them, and their carbonated and sulphuretted iron has been hydrated and peroxydized. The muddy slates formed the present deposits of small ore with white and red clay. The sandy limestones formed the present harder, siliceous, rock-ore belts. The geologist can procure, in the banks, specimens of every stage of this interesting process, from the perfect limestones which refused to disintegrate, and the iron-lime-sandstone with the disintegration and recrystallization begun, to the perfect ball and pot ore of radiated, acicular, crystallized brown-hematite. The great variety in the composition of the original rocks has been the cause of a great diversity in the ores taken from the different openings. But two principal distinctions may be particularly noticed; viz: that the ores which have resulted from the decomposition of the slates are more disposed to the *redshort* side, whereas the ores which have resulted from the decomposition of the limestones are more or less *coldshort;* probably because of the sand in the limestones; it is, in fact, called by the New York geologists the Calciferous sandrock. The slates, on the contrary, are apt to hold a small percentage of sulphur; or perhaps we should say, are less likely to permit the abundant drainage needful for carrying off the sulphur in the form of a salt. Sometimes in the same deposit there is a mixture of the two varieties, producing a neutral ore. But it is not often that such large exposures of both varieties occur in the same neighborhood, as is the case here.

"Taking into view all that we know of these deposits along the southeast side of the Great Valley from the Hudson river to Tennessee and Alabama, and adding what we know of similar deposits, produced in a similar way, out of the exposed outcrop edges of the same rocks in the limestone valleys further back toward the Allegheny Mountains (such as Kishicoquillis, Nittany, &c.), and deposits, in the same geological positions in Lancaster and Chester counties, we can divide them with great certainty, as stated above, into two classes, the *slate-crop banks*, and the *sand-lime-crop banks*, the former being always geologically underneath the latter."

Mr. Lesley also describes the positions of the lignite beds, and refers them to the Tertiary age, as done by Lesquereux (though without mentioning the paleontological evidence on this point afforded by the Mont Alto lignite). He then points out the fact that these beds of lignite and limonite indicate that, to a great extent, the surface-erosion of the Appalachian and Green Mountain regions antedates the Tertiary period of the lignite. He also refers for further evidence with regard to early erosion over these regions to the New Red Sandstone or Triassico-Jurassic of the Atlantic slope, as follows:—

"The New Red is seen in the section dipping northward against or toward a country, the surface of which is three hundred feet lower than its own. There is no evidence of a wide extension of New Red over that lower surface in the New Red age. On the contrary, not a hillock or gravel patch of New Red is to be found throughout the whole Paleozoic country to the north or west of this, its present absurdly constructed overhanging and outdipping margin. How is this to be accounted for?

"There must have been some barrier to the New Red waters between the Schuylkill and the Susquehanna, to correspond with the barrier which we see everywhere else between the Hudson and the James. Otherwise the New Red waters would have overflowed, by *at least* three hundred feet, the Silurian valley in its rear, and penetrated to valleys still further back by means of the principal gaps in the Kittatinny mountains through which the Schuylkill, the Swatara, and the Susquehanna rivers flow. What was this barrier?

"I think none can be suggested but one composed of the *originally much more elevated surface of the Silurian valley itself.* Carry up the whole mean level of the Paleozoic area—the valley beds up to the present height of the mountains, and the mountain crests to a proportionately greater altitude, the gaps to correspond with both, and the anticlinal and synclinal structure to determine the face of the surface at any given stage of the process,—and we have the required barrier to the estuary of the New Red; the explanation of its top Conglomerate; a good reason why there are no New Red traces back of the South mountains; and a closer date for the Lignite of Mont Alto."

Referring to a plate illustrating the paper, he says, it "is noticeable, 1. How vast an amount of Paleozoic rock-substance has been swept away; and, yet, that amount represents only the waste of the four lower Paleozoic formations; superposed upon these at a still older date, eight others, including the Coal-measures, must have formed their surfaces; supposing no cataclysm. 2. How fine a chance was given for collecting toward the present surface the ferruginous elements of the slowly decomposing and cavernous-becoming limestone layers; and 3. How the erosion must have acted, for some reason or other, more upon the Paleozoic surface outside, than upon the Paleozoic surface inside the limits of the New Red; the reason probably being, simply this: that the latter was under the New Red waters, and was being covered up, while the other was being eroded; but the erosion had not yet brought the valley surface down to the New Red water-level, when the uplift of the New Red took place. After which, the two erosions went on with different velocities proportional to the different solubilities, &c., of the Silurian limestone, and of the New Red sandstone, formations.

As for the lignite, therefore, it must have been subsequent to the erosion of the New Red, that is, certainly not older than the Cretaceous lignites of the United States; and when we consider the immense lapse of time needful for carrying the Silurian valley surface from a level with the tops of the New Red Hills, down to a level with their feet, we may well believe that the precise condition of the ore deposits as we see it, while it commenced before New Red times, was not perfected until the latest

Tertiary age, and, therefore, this last must be the age of the lignite—apart from all consideration of fossils.

3. *Eruption of Etna.*—A letter by Mr. Fouqué in "Les Mondes" of the 6th of April, contains the following: At half past 10, on the evening of the 21st of January, there was a severe earthquake, and immediately after, the eruption commenced. It broke out on the northeast side of the mountain at a point about 1700 meters above the sea-level and 500 meters from the foot of the old cone of Frumento. In two or three days the lava had flowed on 6 kilometers with a breadth of 3 to 4, and a variable thickness sometimes amounting to 10 or 20 meters. The craters are seven in number. Four kinds of fumaroles exist there, the *dry*, on the incandescent lava; the *acid*, where the temperature is above 400° C.; the *alkaline*, where the temperature is below this, but mostly above 100° C.; and the *carbonic*, in an old crater near by, where there is the ordinary temperature. There was a remarkable absence of sulphur and all its compounds, its odor being not perceptible over the lavas, and paper containing acetate of lead not being blackened by the fumes. The muriate of ammonia was detected in the *acid* fumaroles, and even in the *dry*, (although in these in but small quantity), as well as in the alkaline. The four lower craters detonated differently from the three others. The detonations of the latter were two or three per minute, and resembled thunder, those of the former were a continuous series, too rapid to be counted, comparable to the blows of a hammer on an anvil.—*Les Mondes, April 6.*

4. *Volcano of Kilauea.*—As all information relating to Kilauea will be read with interest, we insert the following received from Mr. Coan.

"The submerging and uprising of the island in the boiling cauldron of the crater, is a rare and grand spectacle. The same phenomenon occurred in June last, with this difference, that the island disappeared entirely for several days, but was gradually restored by the spouting liquid lava.

"I was at Kilauea on the 9th and 10th inst. There was much action in the great cauldron "Halemaumau." The lavas were boiling with vehemence, as if preparing for action. Besides this raging lake, I saw seven other fires at different points in the crater. One lake was boiling most intensely, about a mile from the fountain-lake in a northwesterly direction. The natives told me that, a day or two previous to my arrival at Kilauea, a jagged cone, of the size of a church, and forming an elevated island near the center of the igneous lake, became so undermined by the intense boiling of the lavas along its apparent base, that it fell over, and was submerged in the fiery abyss; but that, after a little time, it rose again, like a whale from the deep, and shook cataracts of molten minerals from its burning brow. In visiting a *Pulu* station on the highlands, some fifteen miles from Kilauea, I passed many large pit and cone craters, most of them ancient and densely wooded, from 300 to 800 feet in height and depth. I spent a night near a beautiful pit-crater called *Napau*, nearly circular, about 300 feet deep, a mile, perhaps, in diameter, and with a bottom of sand, so smooth and hard that a regiment of cavalry might be reviewed there. One-eighth of a mile from this crater, fissures are opened in the earth, out of which scalding steam and smoke have issued from time immemorial, and affording heat enough to cook for an army."—*Honolulu Commercial Advertiser*, Dec. 10, 1864.

5. *Addition to Prof. Shepard's Notes on the minerals of the Emery mine at Chester, Mass.*, (see p. 112); by the Author.—I have just found the diaspore at Chester, in broad, nearly transparent white laminæ, with a structure like that of kyanite, also of a most delicate violet color, deeper than that of Chemnitz.

White massive corundum, in veins half an inch thick, occurs traversing the massive emery. The latter mineral at Chester is exceedingly uniform in composition, and may be regarded as an aluminate of protoxyd of iron, $\dot{\mathrm{F}}\mathrm{e}\,\dddot{\bar{\mathrm{A}}}\mathrm{l}$.

A vein of Indianite, many inches thick, is found near the tunnel on South mountain, running for many rods through the chloritic rock on the east side of the emery-vein (exterior to its gneissoid wall). This chloritic seam is called by the workmen "*the fringe-rock.*" Small particles of crystalline corundum are diffused through the Indianite.

Masonite (the variety near to ottrelite) is also abundant at many places in the emery-vein on the North mountain. Brookite rarely attends the diaspore and corundophilite.

I would also add that emery occurs in fine grains widely diffused throughout the talcose rock which is the contiguous formation of the emery on its eastern side. Indeed I regard this as the parent rock of the emery, out of which it was deposited (when the strata were horizontal), just as the emery of the Grecian archipelago and Turkey was segregated in fine limestone.

Amherst, Mass., June 23, 1865.

6. *Geological Map of Russia.*—An excellent geological map of Russia containing the most recent discoveries up to the date of publication was prepared by Mr. H. Helmersen and issued in 1863. It presents by colors the distribution of the formations over European Russia and the Urals, and the adjoining countries to and beyond the Aral sea of the east, Constantinople on the south, and Stockholm and Cracow on the west. The lettering upon it is wholly in Russian; and as it may prove convenient to some persons ordering the map, we here give the English names of the formations corresponding to the several numbers: 1, Alluvium; 2 and 3, Pliocene, 2, newer Caspian, 3, ancient Caspian; 4, Miocene; 5, Eocene; 6, Cretaceous; 7, Jurassic; 8, Triassic; 9, Permian; 10, Carboniferous; 11, Devonian; 12, Silurian; 13, Metamorphic schists; 14, Granite and gneiss; 15, Eruptive rocks. The lines, unbroken and dotted, stand for railroads; and the black dots for coal beds. The geology of the region between the Black Sea and Caspian is by the geologist, Abich, and has a separate numbering and coloring: *a*, Pliocene; *b*, Miocene; *c*, Eocene; *d*, Cretaceous; *e*, Jurassic and Triassic; *f*, Carboniferous limestone and Devonian; *g*, Metamorphic schists; *h*, Granite and Protogine; *i*, Eruptive rocks, not volcanic; *k*, Volcanic rocks; N, naphtha springs, solfataras, mud volcanoes. The map is accompanied by a pamphlet descriptive of the formations.

7. *On the Changes rendered necessary in the Geological Map of South Africa, by recent Discoveries of Fossils;* by Dr. R. N. RUBIDGE, (Proc. Geol. Soc.)—Dr. Rubidge first called attention to a former paper, in which he pointed out the occurrence of horizontal beds of sandstone resting on the upturned edges of gneiss, and continuous with inclined sandstone of like

kind, interstratified with gneiss. He therefore conjectured that the Clay-slate and Bokkeveldt schist, which Bain considered distinct, belonged to one formation, that they are of the same age as the gneiss, and that the "Carboniferous rocks" of the Eastern province were not separable from the clay-slate, which Mr. Bain had called primitive clay-slate. It follows from this that if the clay-slate proved Devonian, as Mr. Rubidge believed it would, the horizontal quartzite must be much newer, and probably an outlying mass of the Dicynodon rocks. He explained these phenomena by supposing that rocks of widely different ages had been metamorphosed into masses having the same mineralogical characters. The discovery of certain fossils has lately verified the conjecture respecting the Devonian age of the clay-slates and Bokkeveldt rocks; and Dr. Rubidge therefore infers that the rest of the old rocks are of the same age. Finally, the discovery of a Calamite in the sandstone, not unlike some specimens belonging to the same genus found in the Dicynodon rocks, renders the probability of the truth of the second conjecture very great.—*Reader, May* 20.

8. *Anthrakerpeton, a new Carboniferous Reptile.*—Prof. Owen has described a fossil reptile from the Coal-beds of Llantrissent in Glamorganshire, Wales. It comes from the lower part of the "Middle" if not the upper part of the "Lower" Coal-measures. The species is intermediate in size between the *Baphetes* and *Dendrerpeton;* the ribs are longer than in any known Labyrinthodont, and these and the limb bones indicate that the animal belonged to "that low air-breathing type which, with developmental condition of the bones like those in some fishes, and very common in Devonian, showed forms of the skeleton more like those in Saurian reptiles, than in the modern air-breathing Batrachians."—*Reader*, Jan. 7.

9. *Mineral wealth of Mexico.*—Baron de Morner, a Swedish man of science, is reported to have found a bed of anthracite of excellent quality in the district of Guerrero, Mexico. Already a deposit of bitumen and of two beds of coal are under exploration in the district of Iturbide, besides a source of petroleum near Tenancingo, and a vein of cinnabar at Sultepec. Two Frenchmen, MM. Favre and Gabriel, are about to undertake the working of the iron ore of the district of Chalco, where railroads are being rapidly built.—*Les Mondes*, March 23.

10. *Gneiss with the impression of an Equisetum.*—The museum at Turin contains a fragment of gneiss from an erratic block, derived apparently from the Valteline, from the mass of crystalline rocks of that region which underlie the Infraliassic group of Sismonda. Mr. Sismonda regards the fossil as proof of the metamorphic character of the fundamental gneiss of the Alps, and as affording a fact bearing on the age of the vegetable impressions accompanying the anthracitiferous beds of the Western Alps.—*Les Mondes*, March 23, p. 532.

11. *Kalicine.*—H. St. Claire Deville thus names bicarbonate of potash, of the same composition with that of the arts, found native at Chypis in Valais. An analysis afforded carbonic acid 42·2, potash 46·6, carbonate of lime 2·5, carbonate of magnesia 1·34, sand and organic matters 3·60, water 7·76, corresponding to the formula KO, CO^2+HO.

12. *Geological Excursion.*—Mr. FRANK H. BRADLEY, a member of the Zoological and Paleontological Department of the Sheffield Scientific School, proposes to take charge of a few students, for the purpose of giving a practical introduction to geology, during a trip of about four weeks through the State of New York.

He will meet his class at Burlington, Vt., on Monday, July 31st, and commence work on the following morning at Port Kent on the opposite side of the lake.

The trip will include visits to some of the fine scenery for which New York is famous, as well as to characteristic and productive localities of nearly all the formations from the Potsdam sandstone to the Chemung group, for whose examination, within a small extent of country, the State affords the best opportunity in America, if not in the world.

For some years past Mr. Bradley has been engaged in the study of the New York rocks and fossils, and has made quite extensive collections therefrom. We recommend him for the charge which he undertakes.

Mr. B. informs us that the expenses while with him need not exceed one hundred dollars besides his fee of thirty dollars.

Communications addressed to him at New Haven will be received as late as July 25th.

III. BOTANY AND ZOOLOGY.

1. *Thwaites, Enumeratio Plantarum Zeylaniæ; an Enumeration of Ceylon Plants*, etc., is now completed with the publication of part 5. It forms an octavo volume of 483 pages, and is the only separate work of any consequence upon the botany of Ceylon since the publication of the "Flora Zeylanica" of Linnæus and the "Thesaurus Zeylanicus" of Burmann. In the preface, when referring to the changes which are occurring in the vegetation of the island, through man's direct or indirect agency, Mr. Thwaites records the following interesting fact. "The obtrusive character, too, of a plant brought to the island, about forty years ago, is also helping to alter the character of the vegetation up to an elevation of 3000 feet. The plant alluded to is the *Lantana mixta*, Linn., which appears to have found in Ceylon a soil and climate exactly suited to its growth; for it now covers thousands of acres with its dense masses of foliage, taking complete possession of land where cultivation has been neglected or abandoned, preventing the growth of any other plants, and even destroying small trees, the tops of which its subscandent stems are able to reach. The fruit of this plant is so acceptable to frugivorous birds of all kinds, that through their instrumentality it is spreading rapidly, to the complete exclusion, in spots where it becomes established, of the indigenous vegetation." Dr. Hooker has found time to render his invaluable aid in the identification of the species and synonymy, and in superintending the publication at London, Mr. Thwaites remaining at his post in Ceylon. A. G.

2. *Flora Capensis;* by Drs. HARVEY and SONDER.—The third volume of this standard work, comprising the Monopetalous Orders from *Rubiaceæ* to *Campanulaceæ* inclusive, was issued early in the present year, the preface bearing date, Feb. 24. Nearly five-sixths of the 630 pages of

this volume, are devoted to the order *Compositæ*, which is elaborated by Prof. Harvey. *Aster* is here pretty largely represented, the genus being, as might be expected, brought back almost to its original limits by the suppression of *Felicia*, *Agathæa*, *Munychia*, and *Bellidiastrum*, as well as *Tripolium*. (There ought now, therefore, to be no question about the admissibility of the Polynesian *Agátea* of the Violet Family, which has been thought to be in pronunciation too like *Agathæ'a*.) The price of the volume has been raised from twelve to eighteen shillings, which is still very reasonable. A. G.

3. *Thesaurus Capensis*, by Prof. Harvey, the excellent companion of the above Flora, has reached the two-hundredth plate, completing the second volume. The most interesting plant figured in the last fasciculus is *Hydnora triceps*, illustrated upon a double plate. A. G.

4. *Ammobroma Sonoræ* (literally, the *Sand-food* of Sonora) is the name of an extraordinary root-parasitic plant, of the region at the head of the Gulf of California, which Dr. Torrey has just described and figured in the 8th volume of the Annals of the Lyceum of Natural History, New York. It has been briefly noticed before (but never fully characterized) as a new genus, allied to the rare Mexican *Corallophyllum* of Kunth (or *Lennoa*, Lexarza), and still more to the Californian and hardly better-known *Pholisma* of Nuttal. It hardly throws any new light upon the affinity of these strange plants, which, though justly thought to be rather Monotropaceous than Orobanchaceous, are still obscure. This plant, growing in a forlorn sandy desert, almost covered by the sand in which it lives, was found by its discoverer, the late Col. A. B. Gray, to form a considerable part of the sustenance of the Papigos Indians of the district, and is said to be very luscious when first gathered and cooked, resembling in taste the sweet potato, only far more delicate. A. G.

5. *Annales Botanices Systematicæ*, tom. VI.—With the fourth volume, Dr. Walpers having deceased, the editorship of this work passed into the hands of Dr. Carl Müller of Berlin; and fascicle 7 of the sixth volume, just issued, concludes the enumeration of the Phanerogamous species published from 1851 to 1855 inclusive, and carries down to the letter C the alphabetical index of these three volumes. The remainder of the index will occupy fasc. 8 of this bulky volume, which began to be issued in 1861. The accumulation of species published within the last 10 years (since 1855) is perhaps equally large; and, if treated in the same way, their enumeration may be completed in the year 1875. It is to be regretted that the compacter plan, adopted in the earlier volumes of the series, was not adhered to. A. G.

6. *The Production of Organisms in closed vessels.*—As appears from an abstract in the *Reader* of May 20, a paper by George Child, M.D., in continuation of a former communication, was read before the Royal Society, April 27. Dr. Child's experiments were similar to the earlier ones of Prof. Wyman, and reach the same results, viz. that Bacterians are produced "exactly under the circumstances in which M. Pasteur asserts that they do not exist." And he accounts for the discrepancy thus. "M. Pasteur, in his memoir, speaks of examining his substances with a power of 350 diameters. Now my experience throughout has been, that it is impossible to recognize these minute objects, with any degree of cer-

tainty, even with double that magnifying power. When once their existence on a slide is shown with a power of 1500 to 1700 diameters, it is quite possible afterward to recognize the same object with a power of 750," &c. He continues: "I can now have no doubt of the fact that 'bacterians' can be produced in hermetically sealed vessels, containing an infusion of organic matter, whether animal or vegetable, though supplied only with air passed through a red-hot tube, with all necessary precautions for ensuring the thorough heating of every portion of it, and though the infusion itself be thoroughly boiled. * * * "It seems clear that either (1) the germs of Bacterium are capable of resisting the boiling temperature in a fluid, or (2) that they are spontaneously generated, or (3) that they are not 'organisms' at all. I was myself somewhat inclined to the latter belief concerning them at one time; but some researches in which I am now engaged have gone far to convince me that they are really minute vegetable forms. The choice, therefore, seems to remain between the other two conclusions. Upon these I will not venture a positive opinion, but remark only, that if it be true that 'germs' can resist the boiling temperature in fluid, then both parties in the controversy are working upon a false principle, and neither M. Pouchet nor M. Pasteur is likely at present to solve the problem of spontaneous generation." The decided conclusion as to the organic character of these Bacteriums was reached through an examination of them by the $\frac{1}{50}$ object-glass recently constructed by Messrs. Powell and Lealand. A. G.

7. *On the absorption and assimilation of carbonic acid by plants.*—Mr. BOUSSINGAULT has recently made some experiments on the absorption and assimilation of carbonic acid by leaves exposed to sunlight. His results are thus summed up: 1. Leaves exposed to the sun in *pure* carbonic acid do not decompose this gas, or if they do, it is with excessive slowness. 2. Leaves exposed in a mixture of carbonic acid and atmospheric air rapidly decompose the former gas. Oxygen does not seem to interfere in the phenomenon. 3. Carbonic acid is rapidly decomposed by leaves when that gas is mixed with either hydrogen or nitrogen. The author has pointed out some analogies of these phenomena with the slow combustion of phosphorus under certain circumstances. Thus phosphorus placed in *pure* oxygen does not become luminous, and does not burn, or if it does, burns with excessive slowness. In a mixture of oxygen and atmospheric air, however, phosphorus burns rapidly; it also burns when placed in oxygen mixed with hydrogen, nitrogen, or carbonic acid. Phosphorus, which does not burn in pure oxygen at an ordinary pressure, becomes combustible when the gas is rarefied; and Mr. Boussingault found that, similarly, a leaf placed in rarefied pure carbonic acid decomposed the gas and evolved oxygen.—*Reader*, May 27.

8. *Classification of Polyps;* by A. E. VERRILL.—The following subdivisions of the class of Polyps have been proposed by Prof. Verrill, in the Proceedings of the Essex Institute, vol. iv, p. 145.

ORDER I. MADREPORARIA.—Polyps simple or compound with embryonic or rudimentary basal or abactinal region, which has no special function unless for vegetative attachment while young. Actinal area well developed, form broadly expanded, having a tendency in the higher groups to become narrowed toward the mouth. Tentacles simple, conical. Dermal tissues, and usually the radiating lamellæ, depositing solid coral; the radiating plates being between the lamellæ, are, therefore, ambulacral, and appear to originate from the surfaces of the lamellæ and the connective tissues extending across the ambulacral chambers and filling them from below. Interambulacral spaces distinct.

Suborder I. Stauracea (*Madreporaria rugosa* [1]).—Coral simple, or compound by budding; chiefly *epidermal* and *endothecal; septa* apparently in multiples of four, sometimes wanting. Type embryonic, like a young Astræa or Fungia.

Families—Stauridæ, Cyathophyllidæ, Cyathaxonidæ, Cystiphyllidæ.

Suborder II. Fungacea.—Polyps either simple or compound by marginal or disk budding, rarely by fissiparity. Tentacles numerous, in multiples of six, imperfectly developed, scattered on the actinal surface, usually short and lobe-like. Upper part of polyps scarcely exsert. Coral broad and low, growth mostly centrifugal, tissue chiefly septal; walls imperfectly developed, often perforate, subordinate, usually forming the basal attachment.

Families—Cyclolitidæ, Lophoseridæ, Fungidæ, Merulinidæ.

Suborder III. Astræacea.—Polyps mostly compound, either by fissiparity or various modes of budding. Tentacles usually well developed, long, subcylindrical, limited in number, in multiples of six encircling the disk. Coral mural, septal and endothecal; growth vertical and centrifugal, producing turbinated forms which are often elongated.

Families—Lithophyllidæ, Mæandrinidæ, Eusmilidæ, Caryophyllidæ, Stylinidæ, Astræinæ, Oculinidæ, Stylophoridæ.

Suborder IV. Madreporacea (*Madreporaria perforata*).—Tentacles in definite numbers, twelve or more, well developed, encircling the narrow disk, therefore nearer the mouth; polyps with the upper portion much exsert, flexile; growth chiefly vertical; coral mural and septal, porous. Polyps compound by budding, sometimes simple.

Families—Eupsammidæ, Gemmiporidæ, Poritidæ, Madreporidæ.

ORDER II. ACTINARIA.—Polyps with well developed, often highly specialized, basal or abactinal region. Walls well developed, tentacles longer, more concentrated around the mouth, which is also, usually, if not always, furnished with special tentacular lobes or folds. Ambulacral spaces always open, destitute of connecting tissues and solid deposits.

Suborder I. Zoanthacea.—Polyps encrusting, adherent, budding from mural expansions; tentacles simple, short, at edge of disk.

Families—Zoanthidæ, Bergidæ.

Suborder II. Antipathacea.—Polyps connected by a cœnenchyma, secreting a solid sclerobase or coral axis. Tentacles few, six to twenty-four, simple, conical.

Families—Antipathidæ, Gerardidæ.

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[1] This group is placed here with considerable hesitation, and principally on account of the close resemblance in structure to the young of the succeeding and higher groups, when they first begin to form a coral, which then consists of a ring of epitheca or epidermal deposit with a few, imperfect, rugose septa radiating from the center. If the number four be a constant feature of the arrangement of their septa, it is possible that they may be entitled to rank as a separate order of Polyps. To this opinion Prof. J. D. Dana inclines. Prof. Agassiz unites the group with Hydroid Acalephs, on account of their resemblance, in some features, to the *Tabulata*. It seems to me, however, that the absence of transverse plates in *Cyathaxonidæ* and *Cystiphyllidæ* and the perfection of the vertical septa in *Stauridæ*, *Cyathaxonidæ* and some of the *Cyathophyllidæ*, together with their general structure, shows them to be more closely allied to the *Fungacea* and *Astræacea*, of which they may be considered embryonic types, while at the same time the group is a synthetic one, having analogies with nearly all the higher groups of Polyps, and also, in some respects, with Hydroids.

Suborder III. Actinacea.—Polyps free, capable of locomotion, with a highly specialized, muscular base or abactinal area. Tentacles well organized, either simple or branched, varying from ten to many hundreds, often with accessory organs arising from the same spheromeres, such as inner tentacles, verrucæ, complicated or simple branchial lobes, cinclidæ, eye-spherules, suckers, etc. Mouth with special lobes or folds. Most of the species are simple, a few are compound by fissiparity, many abnormally bud from the wall near the base, a few secrete from the base a horn-like deposit similar to the axis of Antipathes.

Families—Actinidæ, Thalassianthidæ, Minyidæ, Ilyanthidæ, Cerianthidæ.

ORDER III. ALCYONARIA.—Polyps with well developed actinal, mural, and abactinal regions, compound by budding. Tentacles eight, pinnately lobed, long, encircling a narrow disk. No interambulacral spaces. Ambulacral ones open and wide.

Suborder I. Alcyonacea.—Polyps turbinate at base, budding in various ways, encrusting, adherent to foreign bodies by the cœnenchyma.

Families—Alcyonidæ, Xenidæ, Cornularidæ, Tubiporidæ.

Suborder II. Gorgonacea.—Polyps cylindrical, short, connected by a cœnenchyma, secreting a central supporting axis.

Families—Gorgonidæ, Plexauridæ, Primnoidæ, Gorginellidæ, Isidæ, Corallidæ, Briaridæ.

Suborder III. Pennatulacea.—Polyps forming free, moving colonies, the composite basal portion with locomotive functions and special cavities, with or without a solid free axis.

Families—Pennatulidæ, Pavonaridæ, Veretillidæ, Renillidæ.

9. *Embryology of the Star Fish;* by ALEXANDER AGASSIZ. 70 pp. 4to, with 8 lithographic plates, from vol. v, of Prof. Agassiz's Contributions to the Natural History of the United States.—The microscopic researches here described, and very beautifully displayed on the eight plates, relate mainly to the *Asteracanthion berylinus* Ag., and *A. pallidus*. The author closes with the following important observations on the *Radiate type*, and its relation to the apparent bilaterality in the larval Echinoderm.

"From what has been said, it is evident that the plan of radiation underlies this apparent bilaterality of the Brachiolaria, and of the Pluteus. The throwing of the whole of the stomach and the alimentary canal on one side, the complicated system of arms arranged with perfect symmetry on each side of the axis, passing through the mouth and the anus, does not change, though it partially conceals, the radiate plan. We have Holothurians which always creep upon three of their ambulacra, where a dorsal and a ventral side, an anterior and a posterior region, are subordinate to the plan of radiation; and the same takes place to a less extent in Spatangoids. Among Polyps even, which are, as it were, the simplest type of radiate animals, an anterior and posterior region are strikingly shown in the case of Arachnactis. The additional spheromeres are all added at one extremity of the mouth-slit, and yet the Actinia is made up of radiating spheromeres. The earliest stages of the larvæ of Echinoderms, before the apppearance of the water-tubes, remind us forcibly of the young Actinia soon after it has escaped from the egg, or of the first stages of growth of a Scyphistoma, after it has attached itself to the ground, previous to the formation of tentacles. What constitutes the difference in the structural plan of these animals belonging to different classes, in their primary stages of growth? They are all built according to one and the same idea, so carried out as to be eminently echinodermoid in one instance, acalephian in another, and polypoidal in a third. In young Echinoderms, as in young Ctenophoræ, we find nothing of the remarkable preponderance of certain parts which gives these young their bilateral appearance in more advanced conditions. Their radiate character is extremely prominent at first, but becomes gradually obscured and hidden under the guise of this bilaterality, which is, after all, due only to the excessive development of certain spheromeres as compared with the others.

"The case of these larvæ is only an additional example of what we find so often in nature, that one plan of structure apparently prevails, while, in reality, it is only an external analogy, obtaining a great predominance in certain parts, but subservient to the primary plan, even though the latter be perceived only on closer examination. This view solves a question which has hitherto perplexed all investigators of this subject,—viz: how it was possible that a larva, which has always been considered as bilateral, should produce a radiate animal by a process of internal gemmation. It is, indeed, a bilateral larva, but built upon a radiate plan; a larva recalling a lower class of this branch of the animal kingdom, an acalephian larva giving rise to an Echinoderm, which, from its very beginning, is a radiate animal, having all its spheromeres developed at the same time, and equally.[1]

"These transformations are, however, peculiar to the class of Echinoderms; they constitute neither a metamorphosis, nor a case of alternate generation. The egg becomes the embryo larva; nothing essential is lost during the process; no intermediate individual comes into the cycle. It is the yolk which becomes the larva, the latter being, in its turn, transformed into the young Echinoderm. This larva is, in short, an acalephian larva, reminding us somewhat of the twin individuals of free Hydroids, though adapted to the mode of development of the Echinoderms. But, in the latter, we have no intermediate condition corresponding to the Polyp-like Hydroid in Acalephs, from which the Medusæ or reproductive individuals arise, and, in their turn, bring forth the Hydroid again, which completes the cycle by developing another set of Medusæ.

"If the views here taken of the plan of development of Echinoderms be correct, they introduce a new set of facts respecting their affinities with the Polyps and Acalephs, which cannot fail to have an important bearing on the question of the separation of the Echinoderms as a distinct type from the two latter groups. The Echinoderm plutean form, with its mouth, stomach, intestine, with the water-system originally forming a part of the digestive cavity, bears, as it seems to me, the same relation to the Ctenophoræ which the Hydroid Polyps hold to the true Polyps. The Ctenophoræ may be considered, as it were, the prophetic type of the Echinoderms, as the Polyps are the prophetic type of Acalephs. We have in the Ctenophoræ a digestive cavity, from which branches the water-system, and that peculiar funnel, opening outward, through which the fecal matters of the Ctenophoræ are discharged, reminding us at once of the almost identical arrangement of an Echinoderm Pluteus, in the relations of the intestine to the stomach. The plutean forms certainly show that the plan, upon which the Echinoderms are built, does not differ from that upon which the Acalephs are built, and that we have between the Echinoderms and Acalephs the same connection, based upon the identity of plan which exists between the Acalephs and Polyps. We cannot, therefore, admit that the views so frequently urged, and so universally admitted, in support of the separation of the Acalephs and Polyps, as a distinct type (Cœlenterata), from the Echinoderms, have any real foundation in nature; and still less can we concur in them, when we remember that the main argument in their favor rests upon the assumed total want of connection between the ambulacral system and the digestive system. Now this connection has been shown, by Prof. Agassiz, to exist in the adult of many Echinoderms, while the facts above stated prove that it also exists in the early stages of the embryonic development, when, in fact, the water-system is formed from the digestive system. With this evidence falls the strongest argument for the validity of a classification by which the type of Radiates would be broken up, and the Polyps and Acalephs separated from the Echinoderms, as a distinct type, under the name of Cœlenterata. We are, therefore, justified in affirming that the type of Radiates constitutes an independent type of the animal kingdom, containing three equivalent classes,—Echinoderms, Acalephs, and Polyps."

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[1] For a closer comparison of young Ctenophoræ and Echinoderm Larvæ, see the Illustrated Catalogue of the Museum of Comparative Zoology, No. II, now in press.

IV. ASTRONOMY.

1. *Note on the inclination of the planetary orbits to the invariable plane;* by Prof. G. Hinrichs.—On page 138 of the preceding volume appears an editorial footnote stating that the numbers given by us for the inclinations I of the planetary orbits toward the invariable plane "are not quite exact." As this remark might create doubt of the general closeness of the planetary orbits, the following additional developments seem necessary.

Let i be the inclination of the orbit, and i' the inclination of the invariable plane toward the ecliptic. As the values of i themselves are subject to quite considerable changes, we deemed it unnecessary to calculate the exact values of I, and made use of the equation

$$I = i' \backsim i,$$

i. e., subtracting the smaller from the greater angle.

The remark of the editor has induced us to calculate the exact values of I in order to see how great an error is committed in neglecting the spherical excess Δ of the spherical triangle. From the fifth edition of Mädler's Astronomie Populaire (Berlin, 1861) we take the following elements:

	☊	i
Mercury,	46° 23′ 55″·0	7° 0′ 13″·3
Venus,	75 11 29 ·8	3 23 31 ·4
Mars,	48 16 18 ·0	1 51 5 ·7
Jupiter,	98 48 37 ·8	1 18 42 ·4
Saturn,	112 16 34 ·2	2 29 29 ·9
Uranus,	73 8 47 ·8	46 29 ·2
Neptune,	129 59 23 ·1	1 47 0 ·9
Invariable plane,	106 0 49 ·0	1 35 27 ·9 (for 1800)

These values give the following results (stated to the nearest minute):[1]

	I	$i' \smile i$	Δ
Mercury,	6° 18′	5° 25′	53′
Venus,	2 11	1 49	22
Mars,	1 40	16	1° 24
Jupiter,	18	16	2
Saturn,	55	64	1
Uranus,	1 2	49	13
Neptune,	45	12	33

We have also given the values according to the formula made use of in our article, page 138—and the difference of the two first values will give the spherical excess Δ neglected in that approximation.

It will be seen that the deviation only in the case of Mars exceeds one degree, and in the case of Mercury is nearly one degree; but these belong to the smaller planets. Of the large planets there is only Neptune making a considerable deviation (33′) from our former value; the very near coincidence between the orbit of Neptune and the invariable plane, pointed out by Mr. Trowbridge (vol. xxxviii, p. 355) does not *now* exist. If Neptune is not in its maximum of inclination, then this circumstance would make it highly probable that there must be another planet beyond Neptune (§ 5, I).

Thus it appears that our numbers for I given on page 139, though approximations only, and therefore not quite exact, still are sufficiently near the truth to warrant the deductions then drawn therefrom, except in the case of Neptune, which planet at present deviates about 50 minutes from the invariable plane, and thus appears not to be the *ultima thule* of the planetary world.
Iowa City, April, 1865.

2. *New Comet.*—A large comet was visible in the southern hemisphere, in the months of January and February. It was seen at Rio Janeiro on the 24th of January. On the 26th, its tail was 26° in length. The following elements were computed by Mr. Moesta from observations on Feb. 21st, 25th, and 29th. On the 20th, he observed a second very faint tail branching out to the north of the principal tail.

Perihelion passage, 1865, Jan. 14·3867,

$\pi = 3° 33'.79$ $\quad i = 92° 17'.16$

$\Omega = 255\ 46.27$ $\quad \log. q = 8.4511$

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[1] Obtained from the formulæ

$$\sin\frac{I}{2} = \frac{\cos\frac{A+B}{2}}{\cos\theta} = \cos\frac{A-B}{2}\cos\phi,$$

where

$$\tan\theta = \frac{\sin\frac{c}{2}}{\cos\frac{A+B}{2}}\sqrt{\sin A . \sin B},$$

$$\sin\phi = \frac{\cos\frac{c}{2}}{\cos\frac{A-B}{2}}\sqrt{\sin A . \sin B},$$

and c the difference of the longitude of the nodes, and A and B the other two angles of the triangle, i and $180 - i'$, or $180 - i$ and i'.

3. *Mr. Huggins on the Spectrum of the Nebula in Orion.*—At the meeting of the Royal Ast. Soc., March 10th, 1865, Mr. Huggins remarked that "the recent examination of the Great Nebula in *Orion* shows that this large and wonderful object belongs to the class of gaseous bodies. The light from this nebula resolves itself under the refractive power of the prism into the same three bright lines. With a narrow slit they appear exceedingly thin and well defined. The intervals between them are dark, and in the light from no part of this nebula was any indication detected of a continuous spectrum, such as is characteristic of incandescent solid or liquid matter. Different portions of this great nebulous mass were brought successively upon the slit, but the results of minute examination showed that the whole nebula emits light which indicates a constitution identical throughout the body. The light from one part differs from that of another in intensity alone.

The four bright stars of the trapezium and other stars distributed over the nebula gave a continuous spectrum.

According to Lord Rosse and Prof. Bond, the brighter parts near the trapezium consist of clustering stars. If this be the true appearance of the nebula under great telescopic power, then these discrete points of light must indicate separate and probably denser portions of the gas, and that the whole nebula is to be regarded rather as a system of gaseous bodies than as an unbroken vaporous mass.

Since the usually received opinion of the enormous distances of the nebulæ has no longer any foundation to rest upon, in respect of the nebulæ which give a gaseous spectrum, it is much to be desired that *proper motion* should be sought for in such of them as are suitable for this purpose.

If the gaseous matter of these objects represented the 'nebulous fluid,' out of which, according to the hypothesis of Sir Wm. Herschel, stars are to be elaborated, we should expect a spectrum in which the groups of bright lines were as numerous as the dark lines due to absorption found in the spectra of the stars.

If the three bright lines be supposed to indicate matter in its most primary forms, still we should expect to find in some of the nebulæ, or in some parts of them, indications by a more complex spectrum, of an advance in the formation of the separate elementary bodies which exist in the Sun and in the stars.

A progressive formation of some kind is, however, suggested by the presence in many of the nebulæ of a nucleus, the spectrum of which indicates that it is not pure gas, but contains solid or liquid matter.

It may, therefore, be, that nebulæ which have little indication of resolvability, and yet give a *continuous* spectrum, such as the Great Nebula in *Andromeda*, are not clusters of suns, but gaseous nebulæ which, by the gradual loss of heat, or the influence of other forces, have become crowded with more condensed and opaque portions.

So far as my observations extend at present, they suggest the opinion that the nebulæ which give a gaseous spectrum are systems possessing a structure, and a relation to the universe, altogether distinct from the great group of cosmical bodies to which our Sun and the fixed stars belong."—*Ast. Soc. Notices.*

4. *On the Meteorite of Mamboum, Bengal;* by Mr. HAIDINGER.—This meteorite fell 130 miles northeast of Calcutta, on the 22d of December, 1863, at 9 A. M., during a calm. The detonations which accompanied the fall were heard to a distance of 30 miles; and some hours afterward a large fragment was collected at Corsipore, a second smaller at Pandra, and others, as large as a nut, near Govindpur. The paste or base of the stone is ash-gray and is distinctly brecciform in structure, without rounded granules. Monosulphuret of iron is abundant in it, though only in minute particles; grains of metallic iron are less numerous. The specific gravity is 3·424.—*Ber. Wien. Akad.*, Sept. 21, 1864.

5. *New Asteroid.*—Another small planet was discovered April 26th by de Gasparis at Naples. It was equal in brightness to a star of the 10th magnitude.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Experiments on the production of cylinders of ice by pressure through orifices;* by Mr. FRESCA, (Proc. Acad. Sci., Paris, for Feb. 21).—These experiments were made by acting on ice contained in a cylinder 0·16 meter (about 6 inches) in diameter, with the requisite pressure to drive it through a central orifice in its base 0·05 m. (nearly two inches) in diameter. These plates, prepared by Tyndall's method, in some cases colored at the joints and in others not, acted like plates of lead or of porcelain paste, as before explained by the author to the Academy. The surfaces of the planes of division or joints, originally flat, are transformed by the process, as with other substances, into tubes concentric and perfectly distinct from one another, thus indicating the movement of each point of the mass in the course of the transformation. The cylinders of ice are longitudinally furrowed, and the furrows appeared to proceed from fractures produced at the moment when a portion of the cylindrical block leaves the orifice, and when, as a consequence, it ceases to be subjected to pressure at the outer extremity. The porcelain paste afforded the author delicate furrows or fractures of a similar kind, and in some cases the material was divided into small separate lamellæ. The evenly-bedded structure of the cylinder of ice shows that the origin of these fractures is subsequent to the first formation of the cylinder.

For a block of the dimensions employed in the experiments the pressure required for the flow of the ice is 10,000 kilograms, while for lead it is 50,000 kilograms. These pressures correspond, for the square centimeter, to 126 and 637 kilograms. This pressure of 126 kilograms corresponds to a column of water 1300 meters in height. It is evident that if the orifice were smaller in proportion to the diameter of the mass under pressure, the force requisite would be notably less.

Mr. Fresca observes that the circumstances attending the formation of these jets of ice appear to throw light on the controverted question as to the movement of glaciers. The relative displacement of the layers of ice in the process, the change of form in the faces originally flat, the curved form of the beds at the extremity of each partial tube, the large cavities formed toward these extremities, and even the fissures or fractures at the moment of escape from the pressure, are so many points of resemblance to the phenomena of glaciers. There is not the mass of material constituting moraines, but the traces of coloring matter which are deposited in parallel threads, and which are reunited toward the axis, complete to a degree the analogy.

Tyndall had before shown that ice could be moulded to any shape by pressure in a mould. Mr. Fresca observes that his trials prove, in addition, that it may by pushed into a thread, in accordance with the geometrical law of this kind of flow. The facts help to bring the explanations of Tyndall and Forbes into accord, since they indicate that the viscosity, more or less great, of the material does not necessarily play any important part in the phenomenon. They exhibit the material conditions of the flow: and the transparency of the jet, after its escape from the orifice, shows, besides, that, under a comparatively feeble pressure, ice may be changed in form without ceasing to be glassy in texture or aspect.—*Les Mondes, Feb.* 23.

2. *Observations on Sepulture in the Age of Stone, between Castries and Baillargues, France;* by P. GERVAIS.—A natural excavation, several yards in length, has been discovered in the Commune of Baillargues, which had been used for the burial of a number of human bodies of the Age of Stone. The bones were those of adults, and several were of quite an advanced age, as shown by the teeth. One of them indicated a man of high stature; the femur was 0·465 meter long. A cranium presented to the Academy of Sciences by Mr. Gervais was of the type of the white race, it having a brachycephalus head without a trace of prognathism, and a well-developed forehead. It appeared to have belonged to a female, and not one of advanced years. Flint implements found with the remains are regarded as fixing the age to which the people here interred belong. There are also in the graves numerous disks of carbonate of lime having a hole at center, and some calcareous objects of other forms, a few fragments of coarse pottery, bits of charcoal, and bones of some modern species of animals, as of the fox, hare, sheep, vertebræ of snakes, and femurs of frogs.

Mr. Gervais concludes from his observations, that, in the remote period referred to, the country of Castries and much of southern France were inhabited by the race here indicated.—*Les Mondes, Feb.* 23.

3. *Lake-habitations.*—Mr. MESSIKOMER, of Zurich, has again succeeded in making some highly interesting discoveries and observations by his continued excavations in the large turf-bed near Robenhausen. It is true that these latest discoveries do not give the key to the chronological enigma of the Pale buildings and their inhabitants; but they spread a clearer light on the manner of living in that remote period, as well as its condition of civilization. Hitherto it was believed that only two of these old settlements existed on this curious spot, one above the other; these recent observations make it plain that there are three, one on the top of the other. The two oldest settlements have been destroyed by fire, and furnish a great number of antiquarian objects. The third settlement, however, the pales of which do not consist of round wood, but split oak, had not been destroyed by fire, but had been abandoned in the course of time; it is poor in remnants of interest. All three settlements which must have existed for a great number of years, belong to the Stone period. Among the astonishing quantity of objects of industry recently

excavated by Mr. Messikomer, only tools of stone, bone and wood have been found, many for uses unknown, but not the slightest trace of bronze or iron. The distinct separation and length of duration of the pre-historical periods of the so-called Stone period and Bronze period have nowhere been more clearly demonstrated than in the Pale buildings of Switzerland, much clearer and more precise even than in those of Denmark and Mecklenberg. No settlements of the oldest men, among those known till now, can, in fact, be compared in size and preservation with the large lake-villages of the Stone period at Robenhausen, Wauwyl and Wangen, on Lake Constance.—*Athenæum, May* 27.

4. *On the Human Remains of the Trou du Frontal;* by P. J. Van Beneden and Dr. E. Dupont.—Remains of fourteen individuals have been found at the cave, called (from the discovery of a cranium) Trou de Frontal. Of the two best crania, one is orthognathous, the other prognathous; and still the prognathous one has the largest cavity. The various other bones include fragments of crania and of nearly every other portion of the skeleton, part of them the remains of children, and others of youths and adults. All lie in complete disorder, as if they had been disturbed by some catastrophe subsequent to their first burial; and with them are bones of various mammals of kinds now living, besides different species of Helix, Cyclostoma and Unio, flint implements, ornaments, amulets, coarse pottery, a crystal of fluor spar, a bone needle, etc.

5. *Instrument for measuring distances.*—Dr. Emsmann, in a paper in Poggendorff's Annalen, describes a new instrument for measuring distances, which differs from all previous arrangements by being independent of the measurement of angles, or of a base line. It consists simply in an application of the well-known principle that the image of an object is brought to a focus by a convex lens at a distance from the lens varying according to the remoteness of the object. The arrangement described by Dr. Emsmann consists of an object-glass of 30″, and an eye-piece of 1″ focal length, a screen of ground glass, upon which the image is received, being placed behind the eye-piece. The instrument, it will be seen, resembles in principle a photographic camera; the length, however, is about 5½ feet. In order to keep the indications within certain limits, the screen is placed behind the eye-piece, and the distance between the lenses is so arranged that a variation in the distance of 25 paces, at all ranges, requires at least a movement of one line in the screen. Trustworthy readings may be obtained up to 2000 paces. Dr. Emsmann suggests that the instrument will be found useful in coast batteries, for measuring the distance of a vessel out at sea. In siege operations, the time generally admits of the measurement of a base line, the distance of the enemy's works being calculated by trigonometry. Should there be no practical difficulties in the way, it might probably replace with advantage the stadiometer, which depends on the principle of similar triangles, supplied by the War-office to volunteer corps for use in judging distance drill.—*Reader, May* 13.

6. *On a Journey from Chimborazo to Bogota across the Central Andes;* by Mr. Robert Cross, (Proc. Royal Geogr. Soc., April 24.)—This was a narrative of a journey performed by the writer whilst employed by the India-office in collecting seeds of the Pitayo cinchona-tree for planting

in India. This valuable species of cinchona appeared to be now reduced to a few specimens growing near Popayan, from which the writer succeeded in obtaining a good supply of seeds. His journey from the Pitayo forests to the valley of the Magdelena was over the high and bleak plateau of Guanacas, where his mules had a narrow escape from perishing of cold, and where he saw the road strewed with skeletons of men and animals.

General Mosquera, Minister of the United States of Columbia to Great Britain, at the invitation of the President, addressed a few remarks to the meeting in English, in which he described the efforts which the government of his native country were making to open a road across the Cordillera. . . . This was one out of many enterprises which were now being pushed forward with a view to increase the commerce between New Granada and Great Britain in the abundant produce of that part of Tropical America.

Mr. Evan Hopkins, C.E., who had been employed by General Mosquera to survey various parts of New Granada, confirmed the statements of Mr. Oliphant with regard to the geography of the Isthmus. As to the vast region which Mr. Cross had given them an account of, the difficulties of traveling described by him were due to the want of proper precautions. He had himself crossed all the ranges of the Cordillera five times, without suffering any great privation, and a more magnificent region for beauty and fertility of soil he had never seen.

Mr. Gerstenberg reviewed the capabilities of the various routes which had been proposed for a ship canal, and gave his reasons for preferring that which had been first suggested by Humboldt, namely, between the Gulf of St. Miguel and Caledonia Bay, which was the only line that presented the indispensable requirement of good harbors at each end.

Mr. Oliphant differed from Mr. Gerstenberg in supposing that a good harbor at each end was of more importance than a depression in the intervening ridge. Mr. Crawford and Dr. Hodgkin also took part in the discussion, and the President, in summing up, said the question of crossing the Isthmus of Panama had been productive, in years past, of frequent and lively discussions in the Society; but he agrees with Mr. Oliphant, that it would be a disgrace to British geographers if they did not complete the examination of this region.—*Reader*, *May* 6.

7. *Walker Prizes*, *Boston Society of Natural History*.—The following prizes were founded by the late Dr. William J. Walker, for the best memoirs, in the English language, on subjects proposed by a committee appointed by the Council of the Boston Society of Natural History. The first and second are to be awarded annually; the third, once in five years, beginning with 1870.

First—For the best memoir presented, a prize of sixty dollars may be awarded. If, however, the memoir be one of marked merit, the amount awarded may be increased to one hundred dollars, at the discretion of the committee.

Second—For the next best memoir, a prize not exceeding fifty dollars may be awarded at the discretion of the committee.

Neither of the above prizes shall be awarded unless the memoirs presented shall be deemed of adequate merit.

Third—Grand Honorary Prize. The Council of the Society may award the sum of five hundred dollars for such scientific investigation or discovery in natural history as they may think deserving thereof; provided such investigation or discovery shall have first been made known and published in the United States of America; and shall have been at the time of said award made known and published at least one year. If in consequence of the extraordinary merit of any such investigation or discovery, the Council of the Society should see fit, they may award therefor the sum of one thousand dollars.

The following subjects for prizes under the Walker fund have been announced by the Society.

Subject of the Annual Prize for 1865–6. "Adduce and discuss the evidences of the co-existence of man and extinct animals, with the view of determining the limits of his antiquity."

Subject for 1866–7. "The fertilization of plants by the agency of insects, in reference both to cases where this agency is absolutely necessary, and where it is only accessory;" the investigations to be in preference directed to indigenous plants.

Memoirs offered in competition for the above prizes must be forwarded on or before April first, prepaid and addressed "*Boston Society of Natural History, for the Committee on the Walker Prizes, Boston, Mass.*"

Each memoir must be accompanied by a sealed envelope enclosing the author's name, and superscribed by a motto corresponding to one borne by the manuscript.

Boston, May, 1865.

8. *Tunnel of Mount Cenis.*—Of the total length of the Mt. Cenis tunnel, 12,220 meters, 7,977 remain to be made. Having been begun in 1858, and with new methods and energy in 1863, 4423·4 meters were finished on the first of April, 1865; of which, 1646 meters were accomplished by the old methods of tunnelling and 2777·4 by the new mechanical methods since the commencement of 1863—802 meters in 1863, 1088 in 1864, and 337·4 in the first three months of 1865. The rate of progress in 1862 was 2·02 meters per day, in 1864, 2·92 meters, and thus far in 1865, 3·75. At the last rate it will take five years and eight months to complete the tunnel.—*Les Mondes*, May 18.

9. *Centennial Celebration of the Royal Saxon Mining Academy at Freiberg.*—Baron von Beust and Professors Reich, Breithaupt, Cotta, Scheerer, and others have issued an announcement in the *Berg- und Hüttenmännisches Zeitung*, that the Centennial celebration of the Freiberg Mining Academy will take place on the 30th of July, 1866. It is expected that many of the former students of the Academy will be present at this festival; and it is especially requested that every one interested should do as much as possible to acquaint others with the appointed day, and, further, every one is solicited to bring his photograph, so that it may be placed in a book to be kept in the Mining-Academy as a souvenir of the occasion.

10. *The Agassiz Expedition to South America.*—The expedition of Prof. Agassiz arrived safely at Rio de Janeiro on the 22d of April. The Emperor sent a boat along side of the Colorado to take the party on shore, and in the evening had a long interview with Mr. Agassiz. The

Secretary of the Treasury of Brazil gave orders to have the baggage and instruments of the party passed unopened at the Custom House, and every courtesy was extended to the members of the expedition by the officials of the Brazilian government.—*Boston Transcript*, June 9.

11. *Report on the mortality in Paris;* by M. Deville.—As far as we can judge from historical documents, the annual mortality in Paris at the commencement of the last century was 1 in 28; 50 years later, 1 in 30; in 1836, 1 in 36. The year 1840 was exceptional, and the ratio was 1 in 33; in 1841 it was again 1 in 36. In 1846, 5 years later, the ratio was 1 in 37; in 1851, 1 in 38; in 1856, 1 in 39. These numbers apply to old Paris.

In 1860, the time of the annexation, the population was increased by the accession of an area less favorable for health than the interior of Paris. Still, the proportion of deaths in 1861, out of 1,696,141 inhabitants, was 1 in 39. In 1862 and 1863 the diminution in the ratio of deaths continued, it amounting in both years to 1 in 40, the number of deaths being, in 1862, 42,113.

The Commission attribute the improvement of the public health to the great works carried forward in the capital—that is, the opening of avenues, and the improvements in the supply of water, in drainage, in the supervision of the "Commission des logements insalubres," the organization of hospitals; and also to the general prosperity of the working classes, who take better care of themselves, dress more warmly, and eat more wholesome and abundant food.—*Les Mondes, Feb.* 23.

12. *Dr. H. Falconer.*—It has been determined to place a marble bust of Dr. Falconer in one of the London scientific Societies, and to establish a Falconer Scholarship, or Fellowship, in Natural Science in the University of Edinburgh. Upwards of £1000 have already been subscribed.—*Mackie's Repert., May 1st.*

13. *Ink.*—The Paris Société d'Encouragement pour l'Industrie Nationale has offered a prize of three thousand dollars for an ink that will not corrode steel pens.—*Athen.*

14. *Production of the Sexes.*—The views of Mr. Thury, published in volume xxxix of this Journal (p. 84) have recently been controverted by Mr. Coste, in a memoir read on May 8th before the Academy of Sciences at Paris. The author bases his conclusions on an extensive series of experiments and observations.

15. *British Association.*—The thirty-fifth meeting of the British Association will be opened on Wednesday, the 6th of September, 1865, at Birmingham. Prof. John Phillips is President for the year, William Hopkins and Francis Galton, Vice Presidents, George Griffith, General Secretary.

16. *Academy of Sciences, Paris.*—In May last, Otto Struve was elected a corresponding member of the Astronomical Section of the Academy, in the place of Carlini, and Plantamour of Geneva in the place of William Struve; and in the Botanical section, Alexander Braun was elected a corresponding member.

17. *Voyage of the Novara.*—The second part of volume II of the Zoology of the Voyage of the Novara, has been published by the Vienna Academy of Sciences. It treats of the Lepidoptera, and is the work of Messrs. Cajetan, and R. Felder.

OBITUARY.

Valenciennes.—Mr. Valenciennes, the distinguished zoologist, died at Paris on the 12th of April last. He was born in that city on the 9th of April, 1794. Mr. Decaisne announced his death to the Academy of Science in the following words: Mr. Valenciennes was the friend and fellow-worker of the most illustrious naturalist of the age, George Cuvier. He was for half a century the friend and confidant of Alexander von Humboldt. Such friendships will ever honor the memory of our regretted confrère.—*Les Mondes, May* 4.

Pierre Gratiolet, the eloquent professor of zoology of the Faculty of Sciences, Paris, distinguished also as a spiritualist writer, died suddenly on the 21st of February last.

Leon Dufour, one of the most prominent of French zoologists, and especially eminent in entomology, long an associate of Cuvier, Latreille, etc., died on the 18th of April, in his eighty-sixth year.

Admiral Fitzroy, the able and learned commander of the Exploring Expedition of the Beagle, (that of which Darwin was the naturalist,) hydrographical surveyor in various regions, and author of extended meteorological researches, died early in May last, in his 60th year, having been born on the 5th of July, 1805.

A. Gressly, a prominent Swiss geologist, died recently at Soleure, in Switzerland.

Sir J. Richardson, the distinguished naturalist and Arctic voyager, died on the 5th of June last.

Sir Joseph Paxton, the architect of the Crystal Palace of 1851, editor of the "Magazine of Botany," between 1840 and 1852, and author of a Botanical Dictionary and other works, died on the 8th of June, at his residence, Rock Hill, Sydenham, in his sixty-first year.

Kupffer, the meteorologist, and Director of the Central Observatory of Russia, at St. Petersburg, has recently died.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *A Treatise on Astronomy;* by Elias Loomis, LL.D., Professor of Nat. Phil. and Astron. in Yale College. Harper & Brothers, New York, 1865. 8vo, pp. 338.—This treatise is especially designed as a college text-book. This primary object has directed the selection of the topics, and the extent of the development of each. The articles and sections are also arranged to meet the wants of the class room. The mathematical discussions imply a knowledge of the elements of mathematics including Spherical Trigonometry and Conic Sections. The subject is rendered more attractive to students by allotting more than usual space to various physical phenomena, as, the constitution of the sun, the tides, the constitution of the comets, &c. At the end of the volume are fifteen tables, and eight plates. The tables give the elements of the planets, asteroids, satellites, periodical comets and binary stars, the heights and diameters of the lunar mountains, the parallaxes of fixed stars, &c.

Prof. Loomis's Treatise on Practical Astronomy has been deservedly esteemed, and his experience of the peculiar wants of college classes affords assurance that this treatise will be equally worthy of favor.

2. *Entomological Society of Philadelphia.*—The Entomological Society of Philadelphia during the three years past has issued three volumes, containing in all 1650 pages and 20 plates, many of the plates beautifully colored. Since the commencement of the present year two thick numbers of Proceedings have already appeared: No. 1, for January and February, containing 212 pages and one plate, No. 2, for March and April, containing 120 pages and one plate. These nearly complete the 4th volume of the publications of this Society.

These numbers contain the following memoirs:

Page 1, Hymenoptera of Cuba; *E. T. Cresson.*—p. 201, On certain Diurnal Lepidoptera of N. America; *Wm. H. Edwards*, (with a plate).—p. 204, Variation of sexes in Argynnis Diana; *H. W. Bates.*—p. 207, On Synonomy of Parathyris Angelica; *A. R. Grote.*—p. 208, On new species of Chrysopa; *H. Shimer.*—Page 213, Description of three new Canadian Nocturnal Lepidoptera; *C. J. S. Bethune.*—p. 216, On some American Pierinæ; *T. Reakirt.*—p. 222, On new species of Citheronia, and on Arisota rubicunda; *A. R. Grote* and *C. S. Robinson.*—p. 224, New North American Limnobina; *R. Osten Sacken.*—p. 242, Catalogue of Hymenoptera from Colorado Territory, in the Society's collection; *E. T. Cresson.*—p. 313, New species of Cuban Lepidoptera; *C. A. Blake.*—p. 315, Descriptions of North American Lepidoptera, No. 6; *A. R. Grote.*

Along with the last number of the Proceedings we have received a copy of a Circular, signed by the Publication Committee, E. T. Cresson, J. Cassin and A. R. Grote, annnouncing that the Society had been greatly indebted for its means of publishing to the generosity of Dr. Thomas B. Wilson, who had given $5,000 as a fund for this purpose, besides additional aid at various times, and $5,000, also, for the general maintenance of the Society; and that, on account of Dr. Wilson's sudden death without having carried out his expressed intention to place the Society upon a permanent foundation with a large publication fund, it was now greatly crippled in its means. The circular states, that, "To insure the permanence of the Society, and the continuance of its publications, an income of $3,000 is judged to be required, representing a funded capital of $50,000," and that $40,000 are therefore needed and asked for from the friends of science in the land. No scientific Society or Academy in the United States has been doing, of late, more, or better work for science than the Entomological Society; and the failure of its means, which has come upon it so suddenly, is a calamity to the country as well as to the general interests of science. This call merits a ready response. The Society, as it states, cannot offer any equivalent advantages for donations to the fund. They propose, however, to make those who come to their aid at this juncture, and give at least $100, Honorary Members; while those who give $500 or more will be entitled also to the Publications of the Society during life. The public may aid the Society by subscriptions to its publications, which are furnished at the small subscription price of three dollars a volume.

3. *Whitney's Report on the Geological Survey of California.*—The publication of the text of the first volume of this Report, containing the Paleontology by F. B. Meek and W. M. Gabb, was noticed in the last volume of this Journal (p. 99). The engraving of the plates has since been completed, and they are now ready to be issued. They are thirty-two in number, in large 8vo, and are crowded with figures of California

fossils, from the Carboniferous, Triassic, Jurassic, Cretaceous, and Tertiary formations. They are engraved from drawings by Mr. Meek and Mr. Gabb—which is a sufficient pledge of accuracy. These handsome plates annexed to the text make one of the most elegant, as well as most valuable, volumes that has yet been published on any department of American geology. The plates of the Jurassic include figures of the species that were obtained from the metamorphic gold rocks of the Sierra Nevada.

4. *A Preliminary Report on the Geology of New Brunswick, together with a special Report on the "Quebec Group" in the Province;* by HENRY YOULE HIND, M.A., F.R.G.S., &c. 294 pp. 8vo. Fredericton, New Brunswick, 1865.—This Report is made partly from the author's observations, and largely from other sources. It takes up in succession the geographical features of the Province; the general geological structure; the Granitic series of rocks; the Carboniferous series; the Devonian; the Upper and Middle Silurian; the Quebec Group; and the Drift, Terraces, Beaches and Valley-erosion. The Albertite question is discussed, and with the conclusion that it is an "inspissated or altered petroleum" injected from below (from the Devonian) into fissures situated along anticlinal axes.

5. *Comparative Geography,* by CARL RITTER, late Professor of Geography in the University of Berlin. Translated for the use of Schools and Colleges, by WM. L. GAGE. 220 pp. 12mo. Philadelphia, 1865. J. B. Lippincott & Co.—This work is a translation of one of the series of lectures by the great geographer Ritter, whose writings it is almost superfluous to commend. It is a condensed and philosophical review of the geographical features of our globe, presented without encumbering details, in a simple yet animated style; and is full of instruction both for the student and the general reader.

6. *Annual Report of the American Institute of the City of New York,* for the years 1863, '64. Albany, 1864.—Discussions, opinions and information on a great variety of topics, of practical interest, connected with agriculture and the useful arts, are presented in this volume, together with the address of Gen. Wm. H. Anthon and the anniversary address of C. P. Daly, LL.D.

7. *Baird's Review of American Birds.*—Sheets 10 to 20, covering pages 145 to 320 inclusive, of Baird's "Review of American Birds," have been issued. They treat of the *Motacillidæ*, *Sylvicolidæ*, (the latter under the four subfamilies *Sylvicolinæ*, *Geothlypinæ*, *Icterianæ* and *Setophaginæ*), and the *Hirundinidæ*.

8. *Naturalist's Directory;* by F. W. PUTNAM. Published by the Essex Institute.—Part I. of the Naturalist's Directory, noticed in our last number, has been issued. It extends to 32 pages, 12mo, and includes the names of the naturalists of North America and the West Indies.

9. *Chambers's Encyclopedia.*—The seventh volume of this excellent Encyclopedia has been issued, carrying the work nearly through with the letter P.

10. *Report of the Commissioner of Agriculture for the year* 1862. 632 pp. 8vo, with many plates.—Among the many interesting papers in this volume, there is one on Petroleum, or Coal Oil, by J. P. Lesley; others on the Wheat Plant, Cotton, Tobacco culture, Imphee and Sorghum culture, Sheep husbandry and breeding, Marbles of Rutland, Vt., etc.

11. *Report on the Formation of the Canterbury Plains* (*New Zealand*,) with *a Geological Sketch-map and five Geological sections*, by Julius Haast, Ph.S., F.G.S., etc., Provincial Geologist. 64 pp. small fol., with a colored map and sections.—Canterbury Province adjoins Banks's Peninsula, on the west side of the middle island of New Zealand. The memoir contains a description of the old glacial deposits, moraines, lacustrine beds, raised beaches, and alluvium of the Province.

12. *American Journal of Conchology.*—Part 3 of the first volume of this Quarterly, the number for July, contains papers by Messrs. Bland, Anthony, Gabb, Conrad, Tryon, Newcomb and Whitfield. The Paleontological are three on Eocene shells, two of them by T. A. Conrad, and one by R. P. Whitfield. The number is illustrated by seven beautiful plates, three of which are colored.

Chemical Technology, or Chemistry in its Applications to the Arts and Manufactures; by Thomas Richardson, M.A., Ph.D., etc.

The Ibis, a Quarterly Journal of Ornithology, edited by Alfred Newton, M.A., F.L.S., F.Z.S. New Series, 1865.

The Fibre Plants of India, Africa, and our Colonies: A Treatise on Rheea, Plantain. Pine Apple, Jute, African and China Grass, and New Zealand Flax (Phormium tenax). 1865. London: Macintosh.

Animaux Fossiles et Geologie de l'Attique. The 11th part of this work, by Albert Gaudry, has appeared.

Atlas Céleste, contenant plus de 100,000 étoiles et nébuleuses, par Ch. Dien.—This very complete Atlas consists of 26 maps. The projection employed corresponds to the development of a sphere of 65 centimeters in diameter.

On the Oil-property of the Philadelphia and California Petroleum Company, situated in Santa Barbara and Los Angeles Counties, California; by B. Silliman. 36 pp. 8vo. 1865.—The analyses at page 341 of the preceding volume of this Journal, are contained in this Report.

The Geological and Natural History Repertory; an illustrated popular weekly magazine of Geology, Paleontology, Mineralogy, Natural History, Terrestrial and Cosmical Physics and Journal of Pre-historic Archeology and Ethnology; edited by S. J. Mackie, F.G.S., etc.—First number, 28 pp. 8vo, of this weekly Journal was issued on May 1st. Price 2*d.* Kent & Co.; Trübner & Co.; Edw. Stanford.

Note sur les Tremblements de Terre en 1861, avec Suppléments pour les années antérieurs, par M. Alexis Perrey, 112 pp. 8vo; also the same for 1862, 180 pp. 8vo; from the Memoirs of the Dijon Academy for 1863, and 1864. These Reports post up every thing published relating to earthquakes during the years mentioned.

Abbildungen von fossilen Pflanzen aus dem Keuper frauhens, von Dr. J. L. von Schoenlein, mit erläuterndem Texte von Dr. August Schenck, Prof. zu Würzburg: mit xiii Tafeln Abbildungen. Preis 6 Thlr. 20 Ngr. Subscriptions to this work are solicited by the publisher, C. W. Kreidel, Weisbaden.

Transactions of the Amer. Phil. Soc., Philadelphia, Vol. XIII, New Series, Part I.—p. 1, On California Mosses; *L. Lesquereux.*—p. 25, On the mathematical probability of accidental linguistic resemblances; *P. E. Chase.*—p. 35, Comparative etymology of the Yoruba language; *P. E. Chase.*—p. 57, Thoughts on the influence of ether in the Solar System; *A. Wilcocks.*—p. 105, New Mosses (A List of the Mosses in the United States, east of the Mississippi, with descriptions of two new species); *T. P. James.*—p. 117, On the numerical relations of gravity and magnetism; *P. E. Chase.*

Proceedings Acad. Nat. Sci. Philadelphia. No. 5. Nov. and Dec., 1864.—p. 258, Synopsis of the E. American Sharks; *T. Gill.*—p. 265, Notes on the Cœrebinæ (Birds); *J. Cassin.*—p. 276, Partial Catalogue of the cold-blooded Vertebrates of Michigan; *E. D. Cope.*—p. 285, Six new W. Asiatic Unionidæ; *I. Lea.*—p. 286, Birds from S. America; *J. Cassin.*—Reports of Secretary, Librarian, Curators, &c. —According to these reports the Library has received 1201 works; the Museum—besides specimens in all departments of Zoology—the herbarium of Prof. Short of Louisville, Ky., containing 6000 species of American plants, and between 3000 and

4000 European species, 12 thick quarto volumes of Algæ from the late Samuel Ashmead, and 700 species of marine and freshwater Algæ, which had been contributed by Leo Lesquereux to the Central Sanitary Fair. Robert Bridges was elected in December, President for the ensuing year, Wm. S. Vaux and J. Cassin Vice Presidents, T. Stewardson, M.D., Corresponding Secretary, B. H. Rand, M.D., Recording Secretary, I. D. Sergeant, Librarian.—1865. No. 1, January, February and March.—Page 2, Notes on some new and rapacious birds; *J. Cassin.*—p. 5, On a new Cormorant from the Farallone Is., California; *J. G. Cooper.*—p. 7, On extreme and exceptional variations of Diatoms, in some White Mountain localities, etc.; *F. W. Lewis.*—p. 19, Synonymy of the species of Streptomatidæ, a family of fluviatile Mollusca, inhabiting North America, part 4; *G. W. Tryon, Jr.*—p. 37, Descriptions of new species of Birds of the Families Paridæ, Vireonidæ, Tyrannidæ and Trochilidæ, with a note on Myiarchus Panamensis; *G. N. Lawrence.*—p. 40, Resolutions on the death of Dr. T. B. Wilson.—p. 41, Notice of some New Types of Organic Remains, from the Coal measures of Illinois; *F. B. Meek* and *A. H. Worthen.*

Proceedings of the Amer. Acad. of Arts and Sci., vol. vi.—P. 347, Synopsis of North American Gaprineæ; *J. T. Rothrock.*—p. 354, Tides in Lake Huron; *J. D. Graham.*—p. 356, On Right Ascension of the Pole Star; *J. Winlock, U.S.N.*—p. 360, On Jewish Calendars; *C. Pickering.*—p. 361, Researches on Volatile Hydrocarbons; *C. M. Warren.*—p. 363, Revision of the Doctrine of Conditional Sentences in Greek and Latin; *Goodwin.*

Proceedings of the Boston Soc. of Nat. Hist., vol. ix.—Page 319, Diatoms from Randolph, Mass.; *C. Stodder.*—p. 321, On Selandria Cerasi; *A. Winchell.*—p. 323, Fertilization of Cypripedium spectabile and Platanthera psychodes; *S. J. Smith.*—p. 329, On two Albino girls; *B. J. Jeffries.*—p. 332, On Mode of birth in the Opossum; *J. G. Shute.*—p. 333, Arctic plants on Mt. Monadnock, *C. Pickering;* On Amphioxus; *A. S. Bickmore.*—p. 334, Development of Skates; *J. Wyman.*—p. 335, Notice of Record Book of the Linnean Society; *A. A. Gould.*—p. 342, On Tubularia; *H. J. Clark,* (see this Journal [2], xxxvii, 61).—p. 342, A supposed new species of Samia; *S. I. Smith.*—p. 345, Two Ichneumons parasitic on Samia Columbia; *A. S. Packard, Jr.*—p. 346, On Amphioxus, *Chas. Pickering;* Action of sun-light on window-glass; *Thos. Gaffield.*—p. 350, protection against canker-worm; *Ordway.*—p. 352, Skeleton of a Hottentot; *J. Wyman.*—p. 307, Ancient pottery from India; *D. C. Scudder* and *W. B. Capron.*—p. 368, New variety of Parus; *H. Bryant, M.D.*—p. 369, Galeoscoptes Cabanis; *H. Bryant.*

Proceed. California Acad. Nat. Sci., Vol. III, Part 2, 1864.—p. 101, Descriptions of New California Plants—No. I; *A. Gray.*—p. 103, New species of Fishes, Sandwich Island—No. II; *A. Garrett* (of Honolulu).—p. 108, New California Fishes—No. III, with cuts; *J. G. Cooper.*—p. 115, Descriptions of nine species of Helix of California; *W. Newcomb.*—p. 120, New Virgularia of California; *W. M. Gabb.*—p. 120, Notice of plants found growing in hot springs in California; *W. H. Brewer.*—p. 121, New species of Pedicularia; *W. Newcomb.*—p. 123, On California Lepidoptera—No. IV; *H. Behr.*—p. 127, Earthquakes in California during 1863; *J. B. Trask.*—p. 130, Earthquakes from 1800 to 1864; *J. B. Trask.*—p. 153, Cretaceous fossils from Sonora; *W. M. Gabb.*—p. 155, New shells from the coast of California—Part I; *P. P. Carpenter.*—p. 160, California Satyrides; *H. Behr.*—p. 166, Remains of Horse and Elephant in a deposit near San Francisco; *W. P. Blake.*—p. 167, Ammonites or Ceratites from Oregon Bar, Middle fork of the American river; *W. P. Blake.*—Brushite, a new mineral; *G. E. Moore.*—p. 170, 172, Fossils from the auriferous slate of the Mariposa estate; *W. P. Blake,* and *W. M. Gabb.*—p. 173, Fossils from the Quicksilver region, San Luis Obispo; *W. M. Gabb.*—p. 174, Crystallization of Brushite; *J. D. Dana.*—p. 175, New California marine shells; *P. P. Carpenter* (of Warrington, England).

Proceedings of the Essex Institute.—Page 145, Classification of Polyps; *A. E. Verrill.*—p. 153, Habits and distribution of the Duck Hawk in the breeding seasons and description of the eggs; *J. A. Allen.*

Verhandlungen der Kaiserlichen Leop.-Car. deutschen Akademie der Naturforscher (or Acad. Cæs. Leop.-Car. Germ. Naturæ Curiosorum). Vol. xxxi. Dresden, 1864.—Contains a paper *on the Meteorology and Climatology of Central America* by Moritz Wagner (31 pp., 4to); also an elaborate memoir by Dr. A. Baur *on the Synapta digitata* (120 pp., 4to) illustrated by several plates; besides other papers.

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[SECOND SERIES.]

ART. XV.—*Friedrich Georg Wilhelm Struve.*[*]

FRIEDRICH GEORG WILHELM STRUVE was born at Altona, in the Duchy of Holstein, April 15, 1793; his father, Jacob Struve, for forty years filled with great distinction the directorship of the High School in that city, and was widely known for his classical and mathematical acquirements. His mother was the daughter of Pastor Stinde, who went to Russia as chaplain to Peter III; it was this circumstance which subsequently led to the settlement of many of the Struve family in Russia; until then the Struves had from time immemorial lived as respectable yeomen in the Duchy of Holstein. Thus our old and honored associate was descended from the original stock of the Anglo-Saxon race; a descent which many of us remember distinctly expressed itself both in the personal appearance and in the moral qualities of the man. Struve was happily trained by his father into no precocity in his early childhood, but in due time, when removed to the High School, he made such advancement in his studies as at the age of fifteen to be qualified for entrance at the University. In those days of trouble the young student had but little prospect of continuing his studies undisturbed. A constant sense of the oppression of the foreigner and the dread of the French conscription decided his parents, in 1808, to send young Wilhelm to Russia, at that time probably the quietest country in the world, and where his elder brother Carl occupied the post of Classical Lecturer in the University of Dorpat. This university, so soon to be illuminated by the ex-

[*] From Monthly Notices of the Royal Astronomical Society, Feb. 10, 1865.

ample and fame of Wilhelm Struve, had been recently founded by Alexander I, immediately after his accession to the throne, and was intended by him not only to be the intellectual center of the German Provinces of Russia, but to serve as a civilizing link between Europe and Russia proper. Here Wilhelm Struve, in accordance with his father's wishes, applied himself exclusively to classical subjects, and it was in this branch of learning that the young student earned his first literary laurels, by an academical exercise "De systemate metrico apud Alexandrinos;" this essay was honored by a prize, and by being printed at the University expense. It was by this wise and fortunate arrangement of his studies that Wilhelm Struve acquired the power of fluently and accurately expressing himself in the Latin language, thereby enabling him, at a subsequent period, to communicate the results of his researches to men of science living in other parts of Europe, where Struve's native tongue, the German, was at that time but little understood. These early literary pursuits also contributed, in no slight degree, to secure that balance and breadth of mind for which our lamented associate was afterward so remarkable.

In 1811 Struve took his first university degree in Philology, and it was only after having thus fulfilled his father's desire that he passed to that branch of science which henceforth became the principal object of his life. No doubt, while he was a student at Dorpat, the able scientific lectures of the elder Parrot excited a warm interest in his mind; but it was rather an inner call than any external circumstance which led Struve at length to devote himself to astronomy. Parrot's influence was not confined to the mental stimulus thus afforded to his young philosophic pupil, but by the representations which he made to the University authorities relative to young Struve's promise of future eminence, and his present straightened pecuniary resources, he obtained for him the means of still longer prosecuting his studies at Dorpat. It was partly in this way that Struve from the early age of fifteen ceased to be a burden upon the scanty fortune of his father; but the boy's own exertions and self-denial contributed greatly to the same end. It was brought about in this way. Shortly after Wilhelm Struve had entered the University, he applied for and obtained the post of private tutor in the family of Mr. de Berg, a wealthy nobleman of Livland, and his first pupil there was the Count de Berg, the present Viceroy of Poland. By this arrangement the young student was necessarily debarred from a regular attendance on the lectures of the place; but this untoward necessity served, as was natural with one of Struve's mind, only still further to quicken his zeal to make the best of such advantages as remained to him, and to animate his self-reliance.

In 1811, while thus engaged, partly in attendance on the family of the De Bergs, and partly in the prosecution of his own studies at Dorpat, Struve passed on to the class of Astronomy. The Professor at that day was Huth, a man for whose worth and eminent attainments our old associate ever retained an affectionate and respectful memory. Huth's health was too infirm to permit him to assist his pupil to any great extent, and hence young Struve was, by a happy fatality, or in truer words by the discipline of Providence, once more thrown upon self-reliance and the resources of his own efforts. The Professor himself was scarcely ever able to visit the Observatory, but he permitted his pupil to make what use of it he could. This Observatory was at that time but scantily supplied with instruments, and even those for the most part were not in a condition for actual use. Among these instruments was a Transit by our countryman Dolland, and the excellence of the object-glass attracted the special notice of the embryo astronomer. The pillars of this instrument were, it is true, erected, but upon them there was no provision existing for the attachment of the Y's and the other subsidiary apparatus, while the body of the instrument itself had never been removed from the case in which it had been packed. For the mounting of this instrument young Struve could obtain no adequate assistance in the city of Dorpat, but was thrown entirely on the workmanship of his own hands, and the guidance of his own inexperienced judgment. Nevertheless, the work was done, and well done; and in after days Struve would often recur to the fortunate difficulties under which his earliest efforts were made. We must not forget that Struve at this time was only in his nineteenth year.

Early in the autumn of 1813 Struve took his degree of Doctor of Philosophy, and on that occasion wrote his first memoir on an astronomical subject, namely, *De geographica Speculæ Dorpatensis Positione.* In this paper, among other matters, he gave the first determination of the longitude of that Observatory, deduced from occultations observed by himself during the preceding year. In November, 1813, a few weeks after this thesis, he was appointed extraordinary Professor of Mathematics and Astronomy, and two years later, upon the death of Huth, he was advanced to the ordinary professorship. In this capacity his duty was not only to attend to the Observatory, but to lecture also on Astronomy and the higher branches of Mathematics; each subject no doubt suffering from this combination of duties, notwithstanding the efforts of the Professor to prevent it. In 1822, however, the two offices were happily separated, and Struve was henceforth enabled to devote his zeal and his abilities exclusively to the proper work of an Observatory.

Having thus arrived at that point in our venerated associate's career when he was appointed the chief of an Observatory, soon to be rendered by his labors famous to all time, we may for a few moments not improperly revert to the circumstances which years before, indicated the bent of his mind, and, in a certain degree, shaped the after-course of his life. In August, 1811, young Wilhelm Struve, while yet a student, and with extremely slender means at his disposal, verified the orbital motion of the two components of *Castor*, predicted by the elder Herschel in his immortal memoir of 1803, and which by this time was completed to the extent of thirty degrees. This first success had a decided influence in directing Struve's mind to the abundant harvest which he foresaw might be reaped from a zealous devotion to Sidereal Astronomy. Nearly at the same time, while spending the summer at Sagnitz, in the house of his friend, Mr. de Berg, Struve's attention was drawn to Geodesy; and the early success which here again attended his first attempts, laid the foundation for that other gigantic work upon which, together with his labors in sidereal astronomy, rest the fame and the scientific services of this great observer. Although provided with the feeblest conceivable instrumental means, he made excursions in the neighborhood of his friend's house, in order to see if it were possible to make a triangulation of that part of Livland. While practising himself in the use of the sextant, for that purpose, a singular accident befell him, and for some time put a stop to his geodetical studies. The French army at this time was invading the Russian frontier, and against it a Russian corps had been despatched to Livland in order to prevent the French army from penetrating through the Baltic provinces to St. Petersburg. Some officers of this corps seeing young Struve engaged with an instrument and a note-book, mistook him for a French spy, and, notwithstanding his protestations, marched him off, for full a hundred miles, as a prisoner to head quarters, and there, not without some difficulty, he was liberated by the commander-in-chief, on the express understanding that as long as the war continued he would not again, by similar pursuits, expose himself to suspicion. Three years after this occurrence, Struve, at the invitation of the Economical Society of Livland, undertook to make a triangulation and map of the entire province; and this work he accomplished with great ability, although he had no instrumental means at his disposal beyond an excellent 10-inch sextant, by Troughton. This work, thus commenced with a comparatively insufficient instrument, forms the starting-point and the nucleus of that noble survey of the Russian empire, which for nearly half a century occupied the thoughts and the abilities of our lamented associate.

From 1813 to 1839 Struve continued at his post at the Observatory of Dorpat. As we have already seen, the means at his disposal were wholly inadequate to the most modest requirements of an astronomer. It was not long, however, before the success of his labors attracted the attention of the Russian government, and through the benevolent intercession of Prince Lieven, then Chancellor of the University, and as an acknowledgment of the services of the professor, the Observatory was furnished with such instruments and pecuniary means as soon raised it to the rank of a first-rate establishment. Thus, in 1821, the Meridian Circle was obtained from Reichenbach and Ertel, and in 1824, Fraunhofer's famous 9-inch Refractor was added, at once the masterpiece of that great artist, and the commencement of a new era in the history and employment of the telescope.

Struve at first worked alone in the Observatory thus munificently furnished; occasionally, indeed, he was assisted by a few volunteers from among his pupils, and notably so by *Knorre*, who subsequently and for forty years, was the well-known director of the observatory of Nicolaiff; but it was not until the year 1826 that he obtained, in the person of M. Preuss, a permanent observer, who by applying himself with much ability to the meridian circle, enabled Struve to devote his attention to the observation of double stars with the great refractor.

In taking even a cursory survey of Struve's labors at Dorpat with this noble instrument, it becomes necessary to revert, in some degree, to the state of sidereal astronomy before he commenced his work. In 1803, Sir William Herschel had announced the binary nature of several of the double stars; nevertheless, partly from the extreme novelty of the views set forth by that great philosopher, partly from the deficiency of the optical means then existing, and partly from the great difficulty of manipulating the micrometrical measurements, the subject remained in abeyance and almost untouched during the succeeding fifteen or sixteen years. In 1816, Sir John Herschel did, however, commence at Slough a review of his father's double stars, but owing to some of the difficulties already alluded to, he made but inconsiderable progress. Some of these measurements so taken are recorded in the joint communications made by Sir John Herschel and his colleague, Sir James South, to the Royal Society in 1824: among others will be found a remarkable measurement of *Castor*, when the two component stars happened to be on the same parallel. We have already adverted to the then young Struve's measurement of the same remarkable star, and to the effect which it had on the direction of his energies: this, however, was but a single and isolated result, and it was not until some years later, namely, in 1819, that the record of measured angles of position and distance began to be at all consecutive at Dorpat, though differences of right ascension and declination had been pretty copiously observed. In 1820 appeared Struve's first Catalogue of 727 double stars, arranged in the order of their right ascension, together with their corresponding declinations. This Catalogue was expressly intended to facilitate the observation of these objects, either with meridian or with equatorially-mounted instruments, and effectually revived the subject as one of general astronomical interest.

Nevertheless, praiseworthy and valuable as were these new attempts, it was not until the erection of the great Fraunhofer Refractor in 1824, at Dorpat, that Mr. Struve became possessed of an instrument worthy of the subject, and competent, not only to afford facility and precision in respect of measurement, but to add largely to the list of known double stars.

The result of the first two years of his observations with this famous telescope was that most remarkable work, *Catalogus novus generalis Stellarum duplicium et multiplicium*, which appeared in 1827, and will for ever be considered as forming a memorable epoch in Sidereal Astronomy. Nor is this great work remarkable alone for its copious and valuable lists of 3112 double or multiple stars duly arranged in their order of right ascension, and 2343 of which had not been previously described by any astronomer, but still more so for a great physical fact which it announced for the first time. In this catalogue it is shown that the closer classes of double stars, namely, those whose mutual distance is less than 4″ exceed out of all proportion, not only what might be expected from a calculation of chances on the hypothesis of casual or optical juxtaposition, but even in point of numerical majority, those of either of the other classes. The existence of such a fact affords, in the opinion of those competent to form it, a convincing proof of a real physical connexion, and (independently of any question as to orbital motion) places these juxtaposed objects before us, neither as casualties of situation as seen from the earth, nor as mere exceptions to a more general rule of isolation, nor as mere *curiosities* of the sidereal heavens, but as entering largely into the general plan and constitution of the universe. Later observations, carried on both by Mr. Struve himself and by others, with even larger instruments than the great Dorpat refractor, have confirmed this most notable result, and have shown that it is but an ordinary circumstance to find stars previously regarded as single, to be in reality composed of two *very close* individuals. It will enhance our respect for the memory of our lamented associate, and enable us to form some idea of the labor and devotion required for the formation of this catalogue, to remark that in the preface to the Petersburg Catalogue it is stated to have been the result of the examination of 120,000 stars, and that it was one individual who executed this examination with his own eyes and hands.

The formation of this Catalogue was, after all, the commencement only of a work of far greater labor in this department of astronomy. Struve's next undertaking was the determination of the angles of position, distances, magnitudes, and colors of all the objects therein enumerated, together with their exact right ascension and declinations, each resulting from several nights' observation. The results of this immense undertaking he published in the year 1837 in a magnificent work entitled, *Stellarum compositarum mensuræ micrometricæ*, followed, in 1852, by another work, bearing the title, *Stellarum fixarum, imprimis duplicium et multiplicium, Positiones mediæ*, with their proper motions computed when practical from earlier observations.

Another branch of what, with some propriety, may be called the *Physique du Ciel sidéral*, to which Struve directed much of his attention, was the determination of the *law of density* in the distribution of the stars with respect to the plane of the Milky Way. His researches on this subject were not indeed published until after he had removed from Dorpat, as we shall speedily see, but it is here, perhaps, that we can with greatest convenience refer to the fact. The great work which contains the result of his labors, is entitled *Études d'Astronomie Stellaire*, published in the year 1847. By a series of calculations, founded on the number and distribution of stars of sufficient lustre to admit of being individually catalogued, as well as by the star-gauges of Sir W. Herschel subjected to a careful and systematic analysis, he has in this work clearly shown that in the northern hemisphere at least, (northern, that is, when referred to the bisection of the heavens by the Milky Way) the distribution of the stars exhibits a distinct and unequivocal relation to their angular distance from the plane of that sidereal ecliptic. Nor is this view wanting in confirmation. In the same year that Struve published his researches on this great problem, Sir John Herschel gave also the results of his own investigations of the *southern* hemisphere, and the numerical expression obtained by him for the gradation of stellar density is singularly coincident with that resulting from Struve's investigations of the northern heavens. As the two results were obtained from observations made with telescopes strictly comparable, and were each arrived at independently and without the knowledge of the other, it seems that this great physical generalization may be regarded as perfectly established.

Such are some of the great results which form the high rewards of Struve's loyalty to his duties at Dorpat. The year 1833 brought a great change in his life. In the autumn of that year the Emperor Nicholas resolved to erect a great central observatory for the empire of Russia; it is honorable to the

Czar's memory to record that the suggestion was wholly spontaneous, and we feel no surprise that our associate was a most influential member of the commission entrusted with the execution of the Emperor's noble design. The prosecution of this work occupied much of Struve's attention, and necessarily compelled frequent and long visits to the capital, until the spring of 1839, when the building having been completed, Struve took up his permanent residence at Pulkowa as director in chief of the Central Observatory of Russia.

The creation of an Observatory to be, from its commencement, established for well-defined and specific purposes, distinctly set forth in a formal document; and without restriction in cost, to be furnished with instruments constructed by the ablest artists who were at the same time encouraged to new efforts, the observatory and its ample staff being, at the same time, munificently endowed with pecuniary means and all other requisite appliances, is a circumstance unique in the history of science. Nor could this magnificent plan have been successfully executed, had not the man been at hand qualified by nature and experience to organize and direct so difficult an undertaking. It is unnecessary here to describe at any length the Observatory of Pulkowa, but it would be wrong not to refer to the judicious plan of observation which Struve proposed to follow, so as best to realize the generous intentions of its imperial founder. Fully aware of the incomparable value of the Greenwich lunar and planetary observations, and of its traditions in other respects, Struve determined, from the first, to adopt a course wholly different from that followed at our National Observatory. Hence, observations of the moon and planets were to be made on exceptional occasions only, the main strength and efforts of the Observatory being directed to the astronomy and physics of the sidereal heavens. It was proposed, also, not to publish an annual volume of observations, but, after having organized systems of specific research, often of necessity extending through several years, to give the results to the world only when each project was completed, and then with the name of the particular observer attached. How well this plan has been executed the records and scientific memoirs which have proceeded from Pulkowa sufficiently attest. And, lastly, it will not be without its interest to record that, in 1844, when Struve published his elaborate description of Pulkowa, there were no less than 103 persons, including the children, domiciled within the precincts of the Observatory. This numerous family comprised seven astronomers, several *savans* connected with the geodesy of the empire, a secretary, an engineer-in-chief, a cabinet-maker, with ten artisans for the repair of the instruments and the furniture, and eight discharged non-commissioned officers for the service of the place. Assuredly it was no ordinary man who could secure, as Struve secured, order and good-will in so considerable and multifarious an establishment. For the endowment of this noble institution the Emperor of Russia, with truly imperial generosity, assigned no less a sum than ten thousand pounds per annum. The servant was worthy of the master.

Meanwhile, and for nearly half a century, extending almost throughout our venerated associate's directorships both at Dorpat and at Pulkowa, run continuously the gigantic operations connected with the measurement of the Russian and Scandinavian Arc of Meridian, and the Trigonometrical Survey of the enormous Russian empire. The genius of Struve presided over this vast undertaking, ever devising, co-operating, and not seldom working with his own hands and eyes, and on more than one occasion amidst ice and snow. This arc of meridian, extending through 25° 20′, from Ismail at the mouths of the Danube to Fuglenæs on the Arctic Ocean, exceeds in magnitude even our own Indian arc, from Cape Comorin to the Himalayas, the execution of which alone, and with no other work, has been sufficient to raise the fame and exhaust the labors of Lambton and Everest.

Some idea may be formed of the amount and variety of Struve's occupations at this time from the following quotation from his great work on the subject of this very arc. Speaking of that portion which runs through Livland, he says:—"Undoubtedly I could have completed this work long before, had it been the sole object of my scientific duties; but, in 1821, Reichenbach's meridian circle arrived at Dorpat, and at length, in 1824, I was put in possession of Fraunhofer's magnificent equatorial, ordered since 1820, and I thought that my first duties pointed to the immediate use of these exquisite instruments. Hence, I did not consider myself authorized to absent myself from Dorpat for more than a few weeks' work at a time in geodetical proceedings. And I think that I ought here to explain, that between 1821 and 1827 (which period embraces the field-work of the Baltic arc), I made, reduced, and published without any assistance, the meridian observations recorded in volumes III, IV, V, VI, of the Dorpat Annals; at the same time I also finished the revision of the survey of the heavens, which furnished the *Catalogus novus Stellarum duplicium*, published in 1827; and besides this I had to give an annual course of lectures on Astronomy to the University, and of Geodesy to the Imperial staff."

Such labors, great as they were, are not now referred to as being by any means without their parallel among scientific men; on the contrary, they form the rule rather than the exception; but they are mentioned here because they may well serve to remind us, that such is the cost at which our knowledge is advanced, and for which alone the world accords imperishable fame to her sons.

Struve visited England on four different occasions. The first time was in 1830, when it so happened, that a committee was sitting for the improvement of the *Nautical Almanac;* he was invited to assist in its deliberations, and by his ability and excellent temper contributed toward bringing its labors to a successful conclusion. In 1844 he came to England for the purpose of determining the difference of longitude between the Observatories of Pulkowa and Greenwich. A step had already been taken toward the completion of that important but difficult operation, by ascertaining, in 1843, the difference of longitude between Pulkowa and Altona. In those days,—it may seem strange indeed, that we naturally fall into such expressions while speaking of but twenty years ago, but the amazing advances, which, during that short interval, have been made in the application of science to art, justify the terms—in those days, the only known method existing for the exact determination of differences of longitude between two geographical positions, was by the repeated transmission of many chronometers from one place to the other. Hence, it was necessary for Struve to organize, and personally superintend, a series of chronometric expeditions, first between Pulkowa and Altona, and then between Altona and Greenwich. In the former expedition not less than sixty-eight chronometers were sixteen times carried across the Baltic; and in the latter, forty-two chronometers passed sixteen times over the German Ocean and the Thames. In the present day, owing to the combined labors of Oersted and Wheatstone,[1] all this tedious and cumbersome operation is replaced by the mere automatic action of the clock itself, whereby a series of metallic contacts are made, conveying the precise time, to the small fraction of a second, through the electric wire to an observer waiting in quietness for the expected signals at some distant place. All honor be to Struve, and to others like him, who, toiling along rivers and seas, and without our wonderful appliances, by patient genius obtained results comparable in accuracy with our own.[2] Nor may we here omit the mention of an incident which exhibits in a characteristic manner the true greatness of Struve's mind. After he had successfully completed the very troublesome operation before alluded to, and when it had become necessary to arrange and publish the result, for geographical and other purposes, the question arose, shall the astronomical precedence attached to the *first meridian of longitude* be assigned to Pulkowa or to Greenwich? There could be no doubt that Struve's heart was in Russia, rather than in England, and we have already seen how in external magnificence Pulkowa utterly eclipsed the unpretending buildings at Greenwich; but Struve, like a true astronomer, remembered the long line of illustrious men who had toiled at Greenwich, the old traditions of Flamsteed, and Halley, and Bradley, and to Greenwich he assigned the astronomical precedence.

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[1] Some American names are here omitted.—EDS. J. SCI.

[2] In 1844 the Astronomer Royal determined the longitude of Valentia, in the west of Ireland, to be 41m. 9s·67 W., by means of ten transits of thirty pocket chronometers to and fro. In 1862 the longitude of the same place was re-determined by galvanic signals given by the clock itself, and the final result was 41m. 9s·81 W.!—*Mem. R. Ast. Soc.*, vol. xxxii.

Struve was in this country again in 1847. This time, the main object of his visit was to carry back with him to Russia one of the two standard bars used in the great Indian Survey, and without which it was impossible to utilize and connect the measurement of the Indian arc, with that of the still greater Russian and Scandinavian arc just completed. On this occasion, he joined the British Association, which that year fortunately assembled at Oxford, and it was thus that Struve either acquired or consolidated that strong personal regard which united him, in a peculiar degree, with the principal scientific men of England in almost every branch. Perhaps it may be allowable for the writer to record that his first acquaintance with this most amiable man was made at this period when, at the house of Sir John Herschel, and not long after the memorable discovery of *Neptune*, he, for a few days, was permitted to associate with a phalanx of scientific veterans rarely assembled in one spot,—Adams, Airy, Herschel, Leverrier, and Struve, forming the list.

Ten years later, viz. 1857, found Struve in England for the last time. He had never failed in public documents and in private communications to speak in affectionate terms of the ready aid and hospitality which he had received at the hands of the present Astronomer Royal, Mr. Airy. His headquarters were, therefore, again as of old, at Greenwich. But now, though in his sixty-fourth year, the indomitable energy and the genius of work in the man brought him thither on a project of still greater magnitude than the arc of meridian which he had just completed. In early youth, while honorably supporting himself in Livland as tutor in the family of the De Bergs, he had cast a wistful eye over the level tracts which north and south, east and west, are a characteristic feature of European Russia. Our great countryman, Dr. Arnold, used to say, with but slight exaggeration, that there was scarcely a hill between Rugby and the Ural mountains to shelter him from the east wind. Arnold observed this with the eye of a geographer; Struve knew it, and utilized it as an astronomer.[3] No sooner then was the Russian and Scandinavian arc from north to south completed, than Struve set to work to organize and arrange the measurement of an arc of parallel from east to west, of still more gigantic dimensions. He proposed to extend it throughout the entire breath of Europe; from Orsk, at the foot of the Ural Mountains, to Valentia, at the western extremity of Ireland, ranging over perhaps a twelfth part of the equatorial circumference of the earth. The operations for measuring this arc would necessarily lie in many lands; and, perhaps, this man of no jealousies was, if not the only man, at least the one best fitted to negotiate with the various governments, and men in high office, both in state and science, without whose concurrence and aid the execution of the vast project was impossible. The Emperor of the French and other state officials received Struve with especial distinction. It was to arrange for the prosecution of that portion of the arc of parallel which lies in England and Ireland, and also of that part which connects England with Belgium, that Struve now came to Greenwich. In this country he was warmly seconded by our own government, by Mr. Airy, Colonel Sir Henry James, Captain Clarke, and other *savans* connected with the geodesy of Great Britain. It is sometimes accorded by a divine providence, wiser than ourselves, that a man at the age of sixty-four may arrange and even commence so vast a plan, but it is not granted that he may complete it. So Struve labored, but it is for other men to enter into his labors. In the following year, 1858, came the first attack of a cruel malady, which might have warned him that his active labors were drawing to a close. When, however, he could no longer take the field and observe, we are informed on the best authority, that he would sit at his writing-table for fourteen or fifteen hours in the day, endeavoring to further with his pen that great undertaking which he could not assist with his hands.

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[3] Struve remarks that, along the arc between the mouths of the Danube and the Arctic Sea, there is no hill 1200 feet high. Over a great portion of it the country is so level and wooded that it was often necessary to erect lofty scaffolds in order to see the necessary signals.

Before concluding our estimate of the permanent results of Struve's labors on the advancement of astronomy, it is necessary to observe that the data for our more accurate knowledge of the constants of precession, nutation, and aberration, as now generally adopted by astronomers, were furnished by observations made by himself or under his immediate direction, in the interval between 1822 and 1841. Again, in addition to his discovery of the existence of a law in the distribution of stars in space and in the proximities of multiple stars, of which we have already spoken, for Struve may properly be claimed what Sir John Herschel happily terms "the first impression that was made"[4] on the general problem of the distances of the fixed stars from the Sun. It was humiliating to the astronomer (nay, he had almost acquiesced in the belief that for wise purposes it was designedly so) to think that an innumerable multitude of bright objects, which he could not do otherwise than regard as worlds, were within his sight, yet beyond the reach of his geometry to measure. Our associate, however, resolved to determine, if determined it could be, whether this limit had actually been set to the bounds of human knowledge, and in the years 1819–21 he succeeded, by sagacious and diligent observations of 27 circumpolar stars, in showing, beyond a doubt, that certain indications of parallax existed sufficient to encourage further efforts with more adequate instrumental means than he at that time possessed. The history of the definite settlement of the question of stellar parallax is not a little curious, and Struve's part in it is sufficiently conspicuous. Our countryman Henderson, in 1831–2, at the Cape of Good Hope, observed *α Centauri* with the mural circle, and on his return to England shortly afterward deduced a parallax of about 1″ for that remarkable star. Struve at Dorpat, from micrometrical measurement made with Fraunhofer's instrument in the years 1835–8, obtained a parallax for *α Lyræ,* amounting to 0″·261. Nevertheless, astronomers, after their manner of most rigid and impartial scrutiny, decided that possibly the shadow of a doubt rested upon both these sets of observations, and resolved to wait for the results of fresh measurements. Meanwhile, Bessel a year after, and from observations made with the great Heliometer at Königsberg, deduced a parallax of 0″·348 for 61 *Cygni,* such as to be wholly beyond the reach of cavil. Subsequently Peters confirmed Henderson's result of the existence of a parallax in *α Centauri,* and M. O. Struve vindicated the general truth of his father's deductions. Such, then, is the share which must be assigned to our associate in the determination of this most interesting, but difficult cosmical question.

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[4] Address of Sir John Herschel to the Royal Astronomical Society, 1841.

Nor must we pass over our late associate's *Description de l'Observatoire Central de Poulkova* given to the world in 1847. It would be but an obvious and inadequate remark simply to say that this is the most sumptuous and complete work ever published on the description of an observatory, its instruments and its arrangements; and a monument to the enlightened generosity of the Russian government, who defrayed the cost of its publication. A perusal of it can scarcely fail to kindle the admiration of every one who is endued with a taste for practical astronomy. What is of far more importance, almost every page indicates the perfect mastery of the author over the instruments which he thus admirably describes, and attests the scrupulous care with which he attended to the minutest circumstances which could in any way contribute to the accuracy of their construction, or of the mode of handling them. Happily this most important work has on several occasions served as a guide for building and organizing other observatories on the continent of

Europe. But while we are bound, in the interests of scientific truth, to speak thus respectfully of this truly admirable work, and of the Observatory which it describes, it is impossible for us not to turn our thoughts for a moment to our own National Observatory and to its present able Director; not indeed in the spirit of contrast or comparison, but solely in the spirit of duty. Since the establishment of Pulkowa, Greenwich also has been furnished with instruments, entirely new, wholly devised by the Astronomer Royal, and constructed entirely under his superintendence. These instruments differ greatly from those erected at Pulkowa, as might be expected from the difference of genius with which the two nations are inspired; but if the descriptions of these admirable instruments were collected from the several monographs in which they are dispersed, they would form a volume every way worthy of being a companion volume to Struve's grand description of the great Russian Observatory.

Nor are the names of our great artists, Dolland, Ramsden, Troughton, Simms, and Cooke, in any respect unworthy to be placed side by side with Fraunhofer, Ertel, Reichenbach, Repsold, Mertz, Cauchoix, Secretan, and others. In the great Alhambra of Science there are niches for them all. In gladly awarding our meed of thankful acknowledgment to the memory of Struve, and to the astronomical mechanicians who so ably assisted him, we have felt it our duty to say a word of what is due to the genius of their fellow-laborers in England. In so doing we are here claiming no preëminence nor asserting any priority; invidious comparisons, if at all and anywhere out of place, would be preëminently so in a memorial notice of a man like Struve, for it was one of the characteristic features of that great man's life that, although often provoked, he was never known to contest a scientific priority; such contests, he said, were not only destructive of the peace of a philosopher's mind but highly prejudicial to the interests of science.

Struve was, as might be suggested by his labors, a man of uncommon physical strength, greatly corroborated, as he believed, by gymnastic exercises in youth: until the first attacks of his painful malady in 1858, of which we have already spoken, and which ultimately brought him to the grave, he used to say that he had never known what illness was.[6] By the advice of his physicians he was at length induced to cease, at least temporarily, from his incessant work, and travel through Europe for the restoration of his health: that result was not to be; as in other cases, so in his, it was too late. In Struve's illness there occurred a phenomenon which, though at the time supposed to be extremely singular, is not altogether uncommon. When he began to recover from exhaustion occasioned by the first attack of his malady, his memory, for all events of recent occurrence, wholly failed him; while, at the same time, it exhibited its usual, or even increased tenacity in things long since passed away. On such occasions he would seem to be living wholly in the scenes of the past, reciting passages in Greek, Latin, or Hebrew, and speaking in some of those many dialects in which he had learned to converse in his youth. In the midst, however, of this vivid resuscitation of the past, co-existing with the temporary oblivion of all recent associations, it is a touchingly suggestive circumstance to record that he never forgot the face of a friend.

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[6] This is not quite strictly correct. Struve used to say, with an easy pleasantry, that Fraunhofer's noble instrument was rightly named *the Great Refractor*, for he and his friend Schumacher had each broken a leg while using it.

At length, reluctant to continue as the nominal head of a great establishment, which he no longer felt himself able personally to direct, in December, 1861, he requested permission to resign his post. This permission was granted, accompanied, among other special marks of honor, with the grant of an hereditary estate by his sovereign; and thus the Observatory of Pulkowa passed to the directorship of his worthy son, Mr. Otto Struve. That the mantle of the father may descend upon the son is the undoubting hope and expectation of all who are interested in the science of astronomy.

On the 19th of August, 1864, a quarter of a century having now elapsed since the inauguration of the Observatory, a great gathering of astronomers from all parts of Europe assembled at Pulkowa, to pay their respects, and, as it proved, their last respects, to the man whose character and talents had shed a lustre even on the noble science of their common pursuit. Among those present on this happy occasion was the President of our own Society; of the others many had been Struve's astronomical pupils in former days, but now they occupied high and responsible positions of their own; from the former we learn how the venerable old man, now in his 72nd year, shook off for a time the bodily feebleness to which his unceasing malady had reduced him, and assuming the animation of younger years, thanked those around him for the affection which had brought them together, and acknowledged the kindness and coöperation which, through a long life, he had met with in the prosecution of his labors. But what at Pulkowa, on this and on other occasions, struck our President the most, even among the many other remarkable features of that imperial establishment, was the oneness of purpose, mutual confidence, and friendliness of intercourse, which there seemed to have found an especial home. We could fondly believe that it was thus ordained for the old astronomer to die, like a patriarch, in the midst of his children, for within a few weeks of that memorable jubilee, on the 23rd of November, 1864, Struve was called to his rest.

Struve was twice married: his first wife was a German lady, Emilia Wall, by whom he had twelve children, of these eight survive; his second wife, now his widow, was the daughter of his old colleague, Bartels, at Dorpat; by her he had issue six children, and of these four are still living. It may be interesting here to state that, notwithstanding the multiplicity and engrossing character of Struve's occupations, he always found time personally, and with affectionate concern, to superintend the education of his children.

Such are the records of this great man's life, so far, at least, as they are suited to a memoir like the present. Whatever is mortal of Wilhelm Struve rests in the churchyard attached to the beloved institution which he so long adorned. His grave lies under the shadow of its domes, and was selected by himself: but it is not these domes alone which constitute his monument; the spirit of the man still breathes in the zeal, the labors, the unanimity, which survive the master, and reign within them. That spirit will be reproduced again and again in future ages when other men, animated by the story of his example, shall endeavor to follow his steps. C. P.

ART. XVI.—*Experiments with the Ammonium Amalgam;* by CHARLES M. WETHERILL, Ph.D., M.D.

THE existence of the hypothetical radical NH_4 depends less upon the characteristics of its so-called amalgam than upon the parallelism of its salts with those of the alkalies. If, from these analogies, we accept the metallic nature of ammonium, it will be difficult to avoid assigning a similar character to the radicals of all of the organic bases; and especially to those which, like the compound ammonias, have an alkaline reaction and possess physical and chemical properties so like ammonia.

If such be the inference, we must admit numerous *compound metals*, which exists only in certain states of combination of their elements. The assumption of the elementary nature of a metal is destroyed and the ideas of the alchemists are revived; for if NH_4 be a metal and NH_3 be not one, why may not other metals, esteemed elements, be also compounds.

In examining the so-called ammonium amalgam one is interested at the great resemblance which it bears to the amalgams proper in its physical properties. The mercury has lost its fluidity or mobility, and, at the same time, its relations of cohesion and adhesion are very sensibly altered. It no longer coheres powerfully; but adheres to, or wets, platinum, iron and other metals, like the potassium or sodium amalgam. When left to itself, the swollen mass shrinks, and gradually resolves itself into $NH_3(NH_4O)$ H and Hg, because (as it is usually explained) NH_4 has a great tendency to fall apart into NH_3 and H. This explanation might be satisfactory when applied to the difficulty of isolating NH_4, as from any of its salts; but is not so in the case before us.

If ammonium falls apart thus readily in the presence of mercury, why does it combine with it at all to be decomposed in the very act of union? If it be said that in NH_4 Hg the NH_4 has so great a capacity for oxydation that it at once decomposes water, with the formation of NH_4O and H; still why should NH_4 unite with the mercury, *which is not as near to it as, or at least no nearer than, the water?* In reflecting upon the phenomenon, I conceived the idea that there is really no amalgam of ammonium formed; but, in the reaction, the sodium decomposes the water, evolving hydrogen, and forming caustic soda, which in its turn sets free ammonia from the chlorid, the mercury being also liberated. Thus $NH_4Cl+HO+Hg\,Na=NH_4O+H+NaCl+Hg$. At the same time the molecules of the mass are altered in their capacity for cohesion by catalysis, polarity of atoms, or some unknown cause, so that the bubbles of gas are retained, and swelling takes place.

Without the assumption of an unknown cause, the swelling may be accounted for by admitting that the evolution of gas brings portions of the sodium amalgam out of contact with the solution, and the mass thus remains pasty enough to retain the gas bubbles of hydrogen and ammonia for a while. The swelling may subside by reason of the final oxydation of this residual sodium amalgam.

By the kindness of Professor Henry, the opportunities of the laboratory of the Smithsonian Institution were afforded me in the winter of 1863–4, to perform the following experiments upon this subject.

The ammonium amalgams were obtained by the agency of the sodium amalgam, which was itself prepared by warming distilled mercury and projecting sodium therein. By varying the proportion of mercury, the sodium amalgam was obtained either quite fluid, pasty, in acicular crystals, or quite hard. The ammonium amalgam was prepared from all of these alloys; but when a hard amalgam was used in the experiment, the ammonium amalgam formed upon the surface of the lump and proceeded gradually to the inside, and the swelling was not so great at the close of the reaction. The more fluid the sodium amalgam was, the more readily did the swelling take place.

Ex. 1°. Sodium amalgam projected into dilute solutions of sulphuric or hydrochloric acids, or into an aqueous solution of potassa, decomposes water, but not violently. The mercury does not swell, but this phenomenon takes place immediately if a little solution of sal-ammoniac be added. It is not, therefore, merely hydrogen, (itself a hypothetical metal), in the nascent state which occasions the swelling.

Ex. 2°. Sodium amalgam in a solution of ammonia decomposes water without swelling; but this phenomenon ensues immediately if a drop of sulphuric or hydrochloric acid be added. Hence, hydrogen in the nascent state and ammonia in the *condition of stable equilibrium* do not produce the swelling. To effect this the ammonia itself must also be nascent.

Ex. 3°. When the ammonium amalgam is made in a test tube containing a thermometer, a rise of temperature of from 2° to 3° C. is indicated during the swelling. The temperature falls at the completion of the swelling. If the turgidity subsides by reason of the great affinity of NH_4 for the oxygen of the water, decomposing the latter with the evolution of hydrogen, an elevation of temperature ought to be maintained until the NH_4 has become converted into NH_4O.

Ex. 4°. If a test tube be filled with a solution of sal-ammoniac, inverted in a capsule containing the same salt, and a piece of sodium amalgam be introduced under the test tube, the ammonium amalgam is at once generated, with the constant evolution of hydrogen gas; the porous amalgam floats up to the surface of the liquid, and, subsiding after a while, gradually returns to mercury. From the time that the swelling is at its maximum until the mercury is restored to its pristine condition, there is but trifling increase of the volume of the gas, and this increase appears to be due to the minute particles of sodium amalgam which have been brought out of contact with the sal-ammoniac by reason of the turgescence. None of the gas in the test tube is absorbable by water; it is all hydrogen, and the sal-ammoniac remaining reacts strongly alkaline. The same phenomenon takes place when the amalgam is formed in a solution of sal-ammoniac in a capsule under a layer of naphtha. The presence of air, therefore, has no part in the subsidence of the swelled mass.

Ex. 5°. When the amalgam, having again sunk to the bottom of the capsule in the last experiment, is pressed with the finger against the vessel, under the naphtha, it has at first a pasty or smeary nature; but, apparently by the expression of hydrogen gas from its pores, it is very quickly restored to the ordinary condition of mercury.

Ex. 6°. When the ammonium amalgam is squeezed through a piece of muslin it is *immediately*, and without change of temperature, or other evidence of affinity, resolved into mercury.

Ex. 7°. If a drop of sal-ammoniac solution be placed upon a plate of glass, a lump of soft sodium amalgam be added, and another plate of glass be pressed upon the whole, the ammonium amalgam formed cannot swell, but expands laterally, assuming a reticular appearance by reason of the many gas bubbles which in fact thus become perceptible.

If a lump of ammonium amalgam, already in the swollen condition, be pressed between two plates of glass, it is spread out into a thin perforated film resembling lace.

Ex. 8°. If a piece of spongy platinum be heated and stirred into smelted sodium amalgam, the latter fills the pores of the platinum sponge, and coats it uniformly. A piece of this compound in contact with a solution of sal-ammoniac, evolves hydrogen and ammonia, but does not swell; nor does a globule of liquid mercury, expressed from the mass, exhibit any of the characters of the ammonium amalgam.

Ex. 9°. A piece of ammonium amalgam was wiped gently with filter paper and placed in naphtha. After a little while, (to permit any adherent water to be decomposed), a test tube full of naphtha was inverted over it, the evolution of hydrogen gas continued as the amalgam shrank, and when, after the lapse of an hour, the mercury was restored to its usual condition, a bubble of gas, equal in volume to the globule of mercury, was collected.

Ex. 10°. If a drop of sodium amalgam be heated upon a glass plate, then touched with a drop of sal-ammoniac solution, it at once swells to the full size of an ammonium amalgam which would have required several minutes if the reaction had taken place in the cold.

Ex. 11°. The sodium amalgam decomposes cold water with comparative slowness; in boiling water a rapid evolution of gas takes place, although some time is required to fully oxydize the sodium. Boiling solution of aqua ammonia acts like boiling water. In none of these cases does any swelling take place, but this phenomenon is manifested immediately if to the boiling aqua ammonia a few drops of carbonate of ammonia be added.

Ex. 12°. The ammonium amalgam is formed with apparently equal facility, employing the solutions of the following ammonia salts:—Chlorid, oxalate, sulphate, and *bi-sulphate;* the characteristics of the amalgam thus formed being alike. The reaction with bi-sulphate of ammonia is instructive, showing the formation of the amalgam in the presence of an acid which facilitates the decomposition of water by certain metals. If a piece of reddened litmus paper be saturated with solution of bi-sulphate of ammonia, and a piece of sodium amalgam be dropped thereupon, the formation of the ammonium amalgam takes place as usual, and the evolution of the ammonia neutralizes the acid, producing an alkaline reaction upon the paper under the mass.

Ex. 13°. The ammonium amalgam cannot be formed with solution of nitrate of ammonia. With this reagent a very rapid evolution of gas takes place, and a globule of mercury remains. In this reaction there are no indications of hyponitrous or nitrous acids, and a drop of sulphid of ammonium added to the resulting liquid produces no coloration, except in a film upon the globule of mercury. If a drop of fluid sodium amalgam be projected into a drop of solution of sal-ammoniac upon a glass plate, the ammonium amalgam is formed rapidly; but a few drops of solution of nitrate of ammonia poured upon the swelled mass, reduce it instantly, and without the evolutions of nitrous fumes, to the condition of ordinary mercury.

Ex. 14°. By the kindness of M. Carey Lea, Esq., who furnished me with some of the salt, I have acted upon the sodium amalgam with solution of oxalate of methylamine. A slight turgescence is exhibited in this reaction, but not nearly to so great an extent as with oxalate of ammonia. The globule of methylamine amalgam pressed between glass plates manifests a reticulated appearance from gas bubbles, but to a less degree than in the case of ammonia salts. Hydrogen and methylamine are evolved.

Ex. 15°. *With the battery.*—The ammonium amalgam was formed by the battery, using different ammonia salts in contact with mercury at the negative pole.

The general characteristics of the amalgam thus obtained were the same as when sodium was employed.

With a Smee battery of six pairs, of which each zinc plate measured 3×5 inches, the amalgam was obtained in small quantities; but ten of Bunsen's elements were required to obtain sufficient to study its properties.

By the electrolysis of a solution of sal-ammoniac contained in a U tube, which was furnished with a porus diaphragm of filter paper, decomposition took place rapidly with either of the batteries; hydrogen and ammonia appearing at the negative pole, and nitrogen, chlorine, and hydrochloric acid at the positive pole. No formation of chlorid of nitrogen was detected in the reaction.

The amalgam did not form with mercury in the negative branch of the U, the decomposition then being the same as without the metal.

The electrolysis of carbonate of ammonia yielded similar results, carbonic acid being evolved.

With solution of nitrate of ammonia in a U tube, hydrogen and ammonia appeared at the negative pole, and nitric acid and oxygen at the positive electrode. A small quantity of the gases collected at the positive pole were absorbed by water. If a globule of mercury be placed in a cup-like depression in a moistened lump of sal-ammoniac or carbonate of ammonia and be connected with the negative pole, the circuit being completed through the salt, the ammonium amalgam is formed while the current is passing.

If a piece of filter paper be placed upon a glass plate, and be moistened with a solution of carbonate of ammonia containing lumps of the salt, and if upon the paper a globule of mercury be deposited, the amalgam is beautifully manifested when the current of a ten cell Bunsen battery is passing through the mercury and paper, the metal being in contact with the negative pole. The amalgam swells out in a dendritic form, tending toward the positive pole, and maintaining its condition while the current lasts. Upon breaking the connection the swelled mass shrinks gradually. At all times, until restored to the condition of mercury, the amalgam exhibits numerous and minute gas bubbles when pressed between glass plates.

If, during the passage of the current, a glass plate be pressed upon the amalgam, it is flattened into a thin plate or film. By this means the swelling is prevented, *and the ammonium amalgam is not formed.* This result appears to be conclusive as to the non-existence of the ammonium amalgam, for, if it exist, it should be maintained in a *film* as well as in a *globule* as long as the current is passing.

The so-called ammonium amalgam could not be formed by the electrolysis of nitrate of ammonia, and a few drops of a solution of this salt causes the swelling to subside in ammonium amalgam already formed by the action of the battery upon sal-ammoniac, or carbonate of ammonia, in contact with mercury.

If spongy platinum, imbued with mercury, be placed in contact with the negative pole of the battery, and the current be passed through either a solution of sal-ammoniac, or one of carbonate of ammonia to the positive pole, no swelling takes place, even though a large globule of mercury adhere to the sponge; but violent ebullition of gas is manifested.

This is a very instructive experiment. The sponge of platinum cannot act by reason of fine pores, for they are all filled with mercury. The prevention of the amalgam formation must lie in the nature of the platinum itself; it is a catalytic or contact action.

From these experiments it would seem that:—

1st, The so-called ammonium amalgam is *not* an alloy of mercury and ammonium;

2d, The swelling of the mass in the phenomenon is due to the retention of gas bubbles; and,

3d, The coherence of the gases and liquids concerned is changed from a normal condition, exhibiting phenomena which may be classed with those of catalysis.

ART. XVII.—*Gravity and Magnetic Inclination;*[1] by PLINY EARLE CHASE, M.A., S.P.A.S.

A SOMEWHAT critical survey of the anomalies presented by the magnetic inclination, to which I referred in a former article (Proc. Am. Phil. Soc., April 21, 1865), has given me renewed reasons for regretting the want of a complete record of the investigations on which Prof. Secchi based his conclusions, that *all* "the phenomena hitherto known of the diurnal magnetic variations may be explained by supposing that the sun acts upon the earth as a very powerful magnet at a great distance."[2] As I know of no magnetic law which will account for those anomalies, I propose briefly to describe them, and to point out some relations between the gravitation currents and the *dip* of the needle, as a sequel to my papers on the influence of gravity upon the total magnetic force and the magnetic declination.

Gen. Sabine's discussions have shown some important points of difference between the magnetic disturbances at inter-tropical and extra-tropical stations, the Cape of Good Hope being magnetically, though not geographically, inter-tropical. In the third volume of the Toronto Observations, and in Prof. Bache's discussions of the observations at Girard College, projections of the daily and semi-annual inclination-curves are given;[3] and Plate V, of the second volume of the Hobarton Observations, contains a graphical representation of the diurnal variations of the inclination at the different observation hours in the four seasons. If we also project, from Gen. Sabine's tables of the mean results, the daily and semi-annual curves at St. Helena and Cape Colony, and compare the curves at the five stations, it will be found that,

1. The greatest daily disturbance of inclination occurs about noon.

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[1] From the Proceedings of the American Philosophical Society, May 19, 1865.

[2] Phil. Mag. [4], ix, 452. Faraday (Exp. Res., iii, 493), states "that the celebrated Prof. Gazzaniga, starting from his numerous experiments, which demonstrate the influence of magnetism upon the same aerial fluids, in a manner, therefore, different from that of Bancalari, was induced to consider the sun, and all the other celestial bodies, as so many enormous magnets, by which he established that attraction is merely one effect of the magnetism of the great celestial masses placed at an enormous distance,—an idea which reappeared in 1846 in Prussia, and in 1847 in France."

While admitting the intimate relationship of magnetism and gravity, I must dissent from the learned Professor's inference. For the evidence appears irresistible, that the earth's magnetism is directly dependent on the terrestrial gravitation of the thermally disturbed aerial currents, and that it is only slightly affected by the perturbations of solar and lunar gravitation, so that if we regard the relationship as a causal one, magnetism, rather than attraction, should be considered as the effect. (See Gauss, "General Theory of Terrestial Magnetism," § 30, 40; Taylor's Scientific Memoirs, ii, 232).

[3] By the kindness of Prof. Henry, I have been permitted to refer to the proof-sheets of the Fourth Section of Prof. Bache's Discussions.

2. At (magnetically) inter-tropical stations, the dip is diminished, but at extra-tropical stations it is increased in the middle of the day.

3. Increasing temperature and increasing solar altitude, augment the inclination disturbance. This is shown both by the diurnal and the semi-annual curves.

4. As a corollary of propositions 2 and 3, at St. Helena and Cape of Good Hope, the inclination-disturbance is opposed to, and subtracted from the normal dip; but at Philadelphia, Toronto, and Hobarton, the disturbance is added to the dip. Thus the inclination is

a minimum	at	St. Helena,	at	22–23^h.
" "	"	Cape of Good Hope,	"	0– 1^h.
" maximum	"	Philadelphia,	"	22– 0^h.
" "	"	Toronto,	"	22–23^h.
" "	"	Hobarton,	"	23– 1^h.

Reasoning either, *a priori*, or from Secchi's postulates, we might naturally infer that the ellipticity of the atmosphere would be increased by the direct action of the sun, and that, consequently, from the tendency of magnetic parallelism to the gravitation currents, the dip would be augmented at noon in all places between the magnetic equator and the magnetic poles.

Mr. William Ferrel, in his paper[4] which furnished the first satisfactory explanation of the barometric depression at the equator and at the poles, shows that in consequence of the earth's motion fluids tend to assume a form similar to his Fig. 1 (Math. Monthly, i, 215), "*the surface of the fluid being slightly depressed at the equator, having its maximum height about the parallel of* 35°, *and meeting the surface of the earth toward the poles.*" The direct action of the sun, in increasing the equatorial ellipticity of the air, may also increase the tendency to equatorial and polar depression, and the magnetic parallelism may, therefore, be manifested in the solar-diurnal inclination-disturbance *precisely as it is manifested* at St. Helena and Cape of Good Hope, by a diminution of dip between the parallels of 35°, and an increase in higher latitudes.

These coincidences may well suggest the need of more extensive observations in different parts of South America, Northern Africa, Southern Asia, the Pacific Ocean and the Frigid Zones, to furnish the data for determining to what extent mountain ranges, coast lines, land and water radiation, winds, and ocean currents modify the theoretical phenomena of dip and declination.

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[4] "The motions of fluids and solids relative to the earth's surface." See Nashville Journal of Medicine and Surgery for 1856, and Mathematical Monthly for 1859, i, 140, sqq.

By projecting on isoclinal and isogonic charts[5] the magnetic currents as indicated by the position of the needle in different portions of the globe, I have obtained the following results, which serve to show the character of some of these modifications:

I. *Declination.*

1. The currents manifest a tendency to follow the lines of most direct ocean communication between the warmest and the coldest portions of the globe, the general declination being westerly in the Atlantic, and easterly in the Pacific Ocean.

2. The lines of no variation are apparently determined in part by the land contours[6] that divide the waters of the globe into two great bodies.

3. The currents are deflected by the southern pointed extremities of the several continents toward the east on the eastern shores, and toward the west on the western shores, of New Holland, Africa, and South America.

4. The magnetic currents, in the three respects above enumerated, exhibit *a precise parallelism to the ocean tidal flow.*

5. In the comparatively narrow belt of the Atlantic Ocean, the declination between the parallels of 35° reaches 30°; in the broad expanse of the Pacific, the maximum within the same limits of latitude is 15°.

6. Between the parallels of 70° and 80° the declination becomes very great, and the currents tend to a gyratory or cyclonic motion, which appears to be modified by glaciers or local poles of great cold.

7. The cyclonic tendency is most marked in the southern hemisphere, where the ocean waters experience the least interruption to their normal motions. Mr. Ferrel's computation gives "28° 30′ for the polar distance of the parallel where the surface of the fluid, or the stratum of equal pressure, meets the surface of the earth."[7] It will be readily seen that at that distance the south pole is entirely surrounded by water, and it seems, therefore, highly probable, that if there were no land in the frigid zones, there would be no definite magnetic poles, but a simple polar belt toward which the magnetic currents would flow in parallel spirals,[8] except when deflected by land radiation, or tidal or other gravitation currents.

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[5] The charts that I used, were the polyconic projection of the "Lines of equal magnetic variation for the year 1858," which was constructed from Evans's English Admiralty Chart, for the United States Coast Survey Report of 1859, and No. 1 and 2 of the "Admiralty Manual for ascertaining and applying the deviations of the compass caused by the iron in a ship." 2d Edition, 1863.

[6] The influence of coast lines in producing a tendency to equality of declination is beautifully shown in the United States Coast Survey Chart of "Lines of equal magnetic declination on the Gulf of Mexico for the epoch 1860·0."

[7] Loc. citat.

[8] This inference accords with Barlow's conclusion that every place has its particular polarizing axis.

II. *Dip.*

8. The lines of equal dip are arranged in approximate parallels, around the two (principal) magnetic poles.

9. In consequence of this parallelism, they are convex toward the north in the Pacific Ocean, and toward the south in the Atlantic Ocean.

10. The magnetic parallels also approximate to the isothermal parallels, both in direction and in position, but with some important departures.

11. In South America, the magnetic equator is depressed nearly 30° south of the isothermal equator; it is, however, nearly equi-distant from the (principal) north and south magnetic poles.

12. The magnetic parallels near the magnetic poles are more convex than the isothermal lines, but they present some interesting instances of parallelism to the ocean currents, which are indisputably gravitation currents.

13. This parallelism is specially observable in the regions of the equatorial currents, the Gulf Stream, and the North Pacific and Japan currents.

14. If a series of waves were propagated through the air, from the magnetic poles to the equator, with slight deflections by the continental contours and ocean currents, they could be represented with great accuracy by the magnetic parallels.

All of the foregoing modifications, as well as the theoretical phenomena previously referred to, have their exact counterparts in the currents which tend to restore the gravitation equilibrium of the air and water. It may be unwise, ignorant as we are of the true nature of causal efficiency, to assert that one form of force is produced by another, but it is one of the most important duties of true philosophy, to observe sequences and correlations. It has long been known that magnetic action may be so directed as either to assist or counteract the attractions of cohesion, chemical affinity, and gravitation; it has also been known that, under certain circumstances, disturbances of chemical or of cohesive attraction are accompanied by magnetical disturbances,[*] but I have now shown for the first time, by independent examinations of the total force, declination, and dip, that disturbances of gravitation are similarly attended.

It would certainly be very satisfactory, if it were possible, to have some means of exhibiting, by simple laboratory experiments, the direct and mutual convertibility of gravitation and magnetism, but I fear the attempt to reproduce, in any appreciable mechanical form, the magnificent and daily repeated operations in the laboratory of nature which I have feebly endeavored to interpret, must always be futile. In order to obtain even the small amount of disturbance (·00134) which I have noted in the half-daily variation of atmospheric weight (Trans. A. P. S., xiii, 121,) it would be necessary to take observations at two stations, one of which should be 2·655[10] miles more distant from the earth's center than the other. If the differences of vapor, temperature, barometric pressure, force and direction of wind, atmospheric electricity, &c., did not so complicate the problem as to discourage even the most sanguine experimenter from any attempt at solution, any result that could be obtained under such circumstances would give little general satisfaction.

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[*] "A few years ago magnetism was to us an occult power, affecting only a few bodies; now it is found to influence all bodies, and to possess the most intimate relations with electricity, heat, chemical action, light, crystallization, and through it, with the forces concerned in cohesion; and we may, in the present state of things, well feel urged to continue our labors, encouraged by the hope of bringing it into a bond of union with gravity itself."—*Faraday: Exp. Res.*, 2614.

It is possible, however, that the end, which we should vainly strive to reach directly, may be indirectly attained. Indeed, the various stages of an indirect road have long been known, but we have not been able to compare them by any common measure. The motion of gravity, by percussion or the obstruction of simple fall, has been repeatedly converted into the motion of heat; and the motion of heat, by the thermo-electric pile, has been converted into the motion of magnetism. The experiments of Barlow, Coulomb, Kupffer, and Christie,[11] on the influence of heat upon the magnet, furnish data that may lend some aid to any investigator who seeks to ascertain the precise value and modification of each force, in these successive conversions.

But I look most hopefully to researches that are based upon differences of specific gravity. Even the experiments of Barlow and others, to which I have just referred, as well as the electro-magnetic currents which are generated by chemical solution, involve such differences; the thermal aerial currents which harmonize with and increase the effects of simple gravitation toward the sun, are caused solely by the greater centripetal tendency of the cold, dense air which has the greatest specific gravity; and the recent investigations in thermo-dynamics,

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[10] $R\times(\sqrt{D'}-\sqrt{D})=3963\times(\sqrt{1{\cdot}00134}-1)=2{\cdot}655$. At Singapore the daily disturbance of total force is only $\frac{31}{43}$ as great as it should be theoretically. I suspect that the discrepancy is owing mainly to the monsoons and other great temperature disturbances of the station, which shift the lines of force by a kind of conduction polarity.—(Faraday, Exp. Res., 3279). In other important respects there is a satisfactory correspondence between Singapore and St. Helena. E. g.

	Mean Fall.	Mean Tide. (Theoretical $\times \frac{31}{43}$).		
		0 h	6 h.	12 h.
Theoretical,	8h. 29′	−·00031	−·000173	+·00064
Observed,	8h. 28′	−·00031	−·000180	+·00064

(See "Numerical Relations of Gravity and Magnetism," Sections V, IX, and Faraday's Experimental Researches, III, 321—2).

[11] See Enc. Britann., 8th ed., xiv, 35—39.

together with the experiments of Fusinieri and Peltier,[12] confirm the natural conviction that the imponderable agents can only be manifested through their influence on ponderable matter, and, therefore, under tendencies to equilibrium with the force of gravitation. I already find a curious approximate coincidence, to which I attach little importance so long as it is unsupported by corroborative evidence, but I refer to it as an indication of the very character that we might reasonably expect, and one that may possibly become valuable in the course of future research. The last edition of the Encyclopedia Britannica, Article "HEAT," gives for the expansion and consequent diminution of specific gravity between 32° and 212° Fahr., of

Iron, ... $\frac{1}{819}$
Air, ... $\frac{3}{8}$

If we suppose their specific magnetisms to be inversely proportioned to the disturbance of their specific gravities, we have, assuming the specific magnetism of iron as the unit,

$$\tfrac{3}{8} : \tfrac{1}{819} :: 1 : \tfrac{1}{307}$$

a value which is intermediate between those given by MM. Becquerel ($\frac{1}{382}$) and Plücker ($\frac{1}{287}$).[13] This result would be somewhat modified by an accurate determination of the ratios of the linear to the cubic expansions of iron in its several forms.

Faraday disclaims the assumption of any other than a conduction polarity of oxygen (2933, 2934), but that polarity is conveyed in lines strikingly analogous to the thermal gravitation currents (See Exp. Res., 2787, and III, Pl. IV, Fig. 6), which, in their turn, accurately represent the hypothetical indirect action of the sun on the needle, through the atmospheric affection of the lines of force (2936).

I know of no physicist who has given so lucid a theoretical explanation of the various magnetic perturbations, as the illustrious Fullerian Professor, and as his hypotheses appear to me still more satisfactory when viewed in connection with the gravitation disturbances, I will briefly refer to some of the more important points that have helped to confirm me in the various views that I have hitherto advanced. Such are, e. g., the evidences of the identity of helices and magnets (2239); the existence of magnetic repulsion without polarity (2274); the relative magnetic position of gases and vapors (2416); the relations of the magnecrystallic, cohesive, and magnetic forces (2479, 2562, 2578); the probable dependence of the magnetic motions of fluids upon their mass and density (2768, 2769, 2781, 2863); the magnetic influence of winds and varying atmospheric pressure (2952, 2954);[14] the supposed velocity of magnetic transmission in space or æther (2958); the daily bi-polar minima of cold (3006); the closed circuits and prominent characteristics of the magnetic lines of force (3117, 3278, 3279, 3284); the tendency of all bodies to evolve electric currents, when moving in a magnetic field (3337); the relative diamagnetic and magnetic effects of heat and cold upon air and the diamagnetic gases, as well as upon iron, nickel, and cobalt (2861, and III, pp. 446, 460, 464, 472, 473, 489, 490).

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[12] Taylor's Scientific Memoirs, iii, 394.

[13] Prof. Frazer has kindly referred me to the cöefficients of dilatation for iron, in the "Artizan," of Dec. 1, 1860, and to the experiments of Regnault on the dilatation of air (See Jour. of F. Inst. [3], xv, 281). According to these data, the theoretical specific magnetism of oxygen would be between $\frac{1}{294}$ and $\frac{1}{374}$.

It should not be forgotten that there is no such thing within the compass of our observation, as "potential" gravity, no instance of matter in absolute rest, and just beginning or tending to move under a gravitating pull. Every particle of the earth, independently of the action of heat, chemical affinity, and cohesion, is at every instant subjected to four principal and important impulses, two toward the centers of the sun and earth respectively, and two tangential to the earth's orbit, and to its circumference. Of the several motions, the orbital one is by far the most important. Next in point of *velocity* is the one tangential to the circumference,—in point of *intensity*, the one toward the center of the sun. Since the solar central and tangential motions are in equilibrio, it seems eminently proper that the others should be considered as disturbances, which tend, as I have elsewhere shown, to give a daily ellipticity to each section of air parallel to the equator. Not only are the barometric daily tides a necessary consequence of such ellipticity; owing to the difference of specific gravity, the cold air, in addition to the proper motion of convection, is alternately drawn toward and repelled from the earth's surface; and I am inclined to believe that many of the phenomena of the deposition of dew and the magnetic perturbations, which cannot be explained by other more important gravitation currents, may be thus accounted for.

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[14] Humboldt speaks of the accumulation of electricity in the lower equinoctial regions, "at the maximum of heat, and when the barometric tides are near their minimum."—*Taylor's Scient. Mem.*, iii, 398.

ART. XVIII.—*Waterglass;* by JOHN M. ORDWAY. Part V.

[Continued from vol. xxxv, p. 196.]

Its Reaction with Stannates.

THE statement made in Part II,[1] that stannate of soda produces no alteration in a solution of waterglass, is found, by more rigid experiments, to require some limitation. What is true of the stannate prepared, as it commonly is, with such an excess of alkali as is needed to make it dissolve perfectly and keep well, does not necessarily hold good with regard to the pure normal salt. In fact the behavior of neutral stannates with acid silicates, is peculiar, and worthy of something more than a passing notice. The conditions, character, and results of the mutual reaction, may be best shown by giving a condensed account of some few of the many trials which I have made. But it will be proper in the first place to describe, somewhat in detail, the methods adopted for securing materials suitable for such experiments—methods which may lay some claim to novelty as well as convenience.

Stannate of Soda.—Time alters the crystallized salt, and dissolved stannate of soda having just one equivalent of alkali to one of acid, soon begins to change and deposit an insoluble metastannate, leaving an excess of soda in the liquid. Hence, whenever the really normal combination is wanted, it is best to make use of the freshly prepared crystals.

The commercial "preparing salt," when it is free from arsenic, may be advantageously used to produce the pure stannate.

1. A refined product, of English manufacture, was found to contain 47 p. c. $NaO.SnO_2$, 20 p. c. NaO, 11·3. p. c. $NaO.SO_3$, and 2·6 p. c. Na Cl. Some of it was purified thus:

400 grams of the white "preparing salt" and 84 grams of acetate of baryta were dissolved together in 1600 cub. cent. of water; and, as the solution was too caustic for a paper filter, it was passed through thick cotton cloth, the first portions of the liquor being poured back till the filtrate came no longer turbid. The clear liquid treated with about its own bulk of alcohol of sp. gr. 0·850, soon gave an abundant deposit of fine, indistinct crystals, which were gathered into a stout cotton cloth and submitted to very strong pressure. The damp product, weighing 181 grams, was dissolved in twice its weight of water, and the filtered solution was mixed with somewhat more than its own bulk of alcohol. The crystals of this second precipitation, after undergoing very hard pressure, were spread out on paper and left in a cold place over night, to exhale the alcohol. They now weighed 143 grams and contained 76 p. c. of $NaO.SnO_2$. They were free from carbonate, hydrate, sulphate, and

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[1] This Journal, [2], xxxii, 339.

chlorid of sodium, but made a slightly opaline solution with water, thus showing the presence of a trace of metastannate.

Crystals fresh from the press give a clear and complete solution; and as a trifling amount of adhering alcohol is seldom of any consequence, it is best to expose the pressed cake to the air as little as possible.

When the product is freed from mother liquor by absorption and careful drying it appears to have the composition $NaO, SnO_2, 3HO$.

To get pure and clean crystals for an exact determination of the water a strong solution of the preparing salt was treated with acetate of baryta, and as much alcohol was added to the filtrate as it would bear without turning milky. In two days there gathered around the sides and bottom of the bottle a thin, hard coating of transparent crystals, which were detached and pressed in absorbent paper till they appeared dry, but not at all effloresced. The average of two analyses showed 55·695 p. c. of SnO_2 and 22·921 p. c. of NaO. This would make $\dot{Na}\,\ddot{Sn}\,\dot{\bar{H}}_{3\cdot 2}$.

But it is worthy of notice that when the precipitated crystals are rapidly dissolved in water, heat is produced instead of cold; and it might be inferred from this fact, that stannate of soda is capable of entering into intimate combination with more water than is contained in the trihydrate. Thus, by the solution of a quantity of the pressed crystals, still damp, in a little more than twice their weight of water the thermometer was raised 5° C.

Stannate of Potash.—Stannate of Potash is more soluble in water than the soda salt,[2] and cannot be, by any means, so neatly and easily precipitated in the solid state.

[2] Storer, in his Dictionary of Solubilities, under the heads "Stannate of Potash" and "Stannate of Soda," has collected nearly all that is known respecting these salts. We are there told that, according to Fremy, stannate of soda is *much* more soluble in cold than in warm water. To test this matter, and to reduce indefinite terms to exact numbers, I have made the following experiments:

I. A saturated solution was prepared by frequently agitating water with an excess of the fine, freshly prepared crystals, in a flask surrounded with snow. It was filtered in a funnel also kept at 0° C. The clear liquor warmed to 15·5° C. had the sp. gr. 1·472. It contained 32·1 p. c. of dry stannate.

II. A saturated solution made in a room kept at 10°C. had the sp. gr. 1·448, and contained 31 p. c. of the dry salt, or 39 p. c. of $\dot{Na}\,\ddot{Sn}\,\dot{\bar{H}}_3$.

III. A saturated solution was made in a flask kept in water at 20° C. Cooled to 15·5° C. it had the sp. gr. 1·438. It contained 30·3 p. c. of $\dot{Na}\,\ddot{Sn}$. So 100 parts of water dissolve 67·4 parts of the crystals, $\dot{Na}\,\ddot{Sn}\,\dot{\bar{H}}_3$, at 0° C.; and at 20° C. only 61·3 parts, or nine-tenths as much.

Stannate of potash shows no such anomaly, but its solubility is somewhat increased by heat.

IV. At 10° C. a saturated solution was made with crystals of $\dot{K}\,\ddot{Sn}\dot{\bar{H}}_3$ formed by spontaneous evaporation *in vacuo*. It had the sp. gr. 1·618 and contained 42·3 p. c. of $\dot{K}\,\ddot{Sn}$.

V. A saturated solution made at 20° C. had the sp. gr. 1·627 and contained 43 p. c. of dry stannate of potash.

By using many successive portions of strong alcohol, we may withdraw the dissolving water and compel hasty and pretty complete crystallization ; and as all the precipitates subside with great rapidity, the whole round of operations may be carried out in a few hours. But while stannate of soda can be readily procured coarse grained, white, and of normal composition, the precipitated potash salt is very fine, and besides obstinately retaining much of whatever organic coloring matter may have been present in the original solution, it is liable to come out at last with somewhat too much or too little alkali.

2. A crude stannate prepared by heating strongly a mixture of tin, caustic potash, and nitrate of potash, was dissolved in twice its weight of cold water. 400 c. c. of the clear, decanted liquor, were thoroughly agitated with 400 c. c. of alcohol of sp. gr. 0·840, and gave a dense liquid deposit measuring 275 c. c. This being in turn treated with 400 c. c. of alcohol, yielded 175 c. c. of still denser liquor. A third precipitation with 400 c. c. of alcohol gave a deposit with some solid matter beginning to appear in it.

So far a weaker spirit had been used so as to allow the impurities to remain in solution while the stannate was thrown down.

400 c. c. of alcohol of sp. gr. 0·820 now changed the precipitate into a pasty mass, which became crumbly on being stirred up again with 400 c. c. of strong alcohol. The stannate was then gathered in stout canvass and subjected to great pressure. The resulting cake still contained about one-eleventh of an equivalent of alkali in excess, there being originally two equivalents of potash to one of stannic acid ; it was therefore dissolved in a little more than its weight of water and submitted to another round of treatment with alcohol. The hard pressed final product contained 73 p. c. of dry stannate. Being spread out and exposed to the air for some hours, it lost all smell of spirit, and was then pure and almost exactly normal.

When greater nicety is required, such a product should be dissolved in a little less than its weight of water and recrystallized by spontaneous evaporation *in vacuo*.

A careful analysis was made of some pure, hard, colorless, transparent, oblique rhombic crystals produced by this last method and dried by pressure in absorbent paper. It showed

VI. A saturated solution made at about 45° C. had the sp. gr. 1·658 and contained 44 p. c. of $\dot{K}\ddot{Sn}$.

Therefore, at 20° C., 100 parts of water take up 110·5 parts of $\dot{K}\ddot{Sn}\dot{H}_3$, or nearly twice as much as of stannate of soda crystals.

When there is present an excess either of alkali or of stannic oxyd, a saturated solution contains less of the net normal stannate.

VII. A solution of $\dot{K}\ddot{Sn}_{0\cdot9}$ saturated at 0° C. had the sp. gr. 1·64 and contained 40·4 p. c. of $\dot{K}\ddot{Sn}$.

VIII. A solution of $\dot{K}\ddot{Sn}_{1\cdot2}$ saturated at 0° C. had the sp. gr. 1·74 and contained 41·5 p. c. $\dot{K}\ddot{Sn}$ and 5·3 p. c. of SnO_2 in excess.

Or speaking with special reference to the oxyd of tin, we may say that its solubility increases as the amount of alkali present is diminished.

49·430 p. c. of stannic acid, and 30·704 p. c. of potash. If we call the equivalent weight of tin 59, these proportions would make $\dot{K}\ddot{\mathrm{Sn}}\dot{H}_{3\cdot3}$. The true formula must therefore be $KO.SnO_2.3HO$.

The specific gravity of these crystals was found to be 3·197.

Metastannate of Potash.—When a dilute acid is slowly, and with constant stirring, dropped into a cold solution of normal stannate of potash, there is no permanent precipitation till more than three-fourths of the alkali is abstracted. The stannate thus robbed of its base, remains perfectly dissolved, but has acquired different properties. Alcohol now gives with it not a liquid or crystalline deposit, but a light flocculent precipitate which, when it is well drained and exposed to the air for a day or two, dries to heavy, transparent, gum-like lumps. There is thus opened up a neat way to procure, in a state of purity, metastannates containing various proportions of alkali.

3. A nine per cent solution of $KOSnO_2$ was treated with enough five per cent nitric acid to take up two-fifths of the potash. When the liquor had recovered its clearness, somewhat more than twice its bulk of alcohol of sp. gr. 0·830 was stirred in. The precipitate, after being washed once with alcohol, was drained on a filter and left on absorbent paper till it became a transparent, exceedingly shrunken mass containing 79 p. c. of $\dot{K}\ddot{\mathrm{Sn}}_{4\cdot6}$. This easily dissolved in twice its weight of water.

4. In a similarly conducted experiment, the same quantity of stannate with twice as much acid as before, gave nearly twice as much metastannate containing 59 p. c. of $\dot{K}\ddot{\mathrm{Sn}}_{5\cdot6}$. This dissolved in four times its own weight of water to a faintly opaline liquid.

5. Some of the solution of 3 was diluted and cautiously treated with sufficient nitric acid to neutralize one-third of the remaining potash. The despoiled metastannate was then precipitated with alcohol. The final product consisted of $\dot{K}\ddot{\mathrm{Sn}}_{10\cdot6}\dot{H}_4$ and was readily soluble in water.

It is well to dissolve the rough metastannate and reprecipitate, in order to get rid of the last traces of nitrate and obtain a substance which will give a clear and mobile solution. And the product of the second precipitation can be advantageously freed from the adhering liquor by pressing it very strongly in stout closely woven cotton cloth. Even the first precipitate may be pressed, but it is much softer and the pressure must be applied very gradually indeed; otherwise the pulp itself will pass through the pores of the cloth.

Waterglass with Stannates.—As in some preliminary experiments sulphates were not perceived to exert any influence on the reaction of stannates with silicates, sulphates were afterward purposely added, to serve as indicators of the amount of mother liquor retained by the precipitates. The liquid stannate of soda was made to contain about five per cent of sulphate of soda, and stannate of potash was dissolved in a weak solution of potash sulphate. The waterglass was such as had been well purified by precipitation with alcohol.

While treating this part of our subject we will take the formula adopted in planning the experiments, and consider silicic acid as $Si\,O_2$.

6. 80 g. of a solution containing 14 p. c. of $NaO\,SnO_2$, were mixed with 32 g. of a 29 p. c. solution of $NaO.2SiO_2$, so as to have in the united liquors $\dot{N}a_2\,\ddot{S}n\,\ddot{S}i_2$.

There was no apparent change at first, but, in the course of two days, it became an opaque, consistent jelly from which no liquor could be decanted.

Such were also the products in most other cases; and at first these jellies proved rather intractable, for when they were subjected to quick pressure in common cotton cloth, such as had been used in former experiments, both liquor and coagulum passed through. Farther experience showed it best to let the mixtures stand several days to get fully gelatinized, and then proceed to press very gradually in thick, closely woven canvass. The cloth was folded over the drained precipitate and placed between two bits of pine board. To insure moderation at first, the boards were put under a small hand screw which was very slowly turned till the greater part of the clear liquor was forced out and the substantial portion of the coagulum was reduced to small bulk. The boards with their contents were then transferred to a large press urged by the full strength of two men and theoretically capable of multiplying the power nearly 1200 times. The pressed products were hard and brittle, and could be rubbed in a mortar to a damp powder without caking much under the pestle. The powder was generally soluble in chlorhydric acid. On being ignited the cake contracted greatly and became opaque, but showed no sign of fusion.

To make an analysis a weighed portion of the powder was treated with chlorhydric acid in excess and dried down at a moderate heat. The residue digested for twenty-four hours with chlorhydric acid and then well washed with water containing a little of the same acid, left the silica pure and in full quantity.

Another portion of the powder was treated with nitric acid in excess and dried down. By washing the dry mass with water and igniting and weighing the undissolved matter, the sum of the silica and oxyd of tin was ascertained; and a comparison of this with the former determination showed the per centage of oxyd alone. The solution being evaporated to dryness the resulting saline matter was fused and weighed. The amount of sulphate in the mixed salts was found by means of chlorid of barium; and then the quantity of nitrate and the corresponding amount of alkali were easily calculated.

The composition of the mother liquor was ascertained by the same method. Then the percentage of sulphate in the pressed cake, divided by the percentage of sulphate in the mother liquor, showed the quantity of mother liquor retained in one part of the cake. And finally the net composition of the curd was found by deducting from the gross amounts of silica, stannic acid, and alkali in the pressed cake, the quantities of the same substances due to adhering mother liquor.

In the present instance the hard pressed curd weighing 24 grams, contained 40 p. c. of mother liquor, and 38·6 p. c. net of $\dot{N}a_3\,\ddot{S}n\,\ddot{S}i_5$.

7. 75 g. of a 14 p. c. solution of $\dot{N}a\,\ddot{S}n$ were mixed with 20 g. of a 20 p. c. solution of $\dot{N}a\,\ddot{S}i_3$, so as to have a mixture of $\dot{N}a_4\,\ddot{S}n_3\,\ddot{S}i_3$. In a few hours it became a firm opaque jelly. After standing six days it was pressed and left 24·3 g. of cake containing 34 p. c. of mother liquor and 38 p. c. net of $\dot{N}a_5\,\ddot{S}n_4\,\ddot{S}i_8$.

8, *a*. 60 g. of a 14 p. c. $\dot{N}a\,\ddot{S}n$ solution were mixed with 40 g. of a 20 p. c. solution of $\dot{N}a\,\ddot{S}i_3$; so that the quantities of SnO_2 and SiO_2 should be equal. It soon began to get opaline, and, in the course of two hours, made an opaque jelly, so stiff that it was not disturbed by inverting the dish. The hard pressed cake weighed 36·3 g. and contained 45 p. c. of mother liquor and 36 p. c. of $\dot{N}a_4\,\ddot{S}n_3\,\ddot{S}i_6$.

8, *b*. 240 g. of a 3·5 p. c. $\dot{N}a\,\ddot{S}n$ solution were stirred into 200 g. of a 4 p. c. $\dot{N}a\,\ddot{S}i_3$. In the course of two hours it formed a thin opaque jelly. After two days this was thrown on a cloth and much clear mother liquor drained out without pressure. The remainder was very gradually squeezed and finally hard pressed, and gave a cake weighing 31 g., which showed 17 p. c. of mother liquor and 36 p. c. of $\dot{N}a_4\,\ddot{S}n_3\,\ddot{S}i_8$.

8, *c*. 480 g. of a 1·75 p. c. solution of $\dot{N}a\,\ddot{S}n$ were mixed with 400 g. of a 2 p. c. $\dot{N}a\,\ddot{S}i_3$ solution. There was no change at first, but in the course of two days it formed a thin jelly. After standing six days it was drained, squeezed, and hard pressed, and the cake weighed 26·7 g. This product contained 21 p. c. of mother liquor and 36 p. c. of $\dot{N}a_4\,\ddot{S}n_4\,\ddot{S}i_{10}$.

These three trials show something of the influence of dilution, though in the case of *c* longer standing had also modified the composition of the coagulum by increasing the amount of stannic oxyd rendered insoluble.

9, *a*. A 120 g. mixture was compounded so as to contain 18 g. of $\dot{N}a_3\,\ddot{S}n_2\,\ddot{S}i$. There was no change at first, but in the course of two days it became a thin opaline jelly. After seven days being hard pressed it yielded a translucent cake weighing 8·6 g. and containing 37 p. c. net of $\dot{N}a_9\,\ddot{S}n_7\,\ddot{S}i_{13}$.

9, *b*. A similar mixture was made by adding a saturated solution of stannate to fused crystals of silicate and evaporating till it contained 40 p. c. of solid matter. It remained clear, and when exposed to severe cold showed no sign of crystallization.

10. A mixture was made containing somewhat over 16 p. c. of $\dot{N}a_3\,\ddot{S}n\,\ddot{S}i_2$. There was no change for six weeks.

11, *a*. A mixture was made so as to contain about 16 p. c. of $\dot{N}a_2\,\ddot{S}n\,\ddot{S}i$. There was no change for some days, but in the course of a month it gelatinized.

11, *b*. A saturated solution of stannate was put with as much melted $\dot{N}a\ddot{S}i\bar{H}_8$ as would make $\dot{N}a_2\ddot{S}n\ddot{S}i$, and the mixture was evaporated at a gentle heat till it contained 47·5 p. c. of solid matter. It remained clear and would not crystallize though set out in the open air during several of the coldest days of winter. The strong liquor bore boiling without apparent change, but when much diluted was coagulated by heat.

Stannate of Potash with Silicate of Potash.

12. A mixture made so as to contain 21 p. c. of $\dot{K}_2\ddot{S}n\ddot{S}i$, underwent no visible change in a month.

A mixture containing 19 p. c. of $\dot{K}_3\ddot{S}n\ddot{S}i_2$ also remained unchanged for a month.

13, *a*. 72 g. of a 10 p. c. solution of $\dot{K}\ddot{S}n$ were mixed with 60 g. of 12 p. c. $\dot{K}\ddot{S}i_3$, so as to bring together about equal weights of SnO_2 and SiO_2. There was no perceptible alteration for a day, but in three days it became a thin transparent jelly. After standing a week it was squeezed gradually and then hard pressed. The transparent product weighed 28 g. and contained 39 p. c. net of $\dot{K}_6\ddot{S}n_4\ddot{S}i_{13}$.

13. *b*. 288 g. of a 2·5 p. c. $\dot{K}\ddot{S}n$ solution were mixed with 240 g. of 3 p. c. $\dot{K}\ddot{S}i_3$. It remained apparently unchanged for a week. In the course of the second week it became gelatinous, but no liquor could be pressed out. At the end of three weeks it had fully coagulated, and being now hard pressed it gave a translucent cake weighing 23 g. This contained 17 p. c. of mother liquor and 40 p. c. of $\dot{K}_6\ddot{S}n_6\ddot{S}i_{14}$.

14. 77 g. of a solution containing 15 p. c. of $\dot{K}\ddot{S}n$, were mixed with 33 g. of a 20 p. c. solution of $\dot{K}\ddot{S}i_3$, so as to make in all $\dot{K}_3\ddot{S}n_2\ddot{S}i_3$. It underwent no noticeable modification for some time, but in two days it formed a firm, slightly opaline jelly. After standing a week, it was hard pressed and yielded a mass weighing 28 g. and containing 38 p. c. net of $\dot{K}_4\ddot{S}n_2\ddot{S}i_7$.

Metastannate of Potash with Potash Waterglass.

15. 20 g. of a 10 p. c. $\dot{K}\ddot{S}i_3$ liquor were mixed with 20 g. of a solution containing 10 p. c. $\dot{K}\ddot{S}n_{4\cdot4}$. There was no change for some time, but in the course of 17 days it thickened. The hard pressed solid part weighed 5·5 g.

16. 10 g. of a 30 p. c. solution of $\dot{K}\ddot{S}i_3$ were stirred into 10 g. of a 24 p. c. solution of $\dot{K}\ddot{S}n_{5\cdot3}$. It soon got a little thicker but did not gelatinize at all.

Weaker solutions of the same metastannate and silicate, were mixed with similar results.

17. 10 g. of a 10 p. c. $\dot{K}\ddot{S}i_3$ solution were put with 10 g. of a 10 p. c. $\dot{K}\ddot{S}n_{7\cdot5}$; no immediate change. In the course of 10 days it gelatinized, and when hard pressed it gave a cake weighing 4 g.

18. 20 g. of a 15 p. c. solution of $\dot{K}\ddot{S}i_3$ were mixed with 20 g. of 16 p. c. $\dot{K}\ddot{S}n_{10\cdot6}$. It continued unchanged a month, but at length gelatinized.

A comparison of all the experiments from which this selection has been made, brings to light nothing definite or uniform in the products of the reaction of stannates with silicates. The strange looking *quasi* formulas here used to show their composition by equivalents instead of per centages—like the similar expressions employed for the precipitates described in Parts III and IV,—would be altogether preposterous, were they intended for anything more than compact and readily collatable expressions of the results of analyses. In vain do we search for any principle that will enable us to assign a rational constitution to substances which derive their unlimited variety of composition from mere accidents of dilution, purity, temperature, and of time allowed for segregation. Many picked instances, like 7, 8 *a*, and 9, might countenance the supposition of a tendency to the formation of an exact silicate of tin $SnO_2\ 2SiO_2$, united with different proportions of alkali. But such hasty generalization is checked by an enlarged view and more particularly by the special test experiments, 8 *a, b, c.* All that seems to be predicable of the reactions is that mixtures containing as many equivalents of alkali as the sum of the equivalents of SnO_2 and SiO_2, are likely to undergo little or no change at the common temperature of the air; but when the mixture contains less alkali, gelatinization will occur in a few hours or days; and the curd will be greater in amount according as the strength of the liquors put together is greater, and as the total proportion of alkali is less.

The segregated matter retains the alkali with no little force, for when the air-dried precipitate is washed with water a part indeed of the alkali is removed, but the greater portion remains obstinately in combination.

19. Some of the cake of 13 *a*, was reduced to powder and kept over lumps of caustic soda eighteen days. The dry powder was well washed with cold water. The air-dried residue amounted to 42 p. c. of the quantity of fresh cake taken, and contained 78·6 p. c. of $4KO\,.\,4SnO^2\,.\,13SiO^2$. Only one-third of the potash had been washed out.

It is difficult to ascertain whether the fresh undried cake may undergo dissociation in any greater degree; for if we attempt to wash it, though a part settles, the supernatant liquor remains milky a very long time, and the suspended matter cannot be separated by filtration, as it readily goes through the pores of the paper.

After seeing how a deficiency of alkali facilitates the coagulation of a mixture of stannate and silicate, we should hardly expect to find metastannates so slow in producing any effect on waterglass. But metastannates are evidently not mere polyacid stannates, and a higher degree of compatibility is the less surprising when we consider the many points of resemblance between metastannate of potash and waterglass itself. Both are uncrystallizable and dry to transparent gum-like masses, indefinitely soluble in water. Both are precipitated by alcohol with partial decomposition, and indeed in both the acid and base seem to be in a state of association rather than of strict chemical combination. The following experiments, illustrating this point, may be compared with the similar trials of silicates mentioned in Part III:

20. 350 g. of a solution containing 6 p. c. of $\dot{K}\ddot{S}n_{4\cdot 4}$, were mixed with 850 c. c. of alcohol of sp. gr. 0·840. After two washings with alcohol, the flocculent precipitate was collected in a cloth and hard pressed. The thin translucent, brittle cake, after twelve hours exposure to the air, weighed 24 g. and contained 82 p. c. of $\dot{K}\ddot{S}n_{7\cdot 4}$. It was dissolved in 9 parts of water and treated with alcohol as before. The second precipitate when pressed and aired contained 81 p. c. of $\dot{K}\ddot{S}n_{8\cdot 7}$. The third precipitate contained 80 p. c. of $\dot{K}\ddot{S}n_{9\cdot 3}$. The product of a fourth precipitation showed 81 p. c. of $\dot{K}\ddot{S}n_{9\cdot 8}$. The fifth precipitate contained 82 p. c. of $\dot{K}\ddot{S}n_{10\cdot 5}$. And the sixth product was readily soluble in water and showed 72 p. c. of $\dot{K}\ddot{S}n_{11\cdot 4}$.

21. 50 g. of pressed crystals of stannate of potash were dissolved in water, so as to make 600 c. c. 200 c. c. of 5·4 p. c. nitric acid were stirred in, and, when the liquor had recovered its transparency, 1700 c. c. of alcohol were added. After one washing with alcohol the pultaceous precipitate was very gradually squeezed and then hard pressed. After some hours exposure to the air, the cake weighed 27 g. and contained 79 p. c. of $\dot{K}\ddot{S}n_{5\cdot 2}$. It was dissolved in 9 times its weight of water and treated with alcohol. The second flocculent deposit hard pressed and dried in the air, contained 80 p. c. of $\dot{K}\ddot{S}n_{6\cdot 3}$. The precipitation was repeated many times, and finally the tenth product, weighing 5·5 g., was not entirely soluble in water. The soluble part contained $\dot{K}\ddot{S}n_{17\cdot 4}$, and the small insoluble residue had nearly the same composition.

Metastannate of potash is also thrown down as such by many potash salts; and here too, as with waterglass, the acetate and the chlorid prove most efficient.

22. 25 g. of a solution containing 21 p. c. of $\dot{K}\ddot{S}n_{4\cdot 4}$ were mixed with 25 g. of a 20 p. c. KCl solution. The very bulky deposit after being squeezed and subjected to hard pressure, weighed 6·4 g. It contained 28 p. c. of mother liquor and 64 p. c. of $\dot{K}\ddot{S}n_{7\cdot 7}$, and was readily soluble in water.

23. 40 g. of a 9 p. c. solution of $\dot{K}\ddot{S}n_{7\cdot 7}$ mixed with 40 g. of a 10 p. c. solution of acetate of potash gave a precipitate that compressed to 4 g. and contained 68 p. c. of $\dot{K}\ddot{S}n_{8\cdot 1}$. This was wholly soluble in water.

Carbonate, sulphate, chromate, or nitrate of potash give a precipitate after a time when added in inconsiderable quantity to the metastannate; but neutral stannate, of whatever strength and in whatever quantity, has no effect upon a metastannate solution.

On account of the insolubility of metastannate of soda, soda salts soon disturb a solution of metastannate of potash. Thus a liquid containing 10 p. c. of $\dot{K}\ddot{S}n_{5..6}$, became opaque with its own weight of a 2 p. c. solution of $\dot{N}a\ddot{C}$. A much less quantity of sulphate of soda sufficed to produce the same effect. A soda salt therefore affords a test of the presence of metastannate in a stannate of potash solution, but the liquors must not be too concentrated.

Fremy says that stannate of potash is precipitated from its solution by almost all soluble salts, and even by salts of potash, soda, and ammonia. He must have operated with liquors that had been kept too long and had thereby become contaminated with metastannate; for my own experiments afford no confirmation of his statement as far as potash and soda salts are concerned. On the contrary I have not found these salts to have any effect at all on solutions of fresh and really normal stannates of potash and soda. Stannate of soda when mixed with a neutral salt, does indeed become turbid by standing some time, but so does the diluted stannate without any addition.

Indefiniteness of composition pertains to the metastannates in no less degree than to waterglass. Fremy once assigned the formula[3] $KO.2Sn_3O_6+5HO$, but afterward settled down on $KO.Sn_5O_{10}+4HO$, and thus gives his reasons for the change:[4]

"Je préparais autrefois les metastannates en faisant bouiller de l'acide metastannique avec des alcalis, et en précipitant ces dissolutions par l'alcool; j'ai reconnu recemment que pendant l'ebullition, une partie du metastannate se transforme en stannate; en traitant ensuite la liqueur par l'alcool, je précipitais un melange on peut-être une combinaison de stannate, et de metastannate, etc."

"C'est ainsi que j'avais été conduit á représenter l'acide metastannique par la formale Sn_3O_6. Mais je prepare actuellement des metastannates qui ne penvent contenir de traces de stannates; aussi leur analyse donne-t-elle toujours un equivalent d'acide metastannique représenté par la formule Sn_5O_{10}."

Fremy's later method of preparing a metastannate which should contain no stannate, was to dissolve metastannic acid in caustic potash and precipitate the combination by adding to the solution bits of solid caustic potash. The precipitate was dried by absorption on unglazed porcelain. But it can hardly be possible by mere absorption to get a soft, pulpy substance, entirely free from so thick and dense a mother-liquor as the caustic potash would make. Fremy made five analyses which came out very nearly alike, as might indeed well happen so long as the products analyzed were made in just one way and by one individual. But the tacit assumption that none of the 10·5 to 10·9 p. c. of potash found, was due to retained mother liquor, is manifestly gratuitous. Nor was there any good reason for supposing that cold caustic potash exerts no modifying action on metastannate; for time often brings about in part what heat effects rapidly and fully, and in fact I have seen an abundant precipitate, produced by moderately strong potash liquor, disappear in an hour or two with but a very slight elevation of temperature. Some idea of the difficulty of separating a mother liquor from a gum-like precipitate, may be gathered from the experiments on precipitated waterglass given in part IV. There we had a substance as capable as metastannate of potash of being reduced to a very compact form. There we had the advantage of a mother liquor much thinner and much more easily separable from the curd. There was also a powerful pressure effectually accomplishing in a few moments, and consequently without chance of modification by time, what the contractility of metastannate and the imbibing power of porcelain could do but slowly. Yet the precipitated silicates retained, on an average, about thirty per cent of mother liquor. Due allowance being made for the greater density of a stanniferous compound, it certainly would not be making a very high estimate to say that metastannate of potash thrown down by potash, after as complete a drainage as possible, must retain not less than ten per cent of strong caustic potash liquor or some three per cent of dry potash in excess. I see no chance of actually finding out how much of the potash is combined and how much is free in such a case; for if we add any *indicator*, it would alter the conditions of the experiment.

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[3] Annales de Ch. et de Phys. [3], xii, 476.

[4]. Id., xxiii, 395.

Hence, Fremy's formula having no fixed basis must be looked upon as a rather uncertain approximation to the true composition of the metastannate made by his method.

According to Rose,[5] Weber found solid metastannate of potash to consist of $KO.7SnO_2+3HO$. But we are not told by what process it was made.

Berzelius says that one part of potash will dissolve sixteen parts of stannic acid, and these proportions would make about $KO.10SnO_2$.

As to matastannates obtained by precipitation with alcohol or with neutral potash salts, we have found that by varying the conditions the composition of the products may be made to range from less than five up to more than seventeen equivalents of binoxyd of tin to one equivalent of potash. And Graham[6] has shown that when the alkali is eliminated by neutralization and dialysis, metastannic acid is itself soluble in pure water. Hence there is probably no limit to the possible diminution of potash in the still soluble metastannate.

I have heretofore enunciated the general rule that soluble salts having as bases, ferric, chromic, stannic, or other oxyds containing more than one equivalent of oxygen to one of metal, may have a large part of the acid withdrawn and still remain dissolved. And now it is interesting to find that when binoxyd of tin is brought into solution by an alkali, a similar principle holds good, and the greater portion of the base may be removed without any immediate permanent precipitation of the acid. Metastannate of soda, however, is but temporarily soluble, and is thrown down from solutions of the neutral stannate by long standing or by boiling. It is also soon deposited from a solution in which the alkali of normal stannate has been partly taken up by a stronger acid. These precipitates are, by no means, of the same composition, though they often approach very closely to the proportions required for $NaO . 5SnO_2$.

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[5] Poggendorff's Ann. lxxv, 15.

[6] Repertoire de Chimie, Sept., 1864, p. 184.

24. Some purified stannate of soda that had been dried in the air and then kept in a well stopped bottle for two years, on being treated with ten times its weight of cold water, left undissolved one twenty-third of the tin, combined with soda enough to make $NaO . 3{\cdot}7SnO_2$.

The clear liquor, by standing several weeks, let fall one-seventeenth of its tin as $NaO . 4{\cdot}7SnO_2$.

25. A ten per cent solution of normal stannate being kept 34 days, deposited one-twelfth of the oxyd of tin in combination with enough alkali to make $NaO . 5SnO_2$.

26. A ten per cent solution of $NaO . SnO_2$ was boiled a few moments and let fall one-sixteenth of its tin with some soda forming $NaO . 5SnO_2$.

27. A solution containing five per cent of pure stannate, by boiling deposited one-seventh of the tin as $NaO . 5{\cdot}7SnO_2$.

28. Boiling a two per cent solution of $NaO . SnO_2$ caused the precipitation of over one-third of the tin and enough soda to make $NaO . 7SnO_2$.

29. A one per cent solution of pure stannate of soda required long boiling to make a decided precipitate and the clear liquor filtered out of the bulky product very slowly. The well drained gelatinous residue was soluble in water and consisted of $NaO . 7{\cdot}5SnO_2$. It contained over one-half of the tin.

30. A solution containing 1·1 p. c. of $NaO . SnO_2$ and 3 p. c. of $NaCl$, being boiled five minutes, gave a dense, opaque precipitate very easy to collect, drain and press. It contained one-fourth of the tin and consisted of $NaO . 6{\cdot}7SnO_2$.

The addition of chlorid of sodium and subsequent boiling causes a precipitation in a solution containing no more than 0·1 p. c. of normal stannate of soda.

So far as is known at present, there is nothing to show that any one of the metastannates more than another is entitled to rank as a definite chemical compound. None crystallizes; none is a product invariably coming the same by several different ways of formation; none exhibits a plain analogy to any undoubted exact chemical combination; nor is there any point up to which the variation gradually decreases, and beyond which it gradually increases. There being then nothing certain on which a formula may be based, it seems hardly right to take any one chance product, and, rejecting the odd parts of equivalents, to set down that product with its analysis so amended, as the true metastannate of potash or of soda. The composition of substances can not always be squared with exact atomic proportions, for there is a class of indefinites in which one of the constituents may admit of a large increase or diminution without apparent alteration in the character of the compound. As the world was very slow to comprehend Wenzel's proposition that bodies must unite in fixed and invariable proportions, so has it since been quite as slow to learn that there are cases in which this great principle does not hold good. Wenzel says:[7] "Dass eine jede Verbindung der Körper, eine bestimmte und unveränderlich bleibende Abmessung haben muss, die ohne aüsserlich mitwürkende Ursachen weder grösser noch geringer werden kann, weil sonst auch nichts gewisses aus ihrer Vergleichung bestimmt werden könnte, ist schon an sich klar. Es folgt daher nothwendig, dass eine jede mögliche Verbindung zweyer Körper, mit jeder andern beständig in dem genauesten Verhaltnisse stehet, und dieses Verhältniss drücket den Grad der Verbindung aus." That the constitution of bodies is determinate and unalterable, is hardly self evident, and indeed it is a matter to be proved by the balance rather than by mere reasoning. It is true that the confirmatory instances have been found to be numberless. Still a great many do not necessarily make all, and a single negative example is sufficient to disprove absolute universality. Wenzel himself, while laying the sure foundations of chemical science, fell in with some of the indefinites and thus unwittingly furnished some negative instances. He proceeded to determine the equivalents by neutralizing the acids with different bases, and worked correctly so long as those bases were protoxyds. But such a method is as little applicable to alumina and similar peroxyds as it is to bone earth,—"Elfen beinerde,"—which he reckoned among the simple bases. So after attempting to saturate nitric acid with alumina, he concluded that[8] "Das Verhaltniss der Alaunerde zum stärksten Salpetersauern, ist also beynahe . . . . wie 349 : 240." Instead of forming the normal nitrate Wenzel in this case must have got the soluble basic $3Al_2O_3 . 2NO_5$, and stopped at a supposed but not real limit of solubility.

Cases of indefinite combination are not unlike those of solution in which such things as gum or albumen are concerned. These may unite with water in all proportions, while most crystallizable substances dissolve, at any given temperature, until the liquid contains a certain percentage of solid matter,—a percentage which is exactly definable for each particular salt. In the phenomenon of fusion too, though almost all bodies liquefy at a temperature which is invariable and exact for each, yet there are a few substances that pass through an intermediate state of softening before fully melting. In fusion as well as in solution, abrupt transition from the solid to the fluid state is the general rule; and yet, as everybody allows, it is not the universal law. And why should we be over-reluctant to admit the possibility of some instances of chemical union in indefinite proportions?

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[7] Lehre von der Verwandschaft der Körper. Dresden, 1782. p. 4.

[8] Idem, p. 113.

Normal Silicate of Soda.—The only alkaline silicate certainly known to be crystallizable, contains two Berzelian equivalents of silicic acid, SiO_3, to three equivalents of soda. Respecting the amount of water in this salt, analysts do not agree; and as the several observers have examined crystals obtained by unlike methods, there may possibly be three or more hydrates producible under different conditions of temperature, concentration, and purity.

Fritzsche[9] produced a salt in the form of square prisms, by dissolving in caustic soda lye a quantity of silica equal to that of the dry soda present, and allowing the solution to rest several days. The bruised crystals freed from mother liquor by pressure between folds of absorbent paper, gave him amounts of silica and chlorid of sodium corresponding to $NaO.SiO_2.9HO$. These crystals melted at about 40° C. Kept over sulphuric acid they in time effloresced (verwittert) to the center.

Fritzsche says that he once chanced to obtain globular masses studded with minute crystals and having the composition $NaO.SiO_2.6HO$; but he had never succeeded in determining the conditions of their formation.

Yorke[10] fused 23 parts of sand with 54 parts of dry carbonate of soda, dissolved the resulting mass in water, and exposed the solution to slow evaporation *in vacuo*, over sulphuric acid. The rough salt was recrystallized *in vacuo*, to get rid of the excess of carbonate, and the pure product was found to have the composition $NaO.SiO_2.7HO$. He also obtained crystals of the same composition from a solution of silica in caustic soda.

Yorke found these crystals to part with all their water, except a fraction of one per cent, by exposure to a heat of 149° C.

Hausmann[11] says that in the purification of rough soda, the mother liquor has often yielded him, in large quantity (manchmal centnerweise), rhombohedral crystals of silicate, permanent in the air, and having the the composition $NaO.SiO_2.8HO$.

Ammon[12] also obtained the octohydrate in the form of monoclinic crystals, by dissolving silica in soda lye. They fused at 45° C.

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[9] Pogg. Ann., xliii, 135.

[10] Phil. Trans., 1857, p. 533.

[11] Erdmann's Journal, xii, 294.

[12] Will and Kopp's Jahresbericht, 1862, p. 138.

I have found the same melting point, and have made several analyses which invariably go to confirm the formula given by Hausmann and by Ammon. Thus in one case the pure, carefully dried crystals, by suitable treatment with nitric acid, gave 22·74 p. c. of SiO_2, and nitrate corresponding to 23·66 p. c. of NaO. Another portion of the same sample being heated with fused bichromate of potash, lost 54·13 p. c.

The percentages answering to $NaO.SiO_2.8HO$, would be 22·55 of SiO_2, 23·31 of NaO, and 54·14 of HO.

The pure salt may be very easily made with the dense liquid which alcohol throws down from a cleansed solution of the commercial silicate,[18] $\dot{\mathrm{Na}}\dddot{\mathrm{Si}}_{2\cdot25}$. When such a precipitate is stirred up with an equal weight of caustic soda liquor of sp. gr. 1·32 considerable heat is evolved, and if the mixture is set in a cold place, it begins to crystallize in a few hours. The first crop should be dissolved in a little water and recrystallized. We thus get a clear, hard salt permanent, except in presence of carbonic acid. And to avoid the action of this gas existing in the atmosphere, it is well to dry the broken up crystals in a closed space over lumps of caustic soda or lime, and keep them in a tightly stopped bottle. But as in cold weather both the formation of the rough salt and its recrystallization may be carried out in forty-eight hours or less, the absorption of carbonic acid by the dense solution, is too slight to affect the purity of the crystals deposited. Crystallization *in vacuo* is a needless refinement; and spontaneous evaporation is in any case unnecessary, for as long as the alkali is slightly in excess, a weak solution can just as well be rapidly concentrated by heat.

Ammon attempted in vain to crystallize the potash salt and to form a double silicate of potash and soda.

All my own efforts to produce crystals of potash silicate have thus far proved unavailing, though I have exposed very concentrated solutions, for many days, to the extreme cold of winter.

Berzelius fixed on SiO_3 as the constitution of silicic acid, principally on account of an assumed analogy between feldspar and alum; and there would be some show of reason for such an assumption, if feldspars contained twenty-four equivalents of water and were the only known combinations of protosilicates with sesquisilicates. But the numerous well characterized siliceous minerals are an intractable set of compounds, whatever theory we adopt, and any formula for silica which gives conformity to one class, makes other species altogether anomalous. Why should we slight the plain guidance of the simple silicates and selecting feldspar,—a complex, anhydrous combination,—presume on its resemblance to a double sulphate which crystallizes only as a hydrate? Why should we pay so little regard to analogy as to write $\dot{\mathrm{Na}}_3\mathrm{Si}_2\dot{\mathrm{H}}_{24}$ and $\dot{\mathrm{Ca}}_3\mathrm{Si}_2$ for substances as definite as crystallization can make them, when soda and lime are not known to form any other unquestionable basic salts? If we attribute to silica a composition similar to that of carbonic acid, the monosilicates $\dot{\mathrm{Ca}}\ddot{\mathrm{Si}}$, $\dot{\mathrm{Mg}}\ddot{\mathrm{Si}}$, $\dot{\mathrm{Mn}}\ddot{\mathrm{Si}}$, $\dot{\mathrm{Cu}}\ddot{\mathrm{Si}}$, and the basic silicates $\dot{\mathrm{Mg}}_2\ddot{\mathrm{Si}}$, $\dot{\mathrm{Zn}}_2\ddot{\mathrm{Si}}$, $\dot{\mathrm{Mn}}_2\ddot{\mathrm{Si}}$, and $\dot{\mathrm{Cu}}_2\ddot{\mathrm{Si}}$, will be no longer unparalleled; and though some minerals will show a composition unlike that of artificial salts, most of the double silicates will have far less strange looking formulas.

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[18] See Part III, this Journal, xxxiii, p. 34. In the 18th line from the bottom of that page, it should read, "add to the filtrate two parts of alcohol," instead of "twenty parts."

In the gum-like compounds of acid and base, crystallization can take place only when the colloid constituent is reduced to the normal quantity. Thus the sesquinitrates of glucina, iron, and chrome may form crystals when there is a full amount of acid present; but an excess of base utterly prevents crystallization. We have seen also that the stannates can remain stannates only while there is no lack of alkali. Now if we add caustic soda to waterglass, the mixture becomes capable of affording crystals just so soon as the quantity of soda very slightly exceeds that of silicic acid, and no sooner. Fair analogical reasoning would lead to the belief that then, and not till then, had we come to the proportions of the normal salt.

Thermic Relations of Waterglass.—Another argument for the formula $\mathrm{SiO_2}$, though not of itself a perfectly conclusive one, may be drawn from the behavior of waterglass when mixed with caustic alkali. Taking a ten per cent solution of soda, which is too weak to become sensibly warm by mere dilution, if we add half an equivalent to an equivalent of $\mathrm{NaO\,2SiO_2}$, the temperature immediately rises. Another half equivalent of alkali causes the evolution of somewhat less heat than before. A farther addition of soda, is not, indeed, entirely without effect,—as Thomsen[14] also observed with respect to silicic acid as well as phosphoric and boracic acids,—yet the elevation of temperature is now so very slight that we must consider the silicate as having already reached the composition of the normal salt. As $\mathrm{NaO\,SiO_3}$ shows a capability of evolving much heat with soda, it must be an acid salt, and that cannot be its true constitution. Trials of mixture were made with two thermometers graduated to fifths of a degree Fahrenheit.

31. 110 g. of a solution containing 47 p. c. of $\mathrm{NaO\,.\,2{\cdot}37SiO_2}$ at 64·4° F., were mixed with 212 g. of a 10 p. c. caustic soda liquor at 64·2°, so as to make $\mathrm{NaO\,.\,SiO_2}$. The thermometer was raised to 71·4°, making a rise of 7·3° F.

32 *a.* 110 g. of 47 p. c. $\mathrm{NaO\,.\,2{\cdot}4SiO_2}$ at 61·5° F. were mixed with 90 g. of 10 p. c. NaO at 61·7° to form $\mathrm{NaO\,.\,1{\cdot}5SiO_2}$ or $\mathrm{NaO\,.\,SiO_3}$. The thermometer rose to 71·4°, making an increase of 9·8° F.

b. The 200 g. of *a* at 65·2° F. were mixed with 122 g. of 10 p. c.

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[14] Pogg. Annalen, xci, 90, 91.

NaO at 64·9°, so as to produce $NaO.Si_2$. The thermometer then stood at 67·3°, making a rise of 2·2° F.

c. Some of the neutral silicate of *b* at 64·6° F. was mixed with half its weight of a 10 p. c. soda solution at 64·9°. It rose to 65·4°, making an elevation of 0·7° F.

Reckoning the increase in each case as affecting only the original 110 g. of silicate, the rise in 30 would be 21·37° F. In 32 *a*, it would be 17·82°, and in *b* it would amount to 3·54°, or in both together 21·36°.

In 32 *c* it would be only 1·05°.

This heating by no means arises from simple condensation, for dilution of the same waterglass with mere water, causes even more contraction. Thus:

33 *a.* A narrow necked 100 c. c. bottle containing 50·616 g. of 47 p. c. $NaO.2{\cdot}4SiO_2$ of sp. gr. 1·558, was cautiously filled up with water. It was then closed and well agitated and there was a striking decrease of volume. Water was added once or twice with farther agitation till it stood at the right level at the normal temperature. 68·88 g. of water had been put in, and the sp. gr. was now 1·474. Calculation shows that instead of 100 c. c., it should have measured 101·36 c. c. So the contraction amounted to 1·34 p. c.

b. 110 g. of the 47 p. c. waterglass were mixed with 90 g. of caustic soda liquor of sp. gr. 1·164. The sp. gr. was now 1·363, and therefore the contraction amounted to 0·8 p. c.

c. The liquor of *b* being farther diluted with 125·2 g. of caustic soda liquor, the whole bulk was diminished 0·5 p. c.

The total contraction in both mixings amounted to 0·95 p. c. of what the aggregate volume should have been.

Though condensation results from the dilution of waterglass with water, it is a remarkable fact, and, as far as I can learn, one that has hitherto remained unnoticed, that there is at the same time a positive reduction of temperature.

34. 110 g. of 47 p. c. of $NaO.2{\cdot}4SiO_2$ at 63·6° F., were mixed with 90 g. of water at 63·4°. The temperature fell to 61·9°, making a fall of 1·6° F.

35 *a.* 100 g. of 47 p. c. $NaO.2{\cdot}4SiO_2$ at 66·2° F. were mixed with 50 g. of water at 66°. It got down to 65·1°, making a fall of 1° F.

b. These 150 g. cooled to 60·1° were mixed with 50 g. of water at 60°. The thermometer then stood at 59·2°, making a fall of 0·87° F.

c. The 200 g. of *b* at 61·3° were mixed with 50 g. of water at 61·4°. It fell to 60·85 making a fall of 0·45° F.

Bringing all to the same standard by reckoning the reduction of temperature as concentrated in 100 g., the fall in *a* would be 1·5°; in *b*, 1·74°; in *c*, 1·12°; and in 34 it would be 2·9° F.

It needs a more extensive examination than has yet been made, to show whether this unaccountable behavior is peculiar to waterglass. Metastannate of potash is neither heated nor cooled by dilution. Nor is the treatment of the metastannate with caustic potash liquor attended with any more elevation of temperature than might be due to the precipitation which results.

This latter fact, as well as the slight solubility of metastannate in caustic potash, shows that metastannate of potash is very slow to combine with more alkali. Still farther illustrations of this point will be given when we come to treat of the silicates of lime. The so-called metastannate is therefore rightly named, and is not an acid salt nor an unsaturated stannate. And, by contrast, the great avidity with which waterglass unites with more base, goes to prove that the silica in it is not a metacid, but is of the same kind as that in the normal, crystallized silicate of soda.

Manchester, N. H., April 29.

ART. XIX.—*Speculations upon a possible method of determining the distance of certain variably colored Stars;* by JOSEPH WHARTON.

ARAGO conceived the idea of testing the correctness of the corpuscular or emission theory of light by subjecting two rays of different velocities to the same refracting influence; for, as that theory explained refraction to be due to retardation of velocity caused by the molecular attraction of the refracting medium, rays entering the medium at different velocities should be differently refracted. To get rays of different velocities, he took light from a star toward which the Earth in its orbit was moving, and from another star which the Earth was moving away from. Supposing the ray to strike the Earth from the first of these stars at the real velocity of light plus the velocity of the Earth, and that from the other at the real velocity of light minus that of the Earth, Arago had two rays entering his refractor at velocities differing by about $\frac{1}{5000}$. Arago found no difference in the refraction of his two rays, and his conclusion thence derived that the corpuscular theory of light is untenable, has been generally accepted; this experiment having in fact been frequently quoted as one of the props of the adverse, or undulatory, theory.

It seems, however, rather surprising that any great weight should be attached to an apparent disproval, by a single test, of one merely imaginary function of corpuscular light, especially as the test itself is utterly fallacious; for who shall say that retardation by attraction is the only possible means by which emitted light could be refracted? and how can we know that the two stars selected by Arago had either no proper motion of their own, or none of a sort to affect his result?

Perhaps the only cases in which we can be sure of receiving star-light of absolutely different velocities are those of such binary stars the plane of whose orbit is not at right angles with the line from thence to the Earth. When that line lies in the plane of such an orbit, and the two stars are situated at right angles to that line, it is clear that the velocity of the rays reaching us from one star exceeds that of the rays from the other by double the speed of those stars in their orbit. That light reaches us at various velocities from all the various stars is of course as certain as that they have proper motions, or that our solar system is moving through space; but the difficulties in the way of gaining any accurate comparison of those velocities are very great.

In reflecting upon the undulatory theory of light I have been quite unable to conceive how the luminiferous æther could "tremble laterally" as the phrase is, causing vibrations transverse to the line of propagation, without a direct relation existing between those lateral vibrations and the forward impulses by which the phenomena of light are translated. The æther being continuous and material and elastic, being in a word capable of sustaining a vibration, a vibratory impulse in it at right angles to the course of a ray of light seems fairly comparable to a lateral displacement of a point in a rope, or to the merely vertical vibration in a water wave not of translation, or to the vibration of air by a sound, all of which vibrations produce (we may almost say *are*) longitudinal undulations.

But if the lateral vibrations of the æther produce the onward propagation of the ray, then the number of lateral impulses in a second equals the number of forward impulses arriving in a second at a relatively stationary point, and as it is established that the number of lateral impulses varies according to the color of the light, so the number of forward impulses striking a relatively stationary point—say a retina—must vary with the color of the light; and if the source of light, instead of being at a constant distance, should rapidly approach the retina, the latter must receive a greater number of impulses per second, and its impression of color therefore must be correspondingly modified. If the retina and the source of light rapidly separate from each other, the number of impulses striking the retina must on the other hand be diminished, producing the corresponding change in the perceived color.

Now, if we imagine a star emitting white light to approach us in an orbital movement at a sufficient rate of speed, its light should appear to us reddish, changing at the perigee into white, changing again into blueish as the star departs, and again into white at the apogee. There are, however, variable stars whose colors undergo exactly those changes, viz: passing from one color to its complimentary, and back again, with periods of white light intervening. The binary stars, whose colors are frequently complimentary to each other, should, under the proper circumstances, exhibit the same circuit of change, but I am not prepared to say that they do so in any case: it is in fact asserted that the larger star is usually red and the smaller one blue.

Supposing this train of thought to be sound, and that by extremely careful observation a difference could be detected in the position of a variably colored star when it appears red, from its position when blue. we should then be upon the track to calculate its distance. We should need to know the differing rates of light impulses required to produce the observed colors, the time occupied by the star in passing from one extremity of its course to the other, and the angular distance between those extreme positions. Let us assume that such a variably colored star has been found, which has a measurable change of position in one line, in other words the plane of whose orbit coincides with our line of vision, and let us assume that its extreme colors indicate a difference in the rate of arrival of light impulses (or in other words a difference in the velocity of the light arriving from that star at the two periods), equal to $2v$, then the actual speed of the star in its orbit $=v$ and as the orbital period of the star has been found $=t$, it follows that $\frac{v \times t}{3{\cdot}1416}$ is the real length of that diameter which is the measured angular distance between the two extreme positions of the star. Knowing the angle, and the length of the base which subtends it, we have the distance of the star.

If a pair of binary stars could be found whose colors alternate, and which alternately eclipse each other, the matter would be simplified by so much as accurate measurement of the angle of parallax would be facilitated.

Should it be objected that no such binary stars have been observed, and that no change of position has been noticed in any single stars of variable color, I can only reply that possibly it may be worth while to direct attention to those points.

Should it be said that this hypothesis of the cause of variable color in stars would oblige us to believe that rays of different colors are propagated at different velocities, and that this whole suggestion is valueless until those velocities have been determined by direct experiment, I answer that very probably rays of different colors have different velocities, and that to determine them would be a most important achievement.

To conclude, I offer the analogy of the changing tone of a locomotive whistle as you rush past it on another train. Here, as you approach the sound, its impulses reach you more frequently than if its source, and you, were at rest. At the instant of passing, you receive the normal number of impulses, and after passing the impulses reach you less frequently. The shrill shriek, the real tone, and the low roar in this case are facts which I suppose to be parallel to the red, the white, and the blue light of a star moving swiftly, first toward, and then from us.

ART. XX.—*Contributions to the Chemistry of Natural Waters;* by T. STERRY HUNT: of the Geological Survey of Canada.

III.

Chemical and Geological Considerations.

CONTENTS OF SECTIONS.—52, salts of alkaline metals, proportion and sources of potash; 53, potash in a borax lake, in the primitive sea; 54, salts of lime and magnesia, relations of chlorids and carbonates; 55, solubility of earthy carbonates; 56, super-saturated solutions of carbonates of lime and magnesia; 57, salts of barium and strontium, solution of their sulphates; 58, iron, manganese, alumina and phosphates; 59, bromids and iodids; the small portion of bromine and the excess of iodine in saline springs as compared with the modern ocean; 60, probable relation of iodids to sediments; 61, sulphates, their elimination from waters; 62, water holding a soluble sulphuret; 63, borates, their detection and determination; 64, analysis of a borax water from California; 65, carbonates, their amounts in the Caledonia waters; 66, intervention of neutral carbonate of soda; 67, deficiency of carbonic acid in waters; 68, reactions of various waters; 69, silica, its source and its proportion; 70, its condition; formation of silicates; 71, organic matters; 72, geological position of the waters here described: 73, succession of Paleozoic strata; lithological relations of successive formations; 74, Quebec group, its waters; 75, sources of various classes of waters; 76, their relation to the formations; 77, association of unlike waters, changes in constitution; 78, temperature of springs, thermal waters; 79, geological interest of the above analyses; possible results of the evaporation of these springs; 80, relations of mineral springs to folding and to metamorphism of strata; 81, on the supposed origin of the primeval ocean and the earliest sediments; 82, on the theory of metalliferous deposits.

§ 52. *Salts of the Alkaline Metals.*—These salts abound in most saline waters, and except in the few cases in which sulphate of magnesia prevails, form a large part of the soluble matters present. The salts of sodium are by far the most abundant, and the proportion of potassium salt is generally small. The chlorid of potassium in modern sea-water constitutes three or four hundredths of the alkaline chlorids, while in the brines from old rocks, and in saline waters of the first two classes alike from Germany, England, the United States, and Canada, its proportion is much less, sometimes amounting to traces only. In the waters of classes III and IV, where alkaline carbonates appear, and even predominate, the proportion of potassium salt becomes greater. Thus of the waters of the latter class (§ 45), the alkalies of the Nicolet spring calculated as chlorids contain 1·89 per cent of chlorid of potassium, and those of the Jacques-Cartier 2·95; while for the St. Ours spring the chlorid of potassium is equal to not less than 25·0 per cent. There does not however appear to be any relation between the proportion of alkaline carbonate and that of potassium, since the salts from the waters first named are more alkaline than those of St. Ours; while those of the alkaline water of Joly contain less than one per cent of potassic chlorid.

The amount of this salt obtained from the water of the Ottawa River is worthy of notice, being equal to not less than 32·0 per cent of the alkaline chlorids, while in the waters of the St. Lawrence it amounts to 16·0 per cent.[1] A large proportion of potassium relatively to the sodium has already been observed in the case of many ordinary river and spring waters, and this is readily explained when we consider the extent to which potash is set free by the decomposition of both vegetal and mineral matters at the earth's surface. The process by which this base is eliminated in filtering through soils has already been explained in § 5. The occasional presence of considerable amounts of potash in sulphated mineral waters (Lersch, Hydro-chemie, p. 346) is explained by the power of solutions of gypsum to set free this alkali from soils (§ 7), and also probably in some cases by the dissolution of double potassic salts like polyhalite. Strata holding glauconite, which occurs alike in Paleozoic and more recent formations,[2] may also be conceived to yield potash salts to infiltrating waters.

§ 53. It will be seen that the waters above noticed, in which the proportion of the potash to the soda is large, are but feebly saline, so that the real amount of potassium is in no case great. I have, however, recently examined the water of a borax lake from California, which contains in 1000 parts 17·250 of solid matters, of which 1·818 is carbonate of potash, the remainder being soda-salts, carbonate, borate, chlorid, and a little silicate, with no sulphate (§ 64). This amount, if represented as chlorid of potassium, is equal to 1·963, or 11·46 per cent of the alkalies calculated as chlorids. The amount of potassium salt in this water is consequently about forty times greater than in that of St. Ours.

The fact of special importance as regards the alkaline metals in the waters whose analyses we have given in this paper is the very small amount of potassium in the strongly saline muriated waters of the first three classes, which we conceive to be more or less directly derived from the waters of the ancient ocean. To this primeval sea, almost destitute of potassium, the process of mineral decay has been for ages adding potash salts, and despite the partial elimination of these by vegetation (§ 5), and by the formation of glauconite, we find a notable proportion of potash in the waters of the modern ocean.

In the analyses of the saline waters here given lithia was sought for in a few instances, and was detected in the waters of Varennes. Most of these analyses were made before the discovery of the new metals cæsium and rubidium.

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[1] T. S. Hunt, (L., E. and D. Phil. Mag., [4], xiii, 239); and Geology of Canada, page 565.

[2] For a notice, with analyses by the author, of a green hydrated silicate of alumina, iron and potash, allied to glauconite, from the Paleozoic rocks of Canada and of the Mississippi valley, see the Geology of Canada, pages 487, 488; where also will be found an analysis by the author of the glauconite from the Cretaceous formation of New Jersey. See also this Journal, [2], xxxiii, 277.

§ 54. *Salts of Calcium and Magnesium.*—We have to consider under this head the relations both of the chlorids and the carbonates of these bases. The bitter saline waters of the first class, although containing large quantities of chlorids of calcium and magnesium, are, as we have seen, generally destitute of earthy carbonates. These latter, however, are found in small quantities in the alkaline waters of the fourth class, and in somewhat larger amounts in those intermediate waters which form classes II and III, and are apparently formed by admixtures of the two classes previously mentioned. Besides the carbonates of lime and magnesia which the waters of the fourth class hold in solution, the carbonate of soda which they contain gives rise, by its reaction with the chlorids of calcium and magnesium, to additional quantities of the carbonates of these bases. In the waters of Kingston (§ 36), a large amount of chlorid of calcium is associated with earthy carbonates, and these waters thus offer a passage from the first to the second class.

In most of the waters of the second class, as will be seen from the table § 42, there appears but a small amount of chlorid of calcium; and even this depends upon the manner in which the analysis has been conducted. We may suppose in the recent water such a partition of bases between the chlorine and the carbonic acid that chlorid of calcium, chlorid of magnesium, bicarbonate of lime and bicarbonate of magnesia co-exist. When such a solution is submitted to evaporation at ordinary temperatures, provided there is present a sufficient amount of chlorid of calcium, carbonate of lime alone is deposited, and chlorid of magnesium remains in solution.

In case the chlorid of calcium is insufficient, the lime is still first deposited as carbonate, and the more soluble magnesian carbonate is precipitated by further evaporation. When however such a water is boiled, a reverse process takes place; the carbonate of lime slowly decomposes the magnesian chlorid, and carbonate of magnesia is deposited, while chlorid of calcium remains in solution. Hence if the amount of chlorid of magnesium be great enough, and the ebullition sufficiently prolonged, the precipitate will at length contain only carbonate of magnesia; while an equivalent of chlorid of calcium, now found in the solution, represents the carbonate of lime which the analysis of the precipitate at an earlier stage of the ebullition would have furnished.

As an example of this may be cited the analysis of the water of Ste. Genevieve (§ 42, No. 8), where the precipitate after a few minutes' boiling contained carbonates of lime and magnesia in the proportion 12 : 750. When however another portion was boiled down to one-sixth, the precipitate was found to be pure carbonate of magnesia. Again, the Plantagenet water gives, by ebullition, the results set forth in § 42, No. 1; showing chiefly carbonate of magnesia, together with a portion of chlorid of calcium. When however this water is left to spontaneous evaporation, the whole of the lime separates as carbonate, and the liquid remains for a time charged with carbonate of magnesia, probably as sesqui-carbonate. This solution is, however, after a time spontaneously decomposed even in closed vessels, with deposition of a portion of crystalline hydrated carbonate of magnesia; another portion remains in solution, together with chlorid of magnesium, but is precipitated by ebullition. (This Journal [2], xxvii, 173.)

§ 55. Bicarbonate of magnesia and chlorid of calcium, when brought together in solution, undergo mutual decomposition with separation of carbonate of lime if the solutions are not too dilute. At the ordinary temperature and pressure, water saturated with carbonic acid will not hold in more than about one gram of carbonate of lime to the liter (1 : 1000); equal to only 0·88 grams of carbonate of magnesia. (The solubility of carbonate of lime in pure water is well known to be much less, and is, according to Bineau, equal to 1 : 30,000 or 1 : 50,000.) We should not therefore expect to find that water holding chlorid of calcium in solution would yield, by boiling, more than the latter amount of magnesian carbonate; so much might evidently be formed by the action of dissolved carbonate of lime which the water might hold as bicarbonate. I have elsewhere described a series of experiments on the solubility of bicarbonate of lime both in pure water and in saline solutions, and have shown that the presence of salts of soda, lime and magnesia does not increase the amount of bicarbonate of lime which water is capable of holding *permanently* in solution. In view of these facts it seems at first sight difficult to explain how a mineral water like that of Kingston (§ 36, No. 7), holding a large quantity of chlorid of calcium, could yield, as appears from Dr. Williamson's analysis, 1·287 grams of carbonate of magnesia, equal to 1·462 of carbonate of lime to the liter. Recent experiments have however shown me that supersaturated solutions of a certain stability may be obtained, in which comparatively large quantities of neutral carbonates of lime and magnesia exist in the presence of sulphates and chlorids of calcium and magnesium. Reserving for another occasion a description of the details of these investigations, I shall briefly state the results obtained.

§ 56. In a memoir on the salts of lime and magnesia published in 1859 (in this Journal [2], xxviii, 171), it was shown that by the addition of bicarbonate of soda to a solution holding chlorids of sodium, calcium and magnesium, with or without sulphate of soda, and saturated with carbonic acid, it was possible to obtain transparent solutions holding from 3·40 to 4·16 grams of carbonate of lime to the liter; of which however the greater part was deposited after twenty-four hours; when the solutions were found to contain somewhat less than 1·0 gram in the form of bicarbonate. Boutron and Boudet had previously shown that by saturating lime-water with carbonic acid, solutions were obtained holding in a liter 2·3 grams of carbonate of lime; of which one half was soon deposited, even when the solution was kept under a pressure of several atmospheres. It would thus seem that saline liquids favor this temporary solubility of the carbonate of lime.

In all of the above experiments an excess of carbonic acid was present, but this I have since found is not essential, since super-saturated solutions may be obtained holding as much as 1·2 grams of carbonate of lime, together with sulphate of magnesium and chlorid of calcium, in a liter of water, without any excess of carbonic acid. The power of alkaline chlorids and of chlorid of calcium to prevent the precipitation of chlorid of calcium by carbonate of soda has already been observed by Storer, (Dictionary of Solubilities, p. 110). I have found that the precipitate produced by the admixture of solutions of these two salts is readily dissolved, when recent, by a solution of chlorid of calcium, or of sulphate of magnesia; and thus liquids may be prepared holding at the same time from 1·0 to 1·2 grams of neutral carbonate of lime and 1·0 of neutral carbonate of magnesia in presence of sulphate of magnesia. These solutions of carbonate of lime, which are strongly alkaline, may be kept for twelve hours or more without perceptible change at ordinary temperatures, but after a time deposit crystals of hydrated carbonate of lime. The addition of alcohol immediately throws down the whole of the carbonate of lime in an amorphous condition.

The carbonate of magnesia is still more soluble than the carbonate of lime under similar conditions, and it is possible to obtain 5·0 grams of neutral carbonate of magnesia dissolved in a liter of water holding seven per cent of hydrated sulphate of magnesia, without any carbonic acid. These solutions, which are strongly alkaline to test papers, yield a precipitate by heat, which re-dissolves on cooling.

It is evident that the mingling of saline and alkaline waters may give rise to solutions like those just described, and thus explain apparent anomalies in composition like that of the Kingston water. See also in this connection the observations of Bineau, and my own on the properties of solutions of sesquicarbonate of magnesia. (This Journal [2], xxvii, 173.)

§ 57. *Salts of Barium and Strontium.*—As will be seen from the preceding tables, the salts of these two bases are found in very many of the saline and alkaline waters of Canada. Their carbonates probably sustain to the magnesian chlorid a similar relation with that of calcium, and hence these bases appear in some of the analyses partly as carbonates, and partly as chlorids of barium and strontium. The precipitate formed in the concentrated and acidulated water by dilute sulphuric acid was, whenever submitted to analysis, found to contain both barium and strontium. For the separation of these the mixed sulphates were first converted into chlorids; the barium was then thrown down as silico-fluorid, and the strontium subsequently precipitated by a solution of gypsum.

The insolubility of its sulphate must have excluded baryta from the waters of the primeval sea, and when set free, as we may suppose by the decomposition of its silicated compounds existing in the primitive crust, (§ 81) its soluble bicarbonate carried down to the sea would there be precipitated by the sulphates present. A similar process must still go on with all the dissolved barytic salts which find their way to the ocean.

The sulphate of baryta thus accumulated in sedimentary strata, may be partially decomposed by infiltrating solutions of alkaline carbonates, and thus be rendered capable of being subsequently dissolved as carbonate; but the most probable mode of its solution, is, we conceive, through its previous reduction by organic matters to the form of a soluble sulphuret (§ 10), ready to be converted into carbonate or chlorid of barium. In this way we may explain the frequent occurrence of baryta salts in the saline waters of the first three classes, and the consequent absence of sulphates, which will be further considered in § 61. From the similarity of its chemical reactions, the preceding remarks apply to strontia as well as baryta.

§ 58. *Iron, Manganese, Alumina, and Phosphates.*—None of the waters of the four classes here described contain any notable quantity of iron, yet this element is never wanting in those waters which contain earthy carbonates. Whenever a portion of one of these waters, or, better, the earthy precipitate separated from it by boiling, is evaporated to dryness with an excess of hydrochloric acid, the residue, treated with acidulated water, yields a portion of silica, and the solution will then be found to yield with ammonia a precipitate. This, which is partially soluble in caustic alkalies, is often colorless, and will be found to consist of alumina and peroxyd of iron, with phosphoric acid and a trace of manganese, which latter metal is seldom or never absent. The small quantity of alumina which these waters contain appears not to be derived from suspended argillaceous matters, but to be held in a state of solution.

The phosphates are generally present only in very small quantities in these waters, for the reason pointed out in § 5. The largest amount which I have met with was in an alkaline water from Fitzroy (§ 43, No. 4); where it was equal to ·0124 of tribasic phosphate of soda in 1000 parts of water.

§ 59. *Bromids and Iodids.*—The chlorids in these ancient mineral waters are always accompanied by bromids and iodids; but the proportion of the bromids to the chlorids appears to be much less than in the waters of the modern seas. According to Usiglio, 100 parts of the salts from the Mediterranean contain 1·48 of bromid of sodium; while ten analyses by von Bibra of the waters of different oceans, give from 0·86 to 1·46, affording for 100 parts of salts a mean of 1·16 of bromid of sodium, equal to 1·04 parts of bromid of magnesium. The waters of Whitby and Hallowell, on the contrary, which are the richest in bromids of those described in this paper, contain only 0·54 and 0·69 parts of bromid of sodium in 100 parts of solid matters; while few of the saline springs of the second class contain more than one-half of this proportion, and some of them very much less.

With regard to the iodids in many of these waters, however, the case is very different. The waters of the modern ocean, as is well known, contain but traces of iodine, and in some strongly saline springs of the first class, like that of Whitby, it is only in the alcoholic extract of the salts from this water that iodine can be detected. The Hallowell water (§ 36, No. 3), which closely resembles this in its general composition, and in the proportion of bromids, is however so rich in iodine that its presence can readily be discovered without previous evaporation. It is sufficient to add to the recent water acidulated by hydrochloric acid a little solution of starch and a few drops of nitrite of potash to produce an intense blue color. The iodid of sodium in the first-named water was found equal to 0·0017 parts of the solid matters, and that of the second to 0·019, or nearly twelve times as much. The unconcentrated saline waters of Ste. Genevieve, of the second class also give a strong reaction for iodine; and when acidulated with hydrochloric acid, without previous evaporation, yield with a salt of palladium an insoluble precipitate of iodid of palladium after a few hours. The salts from these two springs of Ste. Genevieve, though poorer in bromids, are much richer in iodids than the waters of Hallowell; the spring No. 8, containing in 100 parts of salt no less than 0·138 of iodine; so that there appears to be no constant proportion between the chlorids, bromids, and iodids of these saline waters.

§ 60. The relations of bromids and iodids to argillaceous sediments have yet to be determined. It would appear from the facts just cited that bromine has in the course of ages been slowly eliminated from insoluble combinations, and like potassium, has accumulated in the waters of the ocean; while the facts in the history of iodine seem to point to a process the reverse of this; in other words to a gradual elimination of iodine from the sea-waters, and its fixation in the earth's crust. The observations of numerous chemists unite to show the frequent occurence of small portions of iodine in some unknown combination, in sedimentary rocks of various kinds; from which we may conjecture that it was in former times abstracted from the sea, either directly or through the intervention of organic bodies (as in the case of potash, which is separated and fixed by means of algæ, § 5). Experiments after the manner of those of Way and Voelcker may throw light upon this interesting question. We are aware that insoluble combinations of soluble chlorids with silicates of alumina are found under certain conditions, as appears in the generation of sodalite, eudialyte, and the chloriferous micas, and it is not improbable that the soluble iodids may give rise to similar compounds. By such a process might be explained the rarity of this element in modern seas, while the occasional re-solution of the iodine from these insoluble compounds by infiltrating waters, would help to explain the variable and often large proportions in which this element is met with in some of the waters noticed above.

§ 61. *Sulphates.*—In the preceding sections we have already discussed the principal facts in the history of those neutral waters in which sulphates predominate, or prevail to the exclusion of chlorids (§ 50, 51). The history and the probable origin of those curious springs which contain free sulphuric acid has also been considered (§ 31, 48, 49); and it now remains to notice the relation of sulphates to the muriated waters. The first fact that excites our attention is that of the total absence of sulphates from numerous springs of the first, second and third classes; as shown in the preceding analyses, and also in the observations of Lenny and others on the saline waters over a great area in Western Pennsylvania (§ 40).

The elimination of sulphate in the form of gypsum from evaporating waters containing an excess of chlorid of calcium has already been discussed in § 37; but the bitterns resulting from such a process still retain small portions of sulphates, while it is to be remarked that the saline waters under consideration contain no traces of sulphates, and in many instances hold portions of baryta and strontia, bases incompatible with the presence of sulphates. The modes in which this complete elimination of sulphates may be effected are two in number. The first has already been suggested in § 10, and depends upon the deoxydizing power of organic matters, which reduces the sulphates to sulphurets. These in their turn may be converted into carbonates, the sulphur being separated either as sulphuretted hydrogen (giving rise by oxydation to free sulphur), or as insoluble metallic sulphurets. This reducing action not only decomposes the soluble sulphates of soda, lime and magnesia, but also, as has been pointed out in § 57, may extend to sulphate of baryta, and thus sulphuret or carbonate of baryta be formed. It is the action of these soluble baryta salts which constitutes the second mode of desulphatizing waters; and this, if we may judge from the frequence with which baryta salts occur in the saline waters in question, appears to have been the most general process.

It is a fact worthy of notice that a saline spring at Sabrevois, near Lake Champlain, which holds both baryta and strontia in solution, is at the same time slightly impregnated with sulphuretted hydrogen. Another saline and sulphurous spring, which rises within ten feet of this, contains, however, a portion of sulphates. (Geology of Canada, page 542.)

§ 62. I am indebted to Prof. Croft of Toronto, for some notes of a recent examination by himself of a saline of the first class, which contains at the same time a soluble sulphuret. This water, from a boring sunk to a depth of several hundred feet through the Devonian limestone at Chatham, Canada West, had a specific gravity of 1039·3, and yielded for a thousand parts about 51 of solid matters. It contained large portions of chlorid of calcium and magnesium, with very little sulphate, traces of carbonate, and no free carbonic acid. The water, which gave an alkaline reaction with turmeric, was greenish in color, very sulphurous to the taste, and yielded a purple color with nitro-prussid of sodium, and a black precipitate of sulphuret with a solution of sulphate of iron. A current of carbonic acid rendered the recent water opalescent, and by exposure to the air it deposited sulphur. A quantitative analysis of this water is to be desired.

§ 63. *Borates.*—The reddening of the yellow color of turmeric paper in presence of free hydrochloric acid, affords, with certain precautions, the ordinary means for detecting small portions of boric acid. Most of the waters of the third and fourth classes, and some of those of the second, have been tested in this way, and have never failed, when reduced to a small volume, and acidulated with hydrochloric acid, to give this reaction; which was, however, most marked with the waters of the fourth class. The determination of the amount of boric acid in saline waters presents no small difficulty. In the case of the alkaline water of Joly (§ 45, No. 3) the following process was, however, attempted. The salts left by its evaporation were treated with carbonate of ammonia to separate a portion of silica, and then with recently precipitated carbonate of silver, by which the alkaline chlorids were converted into carbonates. The solution now retained in some undetermined form a portion of silver, which was separated by fusing the evaporated saline residue in a silver crucible. By a second evaporation and fusion there was obtained a mixture of soda and potash, combined only with carbonic, sulphuric and boric acids. By directly determining the other ingredients the boric acid was estimated from the loss, and was found equal to 0·028 parts in 1000 of water, which contained 0·752 of solid matters. The conversion into carbonate of the sulphates in the mixed salts, by the aid of bicarbonate of baryta, would simplify this process. In § 35 it has been explained that the amount of carbonate of soda in the waters of the third and fourth classes was generally calculated from the excess of the alkaline bases, and controlled by the amount of carbonate of baryta precipitated from chlorid of barium by the alkaline salt. It was found, however, that this last method always presented a certain deficit, due to the borate of soda, whose quantity in many of the waters, is too large to be disregarded. The precipitate of carbonate of baryta contained a portion of sparingly soluble borate of baryta, which was not completely removed by long and continued washing.

§ 64. I have recently had an opportunity of examining the water of a borax lake from California, which contains, beside borate and carbonate of soda, a portion of chlorid, and a little silicate, traces only of phosphate, and no sulphate. It held in solution very small quantities of earthy carbonate, and was remarkable for a large proportion of potash, already referred to in § 53. The evaporated and fused saline residue was treated by the ordinary methods for the determination of the chlorine, carbonic acid and silica; while the bases were obtained in the form of sulphates by the aid of sulphuric and hydrofluoric acids, and afterwards separated as chlorids by the aid of chlorid of platinum. From the data thus obtained the following ingredients were found by calculation for 1,000 parts of the water:

Carbonate of soda, - - - - - - - - -	9·476
Biborate of soda, - - - - - - - -	4·395
Chlorid of sodium, - - - - - - - - -	1·702
Carbonate of potash, - - - - - - - -	1·818
Silica, - - - - - - - - - - -	0·129
	17·520

The potasium as above determined, equals 11·46 per cent of the bases weighed as chlorids; another trial gave 11·41. Although for convenience we have represented the potassium as carbonate, it will be seen that the amount of chlorine is such that it might, for the greater part, have been represented as chlorid of potassium, with an equivalent portion additional of carbonate of soda.

§ 65. *Carbonates.*—In describing in § 43 the alkaline-saline waters of Caledonia it has been shown that these contained a quantity of carbonic acid insufficient to form bicarbonates with the carbonated bases present. It was partly with this fact in view that after an interval of more than seventeen years I undertook the new analyses of these waters which in § 47 are given side by side with the earlier results. In these recent analyses, as there remarked, a slight excess of carbonic acid was met with. In the interval the springs had undergone a change in composition. And while the third one still retained in a slight degree its alkaline character, the other two had become waters of the second class, holding, instead of carbonate and sulphate of soda, chlorid of magnesium and baryta salts. The amount of carbonic acid had, however, undergone but little change; and, as will be seen by comparing the figures below with those in the table in § 47, the slight diminution in the first and third corresponds very closely with the falling off in the amount of solid matters between 1847 and 1865; while, on the contrary, the augmentation in the amount of carbonic acid in the second is accompanied with a corresponding increase in the amount of fixed matters present.

Carbonic acid in one liter of the Caledonian waters.

	1847.	1865.
Gas spring, - - - - -	·705 gram.	·671 gram.
Saline spring, - - - - -	·648 "	·664 "
Sulphur spring, - - - -	·590 "	·573 "

While the amounts of fixed matters and of carbonic acid in the several waters have undergone but little change, we find, however, that there has been a great diminution in the proportion of carbonated bases. Thus in the Gas spring in 1847 the carbonic acid required for the neutral carbonates found in the analysis was ·356, while for the same water in 1865 only ·278 of carbonic acid was required. In the Sulphur spring, in like manner, the neutral carbonates required ·449,[1] or more than three-fourths of the carbonic acid present; while the falling off of the amount of carbonates in 1865 is such that only ·191 of carbonic acid, or just about one-third of the carbonic acid present, is required for the neutral carbonate. Nor is this change due entirely to a less amount of carbonate of soda; the carbonates of lime and magnesia in 1847 required ·246, and in 1865 only ·153 of carbonic acid. The changed conditions which we here meet with may be explained by supposing that the carbonated bases are due to the mingling in different proportions of neutral carbonate of soda (generated by the reaction indicated in § 13,) with an earthy saline water holding a constant amount of free carbonic acid; which, in some cases, is more than is required to form bicarbonates, but in others, as we have seen above, shows a deficiency.

§ 66. If we admit, as I have already assumed, that the waters of the second and third classes have been generated by the mingling of solutions of carbonate of soda with waters of the first class, it can readily be shown that these solutions contained chiefly or exclusively the neutral carbonate. If we add a solution of bicarbonate of soda to earthy saline waters of the first class it is easy to obtain solutions holding twenty grams or more of bicarbonate of magnesia to the liter; while in none of the natural waters of the second class do our analyses show the existence of much over one gram to the liter. Again, if we suppose any considerable amount of chlorid of calcium to be decomposed by bicarbonate of soda, separation of the lime in the form of neutral carbonate, and the liberation of the second equivalent of carbonic acid, would yield waters holding an excess of carbonic acid above that required to form the bicarbonates of the solution. From the absence of such an excess, as appears in the case of the waters of Caledonia, Varennes, St. Leon and Caxton, and from the small amount of bicarbonate of magnesia in these waters, it may be concluded that the alkaline salt whose addition has changed their character was the neutral carbonate of soda.

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[1] By mistake this is printed ·349 in § 43.

§ 67. Examples are not wanting of waters in which, as in those of Caledonia in 1847, the carbonic acid is insufficient to form bicarbonates, or even neutral carbonates, with the bases uncombined with sulphuric acid or chlorine. Thus, according to Pagenstecher and Müller, the spring and well-waters of Berne do not contain sufficient carbonic acid for the lime present, a part of which they suppose to be held in solution as a silicate. See Bischof, Chem. Geology, i, 5; who remarks that Löwig seems to have observed the same fact in the thermal spring of Pfäffers. For further examples of this kind see Lersch, Hydro-Chemie, page 333. The carbonic acid in the water of Töplitz, is according to him, not sufficient to form bicarbonates unless the silica present be supposed to be combined with a portion of bases; while in the alkaline thermal spring of Bertrich, according to the analysis of Mohr, a similar deficiency of carbonic acid exists; leading to the conclusion that a part of the earthy bases present is in combination with silica and organic matters. The existence of solutions holding comparatively a large amount of neutral carbonates of lime and magnesia, as described in §56, is not without interest in this connection; since it at once affords an explanation of the nature and origin of all such alkaline waters, and waters deficient in carbonic acid, as contain earthy sulphates and chlorids.

§ 68. It was found that the waters of Chambly in 1864, and of the Sulphur spring of Caledonia in 1865, gave with lime-water a precipitate which was soluble in an excess of these mineral waters, but to a much less extent than in the acidulous saline water from the High-Rock springs of Saratoga. The latter, which contains bicarbonate of soda, and is highly charged with carbonic acid, turns to a wine-red the blue color of litmus tincture, which is not changed by the Chambly or the Caledonia water. The Saratoga water, after some time, gives a feeble alkaline reaction with dahlia paper; this is more distinctly but slowly changed by the Caledonia water, and almost immediately turned to green by that of Chambly. This latter water readily browns yellow turmeric paper, which is scarcely affected by the water of Caledonia.

§ 69. *Silica.*—The silica which exists in solution in cold saline springs is generally very small in amount, as might be expected from the insolubility of earthy silicates, which is such that superficial drainage waters in filtering through the soil lose the silica which they held in solution (§ 5). We have further shown that, as a result of this tendency to the formation of insoluble silicates, the silicate of soda liberated in the sediments by the decomposition of feldspar, generally appears at the surface as carbonate of soda, having been decomposed by earthy carbonates (§ 13).

In two cases, however, considerable quantities of silica are found dissolved in natural waters. The first is met with where the rapid solvent and decomposing action of heated waters is exerted upon alkaliferous siliceous minerals (§ 14), as seen in springs like the Geysers. The second case is that of those rivers and streams which drain surfaces covered with decaying vegetation and decomposing silicates, from both of which they derive dissolved silica. Such waters contain but small amounts of solid matters, but the proportion of silica is relatively considerable, amounting, as we have seen in the water of the Ottawa River, (which contains in 10,000 parts, 0·6116 of solid matters), to 0·2060, or thirty-two per cent; while in the St. Lawrence, (which contains for the same amount of water, 1·6056,) the silica equals ·3700, or twenty-four per cent of the solid ingredients. The analysis by H. Deville of the river-waters of France show, in like manner, large amounts of silica, which seem to have been hitherto overlooked in the analyses of most chemists. (Ann. de Chim. et de Phys., [3] xxiii, 32.

It will be seen by a reference to the tables of analyses given in the second part of this paper, that in the waters of the second class the amount of silica is equal to from 0·15 to 0·60 parts for 100·00 solid matter. In the alkaline waters of the third and fourth classes its proportion is greater, and up to a certain point appears to increase with that of the carbonate of soda. In the following table the proportions of carbonate of soda and silica for 100·0 parts of solid matters are given for certain springs, whose analyses will be found in tables III and IV:

	III. 1.	III. 5.	III. 2.	III. 6.	III. 4.	III. 8.	III. 3.	III. 7.	IV. 1.	IV. 3.	IV. 2.	IV. 5.
Carb. of soda.....	·6	1·6	2·4	3·4	7·0	8·0	9·2	21·0	25·0	30·0	56·0	6·7
Silica	·4	·4	·6	·6	1·6	1·5	1·7	2·9	3·0	3·2	3·2	32·0

The amount of silica which these waters contain does not in any case exceed one or two ten-thousandths, and it is well known that water at the ordinary temperature may dissolve very much more than this amount of silica, even in the presence of alkaline chlorids and of bicarbonates.

§ 70. Inasmuch as carbonic acid, according to Bischof (Chem. Geol. i, 2), decomposes not only the silicates of soda, but those of lime and magnesia, when they are in solution, it might be supposed that the silica in the above waters exists either in a free state or as a soluble silicate with a great excess of acid. The latter view, especially in the case of magnesia, is rendered probable by numerous experiments which I shall describe in another paper, and which form a part of the series already mentioned in § 14. From these it appears that free soluble silica, when mingled with a solution of bicarbonate of magnesia, or with the neutral carbonate dissolved in sulphate of magnesia in the manner described in § 56, whether separating immediately or by a slower process of gelatinization, always carries down with it, in combination, a few hundredths of magnesia.

In these experiments, besides the carbonate of magnesia, sulphate or chlorid of magnesium was present; but the silicated natural waters now under discussion are alkaline from the presence of carbonate of soda, and whatever partition of bases between carbonic and silicic acids may exist in the recent waters, we may suppose that when they are boiled a silicate of soda is formed, with the expulsion of carbonic acid. The silicate thus produced reacts on the earthy bases present, with the production of silicates of lime and magnesia, which are in part precipitated with the earthy carbonates. Berzelius and Kersten long since observed the separation of such silicates during the evaporation of the waters of Carlsbad and of Marienbad (Bischof. i, 5); while a silicate of lime is said to be deposited from the waters of Wiesbaden. But the silicates thus formed are but partially precipitated—a portion remaining in solution till a late period of the evaporation. Dr. J. Lawrence Smith long since remarked the existence of a dissolved silicate of lime, apparently combined with soda, in the concentrated alkaline waters of Broosa in Asia Minor, (this Journal [2], xii, 377).

Many facts in accordance with the above were observed in the analyses of the waters described in this paper. Thus the water of Belœil, which held in 1000 parts ·114 of silica, when evaporated to one-tenth, deposited with the carbonates ·050 of silica, and the hydrochloric solution of the precipitate became gelatinous during evaporation. The solution still retained in solution, besides a portion of lime, ·064 of silica, which was completely separated when the alkaline liquid was evaporated to dryness in contact with the earthy carbonates previously precipitated.

When however these were removed by filtration it was found that during the evaporation to dryness a reaction took place by which the precipitated silicate of lime was partially decomposed, the separated silica being redissolved by the alkaline carbonate. In the case of the Chambly water of 1852, which contained in 1000 parts ·073 of silica, ·042 parts still remained in solution in the water evaporated to one twentieth; and in that of the Ottawa River when reduced to one fortieth there still remained in solution from 10·000 parts of water, ·075 of silica and ·028 of lime. Similar results were observed with the alkaline saline waters of Varennes and Fitzroy, and all of these yielded, by further evaporation, precipitates containing silica and lime, and in one instance magnesia.

It is not however from alkaline waters like these, but from neutral sea-water that the silicates of magnesia (and of lime), which abound in stratified rocks, have been for the most part formed. See further on this point, § 41.

§ 71. *Organic Matters.*—In § 44 we have described some of the reactions of the organic matter found in the Chambly water, and it is to be remarked that small portions of a similar substance were found in all alkaline waters of the third and fourth classes and caused them to become brownish when evaporated to a small volume. This it has been already suggested may have a superficial origin, the organic matters carried down by surface waters being kept in solution by the alkaline salts; it is not, however, impossible that this same menstruum may remove the organic matters which abound in the pyroschists and other materials of organic origin in the ancient rocks. Thus, for example, the coprolites of the Lower Silurian limestones contain so much animal matters as to evolve an odor like burning horn when exposed to heat. (Geology of Canada, 462.)

The Ottawa water (§ 45) when boiled to one-tenth deposits a precipitate in small bright brown iridescent scales. This was found to contain silica, carbonate of lime and a small portion of an organic substance which was dissolved in a dilute potash ley. The brown solution thus obtained was not disturbed by acetic acid and acetate of copper, but by the subsequent addition of carbonate of ammonia yielded a white precipitate. The concentrated water retained a large proportion of organic matter, and when reduced to a small bulk, was dark brown, alkaline to turmeric paper, and continued by evaporation to deposit opaque films of silicate of lime. The finally dried residue was dark brown in color, and carbonized by heat, burning like tinder, and diffusing an agreeable odor. The residue of 10·000 parts dried at 300° F., weighed ·6974, and lost by gentle ignition ·1635, consisting partly of organic matter.

No chemical examination was made of this matter held in solution by the concentrated water. From the late researches of Peligot, however, it appears that the organic matter precipitated by nitrate of lead from the water of the Seine has nearly the composition of the apocrenic acid of Berzelius. It gave on analysis carbon 53·1, hydrogen 2·7, nitrogen 2·4, oxygen 41·8, and is evidently related to the soluble form of vegetable humus. (Comptes Rendus, April 25th, 1864). When exposed to heat this substance evolved ammonia, with the odor of burning wool, while the organic matter from the Ottawa water, on the contrary, gave an odor like burning turf.

GEOLOGICAL POSITION OF THE PRECEDING WATERS.

§ 72. The great Paleozoic area of the St. Lawrence basin is divided into two basins by an axis extending from Deschambault, not far above Quebec on the St. Lawrence, in a southwest direction to Lake Champlain. The eastern part of the western basin is more or less affected by undulations subordinate to the great fault that brings up the Quebec group against the Hudson River formation, and also by other undulations of minor importance. It is in this disturbed region that by far the greater number of the mineral springs already described occur; and although it is often difficult to establish the presence, or to trace the extent of faults in the strata, on account of the alluvial deposits which generally cover the Paleozoic strata of the region, it is apparent that in a great number of cases the mineral springs occur along the lines of disturbance, and it is probable that a constant relation of this kind exists. As the eastern limit of the western basin is approached, the mineral springs become more numerous, but this boundary once passed, a region is soon reached where the rocks become profoundly altered, and furnish no more mineral waters. The great western portion of the occidental basin, which is less disturbed than its eastern part, presents but few mineral springs; yet the wells of strongly saline water which have been obtained by borings at Kingston, Hallowell, St. Catherines, Chatham and elsewhere, show that the undisturbed rocky strata are charged with saline matters. For a better understanding of the relations of these waters a list of the Paleozoic formations in which the springs occur is here given, and numbered in ascending order.

15. **Hamilton**,—shales.
14. **Corniferous**,—limestone.
13. **Oriskany**,—sandstone.
12. **Onondaga** or **Gypsiferous**,—dolomite.
11. **Guelph**,—dolomite.
10. **Niagara**,—dolomite.
9. **Clinton**,—limestone and shale.
8. **Medina**,—sandstone.
6. **Hudson River**,—shales.
5. **Utica**,—shales.
4. **Trenton**,—limestone.
3. **Chazy**,—limestone.
2. **Calciferous**,—dolomite.
1. **Potsdam**,—sandstone.

§ 73. Of the above series the Trenton group includes the Birdseye and Black River limestone, as well as the Trenton limestone of the New York geologists, and is non-magnesian, enclosing beds of chert, silicified fossils and petroleum; in all of which characters it resembles the Corniferous limestone above. In like manner, the Potsdam is represented by the Hudson River and Medina formations, while the gypsiferous dolomite of the so-called Calciferous sandrock corresponds to the great mass of dolomite which constitutes Nos. 10, 11, and 12, and includes the gypsum and the salt-bearing strata of the Onondaga formation. These repetitions of similar strata, marking successive recurrences of similar geological and geographical conditions, which form great cycles in the history of the continent, have been already considered in a paper by me on Bitumens, etc., in this Journal, [2], xxxv, 166.

§ 74. In the eastern basin, which includes not only south-eastern Canada, but the whole of New England, the strata are in an altered and crystalline condition, if we except a narrow belt along the northwest border of the basin. These unaltered strata present a great series of shales, conglomerates, and limestones, pure and magnesian, succeeded by 2000 feet or more of sandstone with shales; the whole forming what the Canadian Geological Survey has named the Quebec group, whose aggregate thickness in the vicinity of Quebec is about 7000 feet. The geological horizon of this group of strata corresponds to that of the Chazy, the Calciferous, and perhaps of the Potsdam. It was in great part a deep-sea deposit, of which the formations just named are but incomplete and littoral representatives. Of the waters described in this paper none are from this eastern basin, although the unaltered portions of it present several mineral springs, some of which are described in the Geology of Canada. Of these the salines of Cacouna, Green Island, Riviére Ouelle and Ste. Anne, are bitter waters belonging to the first class; while a sulphurous spring at the latter place, and another at Quebec are alkaline waters of the fourth class.

§ 75. Of the waters of the western basin, which alone are noticed in this paper, many have been qualitatively analyzed which are not here described. Including two from Vermont, twenty-one alkaline waters of the third and fourth classes have been examined. Of these, as already stated, the waters of Caledonia rise from the Trenton group, and that of Fitzroy from the Chazy or Calciferous, while two others at Ste. Martine and Rawdon, appear to have their source in the Potsdam. All the other waters of these two classes issue from the Hudson-River shales, with the exception of those of Varennes and Jacques Cartier, which seem to rise from the Utica formation.

Of the waters of the second class, of which about thirty have been examined from the western basin, some five or six issue from the shale formations Nos. 5 and 6, but all the others are from the underlying limestones. The bitter salines of the first class flow from the limestones of the Trenton group, with the exception of that of Ancaster, which is from a well sunk in the Niagara formation, and that of St. Catherines, from a boring carried through the Medina down into the Hudson River shales. The source of both of these is probably, like that of the other very similar waters, the Lower Silurian limestones.

§ 76. From this distribution of the waters of the first four classes it would appear that the source of the neutral salts, which consist of alkaline and earthy chlorids, is in the limestones and other strata from the Potsdam to the Trenton inclusive, while the alkaline carbonates are derived from the argillaceous sediments which make up the Utica and Hudson River formation. These sediments are never deficient in alkaline silicates, whose slow decomposition yields to infiltrating waters (§ 13), the alkaline carbonates which characterize the mineral springs of the fourth class. These mingling in various proportions with the brines which rise from the limestones beneath, produce the waters of the second and third classes in the manner already explained. The appearance of several springs of the third class, as those of Caledonia and Fitzroy, from the Lower Silurian limestones is not surprising, when it is considered that the Chazy formation in the Ottawa valley includes a considerable thickness of shales, sandstones and argillaceous limestones, approaching in composition to the sediments of the Hudson River formation.

§ 77. As an evidence that the different classes of waters have their origin in different strata may be cited the fact that springs very unlike in composition are often found in close proximity, and apparently rising from a common fissure or dislocation. Thus in the seigniories of Nicolet and Labaie du Febvre, I have examined six springs, all of which rise through the Utica formation along a line, in a distance of about eight miles. Of these springs two belong to the second, two to the third, and two to the fourth class; these last being probably derived entirely from the shales, while the others have their source in the underlying limestones, and are more or less modified in their ascent. Again, at Sabrevois, within a few feet, are two springs of the second class, of which one contains salts of baryta and strontia, and the other soluble sulphates. In like manner at Ste. Anne, in the Quebec group, a spring of the second class and one of the fourth are found not far apart. The springs of Caledonia offer another and not less remarkable example. In 1847 there were to be seen, not far from a spring of the second class, three others of the third class very near together, one of them sulphurous, but all sulphated, and differing in the proportions of carbonate of soda present. In 1865, while one of these still retained its character of a sulphurous sulphated water of the third class, the others were changed to waters of the second class, and held salts of baryta in solution. These relations which we have already pointed out (§ 47) not only show waters holding incompatible salts issuing from different strata along the same fissure, but mingling in such varying proportions as to produce from time to time changes in the constitution of the resulting springs.

§ 78. The temperature of none of the springs which we have here described exceeds 53°, which has been observed for two springs at Chambly, about twelve miles from Montreal. Inasmuch as the mean temperature of this city, as deduced from the observations of twenty-seven years, is 44°·67, the Chambly waters are to be regarded as slightly thermal. No other springs in Canada are known to present so high temperature unless possibly the acid waters of the fifth class, for which we have pointed out the importance of further observations, (§ 48). The St. Léon spring was found to be 46°, and that of Caxton, 49° F.

§ 79. The extended series of analyses which we have given in the preceding pages presents many points of interest. Nowhere else, it is believed, has such a complete systematic examination of the waters of a region, and of a great geological series been made. Additional importance is given to these results by the fact that the waters are all derived from Paleozoic strata, and we are thus enabled to compare these saline materials of an ancient period with those which issue from, and in many cases owe their saline impregnation to, strata of comparatively modern origin. Comparisons of this kind, such as I have already instituted between brines of different geological epochs in § 39, possess great geological interest.

It is a consideration not without interest, that the valley of the St. Lawrence under different meteorological conditions might become a region abounding with saline lakes, affording sea-salt, natron and borax, the results of the evaporation of the numerous saline and alkaline springs which have just been described.

§ 80 A few considerations are here suggested by the fact already mentioned of the apparent absence of mineral springs from the altered Paleozoic strata of the Quebec group. Metamorphism and disturbance or displacement of strata are generally concomitants, not, as I conceive, because the process of alteration is in any way connected with the disturbance of the rocks, but because a great accumulation of superincumbent strata, a necessary preliminary of metamorphism, is the efficient cause of the folding of the deeply buried and subsiding rocks, in a way which I have already elsewhere pointed out.[1] The subsequent continental uplifting of the altered, plicated, and more or less fissured strata, and their irregular erosion, give rise to the broken surfaces of metamorphic regions, and at the same time permit the saline solutions impregnating the strata to flow out; while solid soluble salts, unless enclosed by impermeable strata, are removed by lixiviation. Hence we shall rarely find muriated waters issuing from crystalline and disturbed strata. Those saline products which result from the decomposition of feldspathic minerals, and the separation of alkaline carbonates; or from the decomposition by these or other agents of the gypsum which is often present in metamorphic strata, may, however, readily give rise to waters of the fourth and sixth classes; so that we are not suprised to find alkaline and sulphated waters issuing from crystalline strata.

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[1] This Journal, [2] xxxi, 412.

§ 81. I have in a previous section (§ 57) alluded to the condition of the primeval ocean, and in this connection it may be well to refer to a hypothesis which I some years since advanced, relative to the origin of its salts and the primeval sediments. Starting from the notion "of a cooling globe, such as the igneous theory supposes our earth to have been at an early period, and considering only the crust with which geology makes us acquainted, and the liquid and gaseous elements which now surround it, I have endeavored to show that we may attain to some notion of the chemical conditions of the cooling mass by conceiving these materials to again re-act upon each other under the influence of an intense heat. The quartz, which is present in so large a proportion in many rocks, would decompose the carbonates and sulphates, and, aided by the presence of water, the chlorids both of the rocky strata and of the sea; while the organic matters and the fossil carbon would be burned by the atmospheric oxygen. From these re-actions would result a fused mass of silicates of alumina, alkalies, lime, magnesia, iron-oxyd, etc.; while all the carbon, sulphur and chlorine in the form of acid gases, mixed with watery vapor, nitrogen, and a probable excess of oxygen would form an exceedingly dense atmosphere. When the cooling permitted condensation, an acid rain would fall upon the heated surface of the earth, decomposing the silicates, and giving rise to chlorids and sulphates of the various bases, while the separated silica might take the form of crystalline quartz. In the next stage of the process, the portions of the primitive crust not covered by the ocean would undergo a decomposition under the influence of a hot moist atmosphere charged with carbonic acid, and the feldspathic silicates become converted into clay, with separation of the alkali. This, absorbing carbonic acid from the atmosphere, would find its way to the sea, where, having first precipitated from its highly heated waters various metallic bases then held in solution, it would decompose the chlorid of calcium, giving rise to chlorid of sodium on the one hand, and to carbonate of lime on the other. In this way we obtain a notion of the processes by which from a primitive fused mass may be generated the siliceous, calcareous and argillaceous rocks which make up the greater part of the earth's crust; and we also understand the source of the salts of the ocean."[1]

§ 82. A further development of this view would lead us too far for the scope of this paper. It will however be seen that the first precipitates from the ocean would contain most of the metals, and that in the subsequent re-solution and deposition of these precipitates is to be found an explanation of the origin of metalliferous deposits, and of their distribution in various formations; either as integral parts of the strata, or as deposits in veins, the former channels of mineral springs. In an essay on American Geology, published in this Journal in 1861, [2], xxxi, 405, I have already sketched the outlines of what I conceive to be the true theory of metalliferous deposits, a subject to which it is proposed soon to return.

Montreal, July 4, 1865.

Art. XXI.—*A new Meteorite from Newton county, Arkansas, containing on its surface Carbonate of Lime;* by J. Lawrence Smith, Prof. Chem. Med. Dep. University of Louisville.

The first notice of the meteorite of Newton county was made in 1860 by Prof. Cox, who was engaged in the geological survey of Arkansas. The original has not been obtained; the only fragment of it, being in the hands of Judge Green, was given to Prof. Cox, who has kindly presented it to me. The weight of the fragment is twenty-two and a half ounces, and was evidently broken off from one corner of the mass, as it presents three of the original surfaces.

This meteorite is of the mixed variety, and cannot be classed with either the metallic or the stony meteorites; it is one of the most interesting that has been discovered in the United States, differing from any other yet found in these regions.

The stony matter is very distinctly crystallized, and some of the minerals can be easily detached and examined separately. The metallic portion constitutes somewhat over one-half of the mass, and owing to the diffusion of the stony matter has a coarsely reticulated structure.

When broken under the hammer, and the iron separated by the magnet, it is obtained in coarse grains varying from 3 to 4 grains down to very small fragments. The exterior is of a rusty color, roughened by projection of nickeliferous iron, and over several parts of the surface there is a white incrustation.

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[1] Canadian Journal, May, 1859, 201, and this Journal, [2] xxv, 102, also Comptes Rendus, June 9th, 1862, and Can. Naturalist, vii, 202.

Specific gravity taken on different pieces varies from 4·5 to 6·1. By mechanical means and the aid of the magnet the following minerals were separated.

Nickeliferous iron,
Chrome iron,
Sulphuret of iron,
Hornblende,
Olivine,
Carbonate of Lime.

Nickeliferous Iron.—I may as well mention the manner in which I separate the iron from the stony matter of meteorites. In most instances it is necessary to sacrifice a fair portion of the specimen. The mass is crushed in a steel mortar; the magnet is then able to take out the iron from the mass of stony matter, especially if the crushing operation is repeated two or three times. The iron is then introduced into an iron, or, better still, a silver capsule or crucible, and a strong solution of potash added, heat is applied until all the water is driven off, and the residue is heated to redness; on cooling, water is applied and the excess of potash washed out, as well as some silicate of potash that is formed. After thoroughly washing the particles of iron, they are moistened with a little alcohol and dried on blotting paper with a gentle heat; and by holding a magnet a little distance from them, the particles of iron will adhere to the magnet almost perfectly free from earthy matter.

The iron, if of a coarse reticulated structure, as the one in question, may require to be crushed in the steel mortar after treatment by potash, to detach particles of silicate, remaining in small crevices, and in this variety I sometimes repeat the treatment by potash. In this way, the foreign matter associated with the iron can be reduced to one-half per cent. Of course this process sacrifices more or less of the iron, especially if the iron be in very small particles; but this sacrifice is of secondary importance compared with the necessity of having the metallic matter in a pure state. Thus purified the iron was found to be composed of

Iron,	91·23
Nickel,	7·21
Cobalt,	·71
Copper, Phosphorus, } too small to be estimated.	
	99·15

In the analysis, after separating the iron by the acetate of soda, the nickel and cobalt were separated by nitrite of potash, which method I have used frequently, and with the best results. Liebig's method for accomplishing the same end has been much improved by the modification lately devised by Prof. Gibbs, of dissolving the oxyd of mercury in the cyanid of mercury, (this Journal, Jan., 1865); but having every arrangement necessary for executing successfully the method by the nitrite of potash, I have not yet tried Prof. Gibbs's modification, but shall do so shortly.

Chrome Iron.—This is found in small quantity in minute particles, some of them showing distinct faces of crystals, but I failed to find any complete octahedron; the quantity was too small for analysis but was readily recognized by the blowpipe.

Sulphuret of Iron.—This also is discernible only in minute quantity, and could not be collected for analysis. I would remark, with reference to the sulphuret of iron found in meteorites, that it cannot be classed with the terrestial magnetic pyrites, whose formula is considered Fe_7S_8 having always found the sulphur too small for this formula; in which conclusion I believe that I am sustained by Rammelsberg and others. My results point to the formula $\dot{F}eS$, and if the compositions of these two kinds of pyrites are correctly made out, then the meteoric variety has no terrestrial representative.

Hornblende.—This mineral is easily separated, and is of a greenish gray color more or less soiled by iron; with some care it can be detached unmixed with other constituents; it has a very distinct cleavage in one direction and an imperfect one in another; on analysis it gave,

Silica,	52·10
Alumina,	1·02
Protoxyd of iron,	16·49
Protoxyd of manganese,	1·25
Magnesia,	29·81
Alkalies—(potash, soda, lithia),	·24
	100·91

The oxygen relations of the silica and protoxyds furnish the formula $\dot{R}_4 Si_3$—the formula of hornblende. In structure and composition it is not unlike some varieties of anthophyllite.

Olivine.—This mineral is diffused through the mass. Some of the smaller pieces are almost colorless; others again are more or less yellow, being stained with oxyd of iron; some of the fragments are iridescent like varieties of oligoclase, which I at first took it to be. Sufficient of it was detached in a pure state for analysis, and was found to be composed as follows:

Silica,	42·02
Alumina,	0·46
Protoxyd of iron,	12·08
Magnesia,	47·25
	101·81

There was a minute quantity of manganese estimated with the oxyd of iron and magnesia. This analysis overruns the 100.

This is accounted for in part by the quantity used for analysis not being more than 0·160 grams. The oxygen ratio of the silica and protoxyds show the composition $\dot{R}_3\dddot{S}i$, which is that of olivine.

Carbonate of Lime.—The observation of this constituent in a meteorite is something entirely new, yet it is found on the exterior surface of the meteorite in question, in various places. There is no doubt in my mind, however, that this ingredient was not a part of the mass when it fell, but that it has been exposed to certain conditions since its fall by which carbonate of lime has been incrusted on its surface.

It is much to be regretted that the entire original mass is not accessible to furnish facilities for determining whether it is an incrustation, or not, and if the former, whether the incrustation was formed prior or subsequent to its fall.

In relation to the presence of carbonates in meteorites, we have the first and only announcement, up to the present time, in connection with the meteorites which fell at Orgueil in 1863; Messrs. Des Cloizeaux, Pisani, Daubrée and Cloez discovered minute rhombohedral crystals of double carbonates of magnesia and iron.

The above statements exhaust about all that I have to say at the present time on the meteorite under investigation. There may be one or two other minerals in its composition, but I could not separate them in a manner to pronounce as to whether they were different from those already described or not.

ART. XXII.—*Researches on the Volatile Hydrocarbons;* by C. M. WARREN.*

II—ON THE INFLUENCE OF C_2H_2 UPON THE BOILING POINTS IN HOMOLOGOUS SERIES OF HYDROCARBONS, AND IN SOME SERIES OF THEIR DERIVATIVES; WITH CRITICAL OBSERVATIONS ON METHODS OF TAKING BOILING-POINTS.

IT is well known that we are indebted to H. Kopp[1] for the discovery of certain definite relations existing between the chemical constitution and some of the physical properties of homologous liquid bodies. Of these, one of the most important is that of a uniform difference between the boiling-points of the contiguous members of an homologous series, corresponding to the uniform difference in their elementary constitution. Kopp has shown by numerous examples, that, as a general rule, in those series which are characterized by a common elementary difference of C_2H_2 between the members, in the order of the series, the corresponding difference of boiling-point is about 19° C.; hence, that the difference between the boiling-points of any two members of such a series is x .19° for a difference of $x\,C_2H_2$ in the elementary formulæ. In the earlier observations on this subject, this relation between the boiling-points and formulæ was found so nearly constant in the different series examined, that any deviations from this apparent general law were referred, not unreasonably, to assumed inaccuracies in the determination of the boiling-points of the bodies compared. But the more recent and extended generalizations of Kopp[1] have led him to point out several exceptional series, in which the boiling-point difference is greater, and others in which it is less, than 19° for an elementary difference of $C_2\,H_2$. That there are such exceptional series is confirmed in a very decisive manner by my own observations, as I shall proceed to show. My determinations make the boiling-point differences in some cases so much larger than those of other observers as to leave no room for doubt on this point; especially if the comparative value of these determinations be duly estimated with reference to the more reliable character to which the preparations are entitled, on account of the more efficient means which I have employed for separating the liquids. Since Kopp first called the attention of chemists to this subject, different theories have from time to time been advanced by Schröder, Löwig, Gerhardt, and others, and supported by laborious research and observation. It will be interesting to examine some of these theories in the light of the new facts which I am about to present. Schröder,[2] not satisfied with Kopp's explanation of the discrepancies between the observed and theoretical boiling-points, on the ground of errors of determination of the former, argues that the influence of $C_2\,H_2$ on boiling-points is variable in different series according to the peculiar nature of the C_2H_2 in each case. He regards organic compounds for the most part made up of radicals, which he calls "components," of which he makes seven. Three of these are composed of carbon and hydrogen, viz:—

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* Continued from page 108.

[1] Ann. der Chemie und Pharmacie, 1842, xli, 79, 169; 1845, lv, 177, etc.

Formyl = $(C_4\,H_2)$—"(C_4H_4)"—which is supposed to raise the boiling-point of a substance 52° C.

Methylen = $(C_2H_2)^m$—"$(C_2\,H_4)^m$"—which is supposed to raise the boiling-point of a body 21°.

Elayl = $(C_2H_2)^e$—"$(C_2H_4)^e$"—which is supposed to raise the boiling-point 17°. Subsequently (Pogg. Ann., lxiv, 101) the latter number was changed by Schröder to 16°.

A fourth component was made up of a double atom of hydrogen, (H_2)—"(H_4)"—which was supposed to lower the boiling-point 3°; but this also was afterwards changed to 10° (Pogg. Ann., lxiv, 372). (The other three components, having no direct bearing on the hydrocarbons, are omitted.) By means of these components Schröder (Pogg. Ann., lxii, 188) proposed to calculate the boiling-points of different substances in the following manner. Having estimated the sum of the influence of the different components of a body, the number 70 was in all cases to be deducted. Subsequently Schröder[4] was led to substitute, in these calculations, the influence of the separate elements for that of the components. Each double atom of carbon (C_2) was estimated to raise the boiling-point of a compound 31°; and each double atom of hydrogen (H_2) to lower it 10°. As in the former case, the number 70 was to be deducted from the sum of the influences of the different elements contained in the compound, to give the true boiling-point. Example: calculation of the boiling-point of benzole, $C_{12}H_6$; $C_{12}=6C_2$; $31\times 6^\circ=186^\circ$; $H_6=3H_2$; $-10^\circ\times 3=-30^\circ$; $186^\circ-30^\circ-70^\circ=86^\circ=$ the calculated boiling-point of benzole by this method; which agrees exactly with the latest determination at the date of Schröder's memoir.

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[1] Annalen der Chemie und Pharmacie, 1855, xcvi, 2.

[2] Poggendorff's Annalen, 1844, lxii, 184, 337.

Löwig[5] estimates the influence of the elementary atoms on the boiling-point differently from Schröder; and obtains numbers such that, to find the boiling-point of a compound it is only required to add together the numbers corresponding to the elementary atoms which it contains, without deducting from this sum a constant number, as by Schröder's method. According to Löwig's theory, one atom of carbon (C) raises the boiling-point 38°·4, and one atom of hydrogen (H) lowers it 29°·2; these numbers being for carbon nearly two and one-half times, and for hydrogen nearly three times as great, as those of Schröder.

Gerhardt,[6] in a special paper "On the Boiling-point of the Hydrocarbons," observes that "The boiling-point of the hydrocarbons appears to obey a very simple law, according to which it is raised or depressed a certain number of degrees, corresponding to the number of equivalents of carbon or hydrogen contained in its equivalent."[7] From a comparison of the boiling-points and formulæ of several well-known hydrocarbons, the determinations of which were repeated with special care for this purpose, Gerhardt finds that the addition of C_2 to the molecule of an hydrocarbon raises its boiling-point 35°·5, and that the addition of H_2 lowers it 15°. The boiling-point of a body is calculated from these numbers by comparing its formula with oil of turpentine, $C_{20}H_{16}$, as a standard, the boiling-point of which is taken at 160° C. Example: cumole (from cuminic acid) has the formula $C_{18}H_{12}$; hence it contains C_2 less than oil of turpentine; therefore 35°·5 must be deducted from 160° (the boiling-point of oil of turpentine,) which leaves 124°·5; but as the cumole contains $2H_2$ less than oil of turpentine, $15° \times 2 = 30°$ is to be added to the above remainder; thus $124°·5 + 30° = 154°·5$, the calculated boiling-point of cumole. Gerhardt's direct determination was 153°, which very nearly coincides with his theory.

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[4] Pogg. Annalen, 1845, lxiv, 367; 1846, lxvii, 45. [5] Idem., 1845, lxiv, 250.

[6] Annales de Chimie et de Physique, 1845, [3], xiv, 107.

[7] "Il parait que le point d'ébullition des hydrogènes carbonés est soumis à une loi fort simple, d'après laquelle il s'élèverait ou s'abaisserait d'un certain nombre de degrés suivant le nombre des équivalents de carbone ou d'hydrogène renfermés dans leur équivalent."

It would be foreign from my purpose on the present occasion to consider these different hypotheses, or even the empirical law of Kopp, beyond their special relation to the boiling-points of the hydrocarbons, and such other series, derivatives from the hydrocarbons, as have been made the subjects of my own experiments. Anything more than this would be merely speculative. The want of more accurate determinations of boiling-points as essential to safe and reliable deductions and generalizations on this question, has frequently been observed. The need of this will be made strikingly apparent by comparison of my results with those of previous observers. Indeed, if my determinations may be taken as a criterion,—which, considering the nature of the materials operated upon, might not be quite fair,—the inaccuracies of the boiling-points which have hitherto been published are probably so numerous, and in many cases so considerable, as to make it appear almost useless to attempt further generalizations upon those unreliable data. It may be hoped, however, that the superior means which my process furnishes for separating mixtures of liquids, will lead to the accumulation of reliable facts of sufficient number and variety for a profitable review of this question in its different bearings, which, from its importance, it clearly merits.

The frequent inaccuracy of the determinations of boiling-points, upon which Kopp has justly laid so much stress, may, I think, be more reasonably attributed, at least in a majority of cases, to a want of purity in the substances themselves, rather than to a neglect of the precautions and corrections which he recommends to be observed in such determinations; although errors as great as those mentioned by Kopp[a] may doubtless occur, and in the particular instances which he had in mind may have occurred from the cause which he assigned for them. It should be borne in mind, however, that these errors, in the case of an impure substance, may be compensating errors; or, on the other hand, they may go to increase that which would arise from impurity.

That the conditions under which my results have been obtained may be clearly understood, and hence the value of these results fairly estimated, in comparison with those of others, I shall endeavor, as I proceed with these researches, to specify, in sufficient detail, the processes which I have employed. Having, in the memoir previously referred to, described the process by which the hydrocarbons were separated, the special object of this paper only requires, in this regard, that I should add a description of the method employed in determining the boiling-points of these bodies, which has already been partially given in the foregoing memoir, when treating of the boiling-point of benzole.

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[a] "Bestimmung des Siedepunkts." Poggendorff's Annalen, 1847, lxxii, 38.

Of the Method of determining Boiling-points.—I use for this purpose a small tubulated glass retort, and usually operate on about 150 c. c. of the liquid. The thermometer extends into the liquid, even nearly to the bottom of the retort, taking care that the bulb shall not come in contact with the glass, but remain free in the liquid. To prevent abnormal elevation of temperature from adhesion to the glass,—which I have observed in some instances, when operating on impure hydrocarbons, to amount to several degrees,—I introduce pieces of sodium, instead of platinum, as it seems to serve at least as well for this purpose, and at the same time tends to preserve the purity of the material. Sodium has also this advantage over platinum for hydrocarbons, viz: that it does not lose its virtue by use, so long as any of it remains; platinum, on the contrary, being liable, especially if the liquid is not quite pure, to become after a while slightly coated, and its efficiency thus impaired.*

Except for low temperatures, the retort rests on a piece of wire gauze laid over the ring of an iron lamp stand, and is heated with a small gas flame. When operating on liquids of low boiling-point, I have observed the liability of the thermometer to be considerably affected by the ascending current of hot air striking the sides of the retort above the level of the liquid, thus causing an elevation of several degrees of temperature. To prevent this, I proceed as follows. For low temperatures, and yet above the common temperature, I place upon the gauze on which the retort is to stand, a screen of felt or thick woolen paper, which has been provided with a hole in the center about two inches in diameter. This screen extends several inches from the sides of the retort, and has been found effectual for the purpose.

For temperatures below the common temperature, the retort is set in a water-bath containing ice-water, the temperature of the bath being gradually raised by means of a small gas-flame.

As is customary, in order to ascertain the temperature by which to calculate the correction for the upper column of mercury, a thermometer is attached, by means of elastic bands, to the side of the thermometer in the retort; the bulb being placed, during ebullition, midway between the center of the cork and the upper extremity of the mercurial column. And, as usual, a paper screen, closely fitting the thermometer, is placed across at the top of the cork to shield the upper column of mercury from the direct influence of the ascending heat.

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* For common use in fractioning, when not desirable to use sodium, I have found pieces of coke to be more effectual and much more durable than platinum. Not unlikely it would be found equally preferable to platinum for general use in taking the boiling-points of liquids in which sodium could not be employed. It is certain that nothing could operate better than coke for the nitro-compounds and alkaloids derived from benzole and its homologues.

I have observed that it often requires considerable time—variable according to its length and the thickness of the glass spindle—for that part of the thermometer above the retort to acquire the highest temperature which the boiling liquid can communicate to it. During this time the thermometer evidently is not in a fit state for an observation. While this gradual change in the condition of the thermometer is taking place, it is desirable, for obvious reasons, that no vapors should escape from the retort. I therefore proceed as follows. The retort, the neck of which has previously been wrapped with a wet cloth, is placed in such a position that the neck shall slightly incline towards the body of the retort. If necessary, some pieces of ice, which will adhere firmly to the cloth, may be laid along the neck to insure complete condensation of the vapors during ebullition. While the retort is in this position, ebullition is continued for considerable time, until it ceases to have any effect on the height of the mercury in the thermometer. The lamp being now removed for the moment, the neck of the retort is turned down, and quickly connected with a Liebig condenser. The lamp being now replaced, the distillation is commenced. So soon as the mercury in the thermometer shall have become constant, which will now occupy but a few seconds, the temperatures by the retort thermometer and the side thermometer are carefully noted, and also the time at which these observations are made. During the distillation, which is continued nearly to dryness, the readings of the thermometers and of the watch are noted at regular intervals, or so often as any appreciable variation of the retort thermometer shall have taken place. The average of the several observations, or of those corresponding to the longer intervals of time, applying the corrections for atmospheric pressure and for the upper column of mercury, according to Kopp,[10] is taken for the true boiling-point. I have generally obtained the hydrocarbons so pure that the whole quantity operated upon would distil within the range of 1° of temperature, and not unfrequently within 0°·5. In a few cases, however, when the quantity of material at command would not permit of the attainment of so high a degree of purity, the distillation would range over two or three degrees; in such cases I have generally taken the average of the temperatures corresponding to the longest interval of time, as probably representing more nearly the true boiling-point of the body. In stating my results, however, I shall give the limits of temperature within which the distillation was effected. The thermometers employed in the determinations were the best that I could obtain from Fastré of Paris; for the temperatures below 100° the instrument used was calibrated, and the scale divided into fifths of a degree. The determinations above 100° were all made with one thermometer.

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[10] Poggendorff's Annalen, 1847, lxxii, 38.

The method just described differs in some respects from that of Kopp. He objects to the practice of taking boiling-points with the thermometer bulb immersed in the liquid,[11] on the ground that the thermometer in this condition hardly ever indicates a constant temperature, the end of the mercurial column being in a state of motion. He states that a boiling-point taken in this manner may lie several degrees above that found with the thermometer bulb in the vapor. As bearing on this point, I propose, a little farther on, to give the results of a few experiments and observations, which, with others of similar character, have induced me to depart from the now more common custom of taking boiling-points with the thermometer bulb in the vapor.

Under normal conditions, the temperature of the boiling liquid and that of the vapor evolved should be the same. The only disturbing influence which appears to have been specially dwelt upon as likely to alter these conditions in the taking of boiling-points, is the liability of some liquids to adhere to the surface of the glass in such a manner as to produce abnormal elevation of temperature, generally attended with irregular ebullition, and consequent fluctuation of the thermometer. To remedy this it is usual to introduce pieces of platinum; iron filings, coal, etc., have also been employed. As above remarked, pieces of coke—or, when admissible, sodium—are found to be more surely effectual with hydrocarbons than platinum. Indeed, during more than three years of experience and careful observation upon a large number of hydrocarbons, I have not yet met with a single instance in which irregular ebullition and its consequent disturbing influence upon the boiling-point might not be completely prevented by these means. Although I cannot, of course, go so far as to say that equally satisfactory results would be obtained with other liquids by the use of coke, it is nevertheless my belief that in a majority of instances such would be the case.

I have dwelt upon this point for the reason that the objections

[11] Lässt man die Kugel des Thermometers in die siedende Flüssigkeit tauchen, so zeigt fast nie das Instrument eine constante Temperatur, sondern das Ende des Quecksilberfadens ist in stetter hüpfender oder zittender Bewegung; der auf diese Art gefundene Siedepunkt kann nur mehrere Grade höher liegen, als der, welcher gefunden wird, wenn sich die Kugel des Thermometers in dem Dampf der siedenden Flüssigkeit befindet."

to the custom of taking boiling-points with the bulb in the vapor, appear to be even greater than those which Kopp has raised against the opposite course of placing the bulb in the liquid, as I shall proceed to show. It therefore becomes a matter of some importance that the objections to one or the other custom should be removed; and I think it will be found easier to overcome the objections to placing the bulb in the liquid, as I have done in the case of many hydrocarbons, even if coke shall not be found equally efficient with most other liquids.

My experience has shown that, when irregular ebullition is effectually prevented, the temperature of the vapor from a boiling liquid is more liable to lead to an erroneous determination of the boiling-point, than the temperature of the liquid itself. The reasons for this are, first, that the vapor is liable to become superheated by the hot air from the flame coming in contact with the sides of the retort above the surface of the liquid; second, that, with the bulb in the vapor, the thermometer is more liable to sudden depression from currents of cool air passing over the retort. If the bulb be in the vapor, the occurrence of either of these disturbing influences would then affect the principal mass of the mercury in the thermometer; while, on the contrary, if the bulb were in the liquid, only the small quantity of mercury in the stem of the thermometer would be subjected to these influences; the liquid then serving as a regulator, and reducing the error from these sources to a minimum. Fluctuations from currents of cold air are comparatively slight, and more easily prevented than those from overheating the vapor. The latter is the more likely to occur the lower the boiling-point of the liquid, or when the quantity of liquid in the retort is small. I have, however, observed from this cause an elevation of 3°–4° in distilling a body boiling as high as 98° C., without an unnecessarily large flame. But the liquid in this instance was pretty low in the retort.

In the case of liquids boiling below the common temperature, it seems indispensable that the bulb of the thermometer should be placed in the liquid. As evidence of this I will here state the results of observations made while occupied in fractioning some exceedingly volatile products from American petroleum.

Experiment 1.—The liquid operated upon boiled at so low a temperature that the distillation was effected by the heat of the surrounding atmosphere. The distillation was conducted in a flask, and the bulb of the themometer placed in the vapor. The flask was attached to my condensing apparatus, including the "refrigerator, B, fig. 2." [13] The temperature of the condensing-worm contained in the "elevated bath, *aa*, fig. 2,"[12] and also that of the "first receiver, *k*, fig. 2,"[12] was 11°·5. The temperature of the "cold bath, *ii*, fig. 2,"[12] was 11°. The condenser in "the refrigerator, B," and the "second receiver," were cooled in a mixture of ice and salt. With the liquid boiling steadily from several points on the bottom of the flask, and the condensed product from the distillation running well from the refrigerator into the "second receiver," not a drop was condensed in any of the apparatus intervening between the flask and the "second receiver," although this part of the apparatus was cooled, as already stated, to about 11°. The temperature of the vapor in the flask at this time was 18°·5, or only 2°·5 below the temperature of the laboratory. These observations show that the liquid was boiling at a temperature considerably below that indicated by the thermometer in the vapor. Additional evidence of this was furnished by the fact that, during the distillation, the exterior of the flask, from the bottom to about one quarter of an inch above the surface of the liquid, was thickly covered with water condensed from the atmosphere, resembling heavy dew; while above, the sides of the flask were perfectly dry. It was these observations which first directed my attention to the fact that the temperature of the vapor could not in all cases be depended upon for the true boiling-point of a liquid, and naturally led me to make other experiments with special reference to this question.

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[13] See Memoir "On Process of Fractional Condensation," Memoirs of the American Academy, 1864, and this Journal, last vol., p. 327.

Experiment 2.—The conditions of this experiment were somewhat different from those of the first. The liquid operated upon was the extremely volatile product collected in the "second receiver" of Experiment 1. The flask employed was smaller, and provided with two thermometers; the bulb of one of these was placed in the liquid, and that of the other in the vapor. The flask stood in a water-bath containing ice-water; this bath was also provided with a thermometer. The temperature of the ice-water bath was very gradually raised by means of a small flame from a Bunsen's burner. Temperature of the laboratory, 20° C. Observations during the distillation:—

	1.	Temperature of the water-bath, - -	10°
		" " boiling liquid, -	8°
		" " vapor, - - -	18°·5
	2.	Temperature of the water bath, - -	12°
		" " boiling liquid, -	9°
		" " vapor, - - -	18°
15 minutes later.	3.	Temperature of the water-bath, - -	18°
		" " boiling liquid, -	10°
		" " vapor, - - -	14°

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[12] See Memoir "On Process of Fractional Condensation," Memoirs of the American Academy, 1864, and this Journal, loc. cit.

10 minutes later.	4.	Temperature of the water-bath,	20°
		" " boiling liquid,	12°
		" " vapor,	19°
20 minutes later.	5.	Temperature of the water-bath,	23°
		" " boiling liquid,	15°
		" " vapor,	19°

Experiment 3.—The subject of this experiment was a liquid which I had separated from the most volatile product of the redistillation, on a manufacturing scale, of the crude benzole obtained in the distillation of coal-tar. The apparatus employed was essentially the same as that used in Experiment 1, with the addition of the extra thermometers, as in Experiment 2. The condensing-worm in the "elevated bath," and that in the "cold bath," and also the "first receiver," were all cooled in pounded ice. The condenser in the "refrigerator," and also the "second receiver," were both cooled in a mixture of ice and salt. The retort, which stood in a small copper bath containing pounded ice, was charged with about 250 c. c. of the liquid, which had been previously cooled in a mixture of ice and salt. Temperature of the laboratory, 16° C. Observations during the distillation:—

	1.	Temperature of the retort-bath,	0°
		" " boiling liquid,	0°·6
		" " vapor,	13°·5
45 minutes later.[13]	2.	Temperature of the retort-bath,	0°
		" " boiling liquid,	1°·3
		" " vapor,	12°·2
15 minutes later.	3.	Temperature of the retort-bath,	6°
		" " boiling liquid,	1°·8
		" " vapor,	12°·6
30 minutes later.	4.	Temperature of the retort-bath,	11°
		" " boiling liquid,	3°·8
		" " vapor,	12°·4
30 minutes later.	5.	Temperature of the retort-bath,	14°·5
		" " boiling liquid,	7°·3
		" " vapor,	13°·8

The apparent inconsistency that the temperature of the boiling liquid should be above that of the heating medium,—viz: an ice-bath—which continued during the first forty-five minutes of the experiment, is to be explained by the fact that there was a long column of mercury, above the surface of the liquid, which was subjected to the heating influence of the vapor. I would further remark that the gradual elevation of the boiling-point, as indicated by the thermometer in the liquid, is also only apparent, and is due to the gradual uncovering of the bulb as the liquid was distilled off. At the close of the experiment only about one-fifth of the bulb, which unfortunately was a long one, was under the surface of the liquid. That this is the true explanation is evinced by the fact that during the experiment not a drop of liquid was observed to fall back into the retort from the "elevated condenser," although this was a tube ten feet in length, and cooled to the temperature of 0°.

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[13] From this point the temperature of the retort-bath was gradually raised by means of a small gas-flame.

I will now proceed to give my determinations of the boiling-points of various hydrocarbons, and of some of their derivatives, and then pass directly to consider the bearing of these results on the question concerning the increment of boiling-point for the addition of C_2H_2 in homologous series.[14] The data for these considerations may be more conveniently arranged in tabular form, exhibiting at once, in serial order, the formulæ, boiling-points, elementary difference, and the corresponding difference of boiling-point.

1. *Of the Hydrocarbons obtained from Pennsylvania Petroleum.*

1st Series.

Formula.	Boiling-point.	Elementary difference.	Difference of boiling-point found.	Range of Temperature within which the substance would all distil.[15]
	°		°	°
C_8H_{10}	0·0 (?)			
$C_{10}H_{12}$	30·2	C_2H_2	30·2	1·5
$C_{12}H_{14}$	61·3	C_2H_2	31·1	0·8
$C_{14}H_{16}$	90·4	C_2H_2	29·1	1·0
$C_{16}H_{18}$	119·5	C_2H_2	29·1	1·0
$C_{18}H_{20}$	150·8	C_2H_2	31·3	0·8
			150·8÷5=30°·16	

Average increment of boiling-point for the addition of C_2H_2=30°·16.

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[14] In considering this question I shall include the boiling-points of the substances which I have separated from Pennsylvania petroleum, and the oil distilled from Albert coal; reserving for a subsequent memoir all other facts which have been derived from the study of these bodies.

[15] The ranges of temperature given in this and in the corresponding columns of the following tables, are for the purpose of showing the impossibility of there having been any essential error in the determinations of the boiling-points; as is evinced by the fact, in each case, that the whole product was found to distil without residue within such narrow limits. With so small a range of temperature, it is evident that it would make no practical difference whether either extreme or the mean of the observations be taken for the boiling-point.

The fact that these substances distil without residue within so short a range of temperature, is also of much value as proof of the existence of the two parallel series in petroleum and in coal-oil, with boiling points so near together [as shown by comparison of the boiling points of the first with the second series from petroleum; and also of the two corresponding series from Albert coal-oil]; especially if

2D SERIES.[16]

Formula.(?)	Boiling-point.	Elementary difference.	Difference of boiling-point found.	Range of temperature within which the substance would all distil.
	°		°	°
C_8H_{10}	8–9			
$C_{10}H_{12}$	37·0	C_2H_2	29·0	0·4
$C_{12}H_{14}$	68·5	C_2H_2	31·5	0·6
$C_{14}H_{16}$	98·1	C_2H_2	29·6	1·2
$C_{16}H_{18}$	127·6	C_2H_2	29·5	1·5
			119·6÷4=29°·9	

Average increment of boiling-point for the addition of C_2H_2=29°·9.

3D SERIES. (Not completed.)

Formula.	Boiling-point.	Elementary difference.	Difference of boiling-point found.	Range of temperature within which the substance would all distil.
	°		°	°
$C_{20}H_{20}$	174·9			1·7
$C_{22}H_{22}$	195·8	C_2H_2	20·9	1·5
$C_{24}H_{24}$	216·2	C_2H_2	20·3	2·2
			41·2÷2=20°·6.	

Average increment of boiling-point for the addition of C_2H_2=20°·6.

this is considered in connection with the fact, so far as my experience goes, that the quantities of material in one series are generally about equal to those in the other.

That no erroneous conception may be formed as to the degree of purity of the substances treated of in this and the following tables, from a mere inspection of the *ranges* of temperature here given; and in order that the almost absolute constancy of the boiling-points, in most cases, may not be overlooked, I would refer to the preceding memoir for further details concerning the boiling-points of such of these bodies as are therein treated of. For example, it will be found under the head of "Determination of boiling-point" of benzole, that in the distillation it required 50 minutes for the temperature to rise 0°·2; while in one of the following tables it will be seen that the *range* of temperature within which the benzole distilled to dryness was found to be 0°·8. Likewise, by reference to the "Determination of boiling-point" of toluole it will be observed that it was found to boil absolutely constant 48 minutes; while the *range* of temperature given in the table referred to is 0°·7. In such cases as these, the slight rise of temperature which takes place just before going to dryness, is doubtless to be attributed to superheating of the vapor, in consequence of there being so small a quantity of liquid in the retort. Similar instances of absolute constancy of boiling-point to those just cited, might be given from among the products in either series from petroleum and Albert coal; which the *ranges* of temperature given in these tables do not indicate.

[16] I am somewhat in doubt whether the bodies composing this series and the second series from Albert coal have the formula C_nH_{n+2} as here represented, there being some indication that they contain less of hydrogen. For the purpose for which they are now presented, it is immaterial which formula is employed, as the common elementary difference and the boiling-point differences would remain the same; the solution of this question is therefore deferred for a subsequent memoir.

2. *Of the Hydrocarbons obtained from Albert coal.*

1st series. (Not completed.)

Formula.	Boiling-point.	Elementary difference.	Difference of boiling-point found.	Range of temperature within which the substance would all distil.
	°		°	°
$C_{10}H_{12}$				
$C_{12}H_{14}$	59·9	C_2H_2		1·5
$C_{14}H_{16}$	90·6	C_2H_2	30·7	0·5
$C_{16}H_{18}$	119·7	C_2H_2	29·1	0·5
			59·8÷2=29°·9	

The average boiling-point difference, in this series, for the addition of C_2H_2, is, therefore, 29°·9.

2d series. (Not completed.)[17]

Formula. (?)	Boiling-point.	Elementary difference.	Difference of boiling-point found.	Range of temperature within which the substance would all distil.
	°		°	°
$C_{10}H_{12}$				
$C_{12}H_{14}$	68·0	C_2H_2		1·0
$C_{14}H_{16}$	98·5	C_2H_2	30·5	0·6
$C_{16}H_{18}$	125·1	C_2H_2	26·6	
			57·1÷2=28°·6	

Average boiling-point difference =28°·6.

3. *Of Hydrocarbons obtained from Coal-tar Naphtha.*

Name of substance.	Formula.	Boiling-point.	Elementary difference.	Difference of boiling-point found.	Range of temperature within which the substance would all distil.
		°		°	°
Benzole,	$C_{12}H_6$	80·0			0·8
Toluole,	$C_{14}H_8$	110·3	C_2H_2	30·3	0·7
Xylole,	$C_{16}H_{10}$	139·8	C_2H_2	29·5	0·4
Isocumole,	$C_{18}H_{12}$	169·9	C_2H_2	30·1	1·0
				89·9	

Average increment of boiling-point for the addition of C_2H_2=89·9 ÷3=29°·97.

4. *Of Cumole from Cuminic Acid, and Cymole from Oil of Cumin.*

Name of substance.	Formula.	Boiling-point.	Elementary difference.	Difference of boiling-point found.	Range of temperature within which the substance would all distil.
		°		°	°
Cumole,	$C_{18}H_{12}$	151·1			3·6
Cymole,	$C_{20}H_{14}$	179·6	C_2H_2	28·5	1·2

With only a single exception, the results presented in the above tables point clearly to 30° as the common increment for the addition of C_2H_2 in homologous series of hydrocarbons.

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[17] See footnote on preceding page.

Indeed, leaving out of the calculation the third series from petroleum (having the general formula C_nH_n),—which must remain anomalous,—and also the products from oil of cumin, the average of all the other boiling-point differences is 29°·75. The few individual variations from the number 30°, rarely exceeding a single degree, may reasonably be attributed to errors of the thermometer (especially in case of temperatures above 100°), or in some instances to a want of purity of one of the compared substances; which latter cause I doubt not is the case with the body from petroleum boiling at 37°, as upon this body I had bestowed less labor in fractioning than upon most of the others, on account of the extreme volatility and consequent loss of the substance, by which the quantity had become so much reduced that I could ill afford further loss. In the case, also, of cymole from oil of cumin, and cumole from cuminic acid, in which the boiling-point difference varies only 1°·5 from the common difference of 30°, the want of perfect agreement may be fairly accounted for by the fact that the quantity of cumole at command was too small to admit of continuing the process of fractioning far enough to obtain perfect constancy of boiling-point. In consequence, also, of the quantity being so small, the *determination* of the boiling-point of cumole is less reliable, as this had to be conducted in a test-tube. It came into full ebullition at 148°·4, the temperature rising gradually to 151°·6 (observed temperatures), at which latter temperature it had distilled nearly to dryness. The distillation occupied thirteen minutes in passing over the range of three degrees. The average of the extremes, with the usual corrections for pressure, &c., was taken for the boiling-point. Abel,[18] who probably operated on a larger quantity, found the boiling-point of cumole to be 148°. It does not appear that he applied the corrections for pressure and the upper column of mercury. I do not doubt that the true boiling-point of this body will be found to be 150°, which would establish the difference of 30° between it and cymole.

I would here remark that this difference of 30° for the addition of C_2H_2 was first observed while engaged in fractioning Pennsylvania petroleum, and the oil from Albert coal,—substances the most difficult to separate, on account of the presence in each of two parallel series of constituents, whose boiling-points lie so near together.

As no one had preceded me in the investigation of these substances, my mind was as far as possible unbiased as to the boiling-points of the constituents of these mixtures. I was, however, aware of the beautiful relation between elementary constitution and boiling-point which Kopp had discovered, and familiar with the fact that the more recent investigations had shown the boiling-point difference among homologous hydrocarbons to be about 22°·5. If there was any one thing which more than another tended to bias me, it was the recent work of Church[19] on the boiling-points in the benzole series, in which he made the boiling-point difference invariably 22° and a fraction, a number varying but 3° from the theory of Kopp. Soon after the publication of Church's results, however, Kopp[20] accepted the number 22°·5 as about the boiling-point difference in this series, therefore regarding it as one of the exceptional series in which the boiling-point difference is greater than 19°. The work of Church had certainly the appearance of having been performed with great care, conducting to a beautiful harmony of results. My confidence in his determination of boiling-points was increased not a little by his alleged discovery in coal-naphtha of xylole, boiling at 126°·2, indicating a more thorough analysis of this naphtha than those which had been previously published. This body, the supposed middle member of the benzole series, had up to that time been regarded as wanting in coal-tar naphtha, although all of the other members, above and below it, were found to be present,—an anomaly not easily reconciled with any plausible theory in regard to the formation of these bodies. In view of these circumstances, therefore, I was naturally led, from analogy, to anticipate that the boiling-point difference among the hydrocarbons from petroleum and Albert coal would not vary much from 20°. Not being able, however, to reconcile with previous facts and theories on this subject the indications which were being gradually unfolded by my seemingly unerring process of separation, I was compelled to lay aside all bias, and to regard these indications as pointing unmistakably to a much greater difference of boiling-point for the addition of C_2H_2 than had previously been supposed to exist in this class of substances.

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[18] Annalen der Chemie und Pharmacie, 1847, lxiii, 308.

Having finally established beyond question the common difference of 30° for the addition of C_2H_2 among the hydrocarbons from Albert coal and petroleum (the third series from petroleum, with the difference of 20°, had not then been reached), I began to surmise that this difference might be found to be common among all other series of hydrocarbons. In this connection my mind naturally reverted to the earlier determinations of the boiling-points of the members of the benzole series, some of which, especially those of benzole and toluole, which had been more studied than the others, indicated strongly that 30° might prove to be the true difference for the addition of C_2H_2 in this series. My confidence in Church's determinations thus began to diminish, and finally, I undertook to make a thorough analysis of coal-tar naphtha, the results of which are given in table 3.

[19] Philosophical Magazine, 1855, [4], ix, 256.

[20] Annalen der Chemie und Pharmacie, 1855, xcvi, 29.

As there shown, the boiling-point difference in the benzole series is also 30°, and the number of its members is reduced to four, in place of five, as alleged by Church.

This difference of 30°, thus shown to be so common with the hydrocarbons, is so much larger than the difference of 19° which Kopp had found so frequent in other classes of substances, that the discrepancy cannot be regarded otherwise than as conclusive evidence, if such were wanting, that all liquid bodies do not obey the same law in this regard, but that there are unquestionably those series in which the boiling-point difference for the elementary difference of C_2H_2 may be greater than 19°, of which Kopp has already furnished some examples.

That the difference may also be *less* than 19° in some series receives confirmation from the facts presented in the following tables.

6. *Of the Nitro-compounds derived from the Hydrocarbons of the Benzole Series.*

Name of substance.	Formula.	Boiling-point.	Elementary difference.	Difference of boiling-point.
Nitro-benzole,	$C_{12}H_5\ NO_4$	212·1°	C_2H_2	13·8°
Nitro-toluole,	$C_{14}H_7\ NO_4$	225·9	C_2H_2	13·4
Nitro-xylole,	$C_{16}H_9\ NO_4$	239·3	C_2H_2	
Nitro-isocumole,	$C_{18}H_{11}NO_4$			

7. *Of the Alkaloids derived from the Hydrocarbons of the Benzole Series.*

Name of substance.	Formula.	Boiling-point.	Elementary difference.	Difference of boiling-point.
Aniline,	$C_{12}H_7\ N$	184·6°	C_2H_2	17·1°
Toluidine,	$C_{14}H_9\ N$	201·7	C_2H_2	
Xylidine,	$C_{16}H_{11}N$	216·0*	C_2H_2	
Iso-cumidine,	$C_{18}H_{13}N$			

In regard to the results presented in the last two tables, it may be remarked, first, that of the difference shown in the table of nitro-compounds, viz: an average of 13°·6, the discrepancy between this and the number 19°, being 5°·6, is so large as to leave no room for reasonable doubt that this is one of those exceptional series in which the boiling-point difference is less than 19° for the elementary difference of C_2H_2. As this series does not appear to have been examined by Kopp, I have taken care to make as accurate a determination of the difference as circumstances would allow. The boiling-points were corrected as usual for pressure and the upper mercurial column. The boiling-points which have already been published of these bodies, so far as I have noticed, appear to have been given in the observed, i. e. uncorrected temperatures. The quantities of nitro benzole and nitro-toluole which I operated upon were sufficiently large, and of a high degree of purity, presenting perfectly constant boiling-points. The quantity of nitro-xylole, however, was not so large as would have been desirable. Although the boiling-point of this body is doubtless very nearly correct, those of nitro-benzole and nitro-toluole are more to be relied upon; and omitting the fraction, the number 14° may, I think, be safely taken as the true boiling-point difference in this series. Secondly, that the less striking difference presented in the series of alkaloids, being only 2° under the number 19°, cannot reasonably justify the assumption that this small discrepancy of 2° is attributable to impurity of the substances, or to inaccuracy in the determination of the boiling-points, when it is considered that great care was taken to obtain a high degree of purity and accuracy, and when it is considered also that previous observers have made this discrepancy larger than mine. It was on account of the fact that so small a discrepancy would naturally raise a doubt as to the reliability of the determinations, and for the reason that Kopp[31] has considered this series of alkaloids as agreeing tolerably well with his general law, that special care was taken on my part to arrive at a correct result. I am confident, therefore, that the boiling-point difference here will not be found to vary more than a fraction from 17°. Of the absolute accuracy of the boiling-points themselves I do not speak so confidently, since these depend so much on the accuracy of the thermometer at these high temperatures; but the correction of any errors which may have arisen from this source would not be likely to alter the relation, and the difference between the boiling-points would still remain about the same. This remark applies with equal force as to the reliability of the other boiling-points presented in this paper, especially of those of high temperatures.

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* Not corrected.

It remains now to consider the foregoing facts with reference to the other theories mentioned.

[To be concluded.]

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[31] Annalen der Chemie und Pharmacie, 1855, xcvi, 24.

ART. XXIII.—*Barometer;* by JAMES LEWIS.

IN the present condition of meteorological science, there is a growing demand for facilities of observation that invites the aid of mechanical appliances which have for their purpose to make automatic records of phenomena practically free from sensible errors. The remarks about to be made are suggested by experiments and calculations that have been made preliminary to the construction of a compensated, self-registering barometer. The form of instrument to which they relate is an improvement upon the apparatus called the "Syphon Barometer." In the course of the experiments referred to, a syphon of steel was made, the dimensions of which are approximately as follows, viz: vacuum chamber—length=4·5 inches; diameter=0·41 inch. Section of tube connecting the vacuum chamber with the base of the syphon—length=28·75 inches; diameter=0·3 inch. A capillary passage through the base connects these parts with the short arm of the syphon. Before filling the tube with mercury, a piston with a porous packing was introduced into the detached and inverted long arm of the syphon, the open end of the tube being embraced by a suitable funnel to prevent loss of mercury. Mercury was then filtered into the tube, above the packing of the piston, until the whole tube was filled, the funnel around its open end embracing an excess. The piston was then made to wipe the walls of the vacuum chamber, and was afterward withdrawn from the tube by a pumping process—wiping and pumping successive portions of the tube repeatedly, the mercury filtering through the porous packing of the piston into a vacuum. Tests for vacuum expanded any remaining bubbles of air, and the pumping process again repeated reduced the tube and its contained mercury to a condition indicative of a perfect vacuum which has maintained its integrity through a period of over six months under circumstances well calculated to test it. The process of boiling, usually deemed essential, was omitted. The operations of this syphon indicate that the diameters of the short arm and vacuum chambers are nearly large enough to remedy the resistance of capilarity and adhesion to the free movement of the mercury. The syphon is supported by an adjusting screw beneath its base. In the short arm of the syphon, and in contact with its contained mercury, is an iron float, the diameter of which where it touches the mercury, is about 0·25 inch. The contact is perfected by amalgamation. The float is suspended by a slender thread of untwisted silk, which passes around a wheel $\frac{3}{8}$ inch in diameter, the axis of which is about 0·02 inch in diameter, and rests on four friction wheels. One end of the axis of the wheel carries an index which points to divisions on a dial graduated to hundredths of an inch; the spaces between the divisions are sufficiently wide to be readily divided into tenths (= thousandths of an inch) by an experienced eye. Behind the index, and attached to the same axis, is a very light skeleton wheel three inches in diameter, the periphery of which is grooved for the reception of a very fine silk cord which supports a light metallic frame carrying a registering point, the movements of which are restricted to a vertical path by means of a very slender guide-wire. The metallic frame and registering-point are rather more than counter-poised by the weight of the float that communicates with the mercury. The amount of friction involved in these parts is slight and does appreciably interfere with the movements of the mercury. The pully which is embraced in this arrangement, and which transmits the movements of the mercury to the registering point, multiplies precisely eight times; but as only one half of the barometical fluctuation is manifested in the short arm of the syphon, the scale of record requires four standard inches for the measurement of a barometrical inch.

The registering apparatus embraces a number of cylinders (the axes of which are parallel with the vertical path of the registering point) for the purpose of moving a fillet of paper six inches wide, so as to receive impressions from the registering point; also, a registering hammer with a radius of $\frac{3}{4}$ inch, consisting of a bar of metal six inches long, revolving about an axis parallel with itself and the axis of the cylinders, so as to impinge on the registering point at any part of its traverse across the fillet of paper. It would be better were these parts designed to embrace eight instead of six inches movement of the registering point. The registering apparatus is operated by clock work which imparts motion at stated intervals, as one hour, a half or a quarter, as may be determined by shifting the position of a lever arranged for that purpose. The announcement may be new that a syphon barometer contains within itself the elements by means of which it may be successfully compensated for temperature.

Compensation, outside of, and separate from the tube itself, is objectionable for two reasons: first, the error of temperature varies with the height of the barometrical column, and also with the quantity of mercury in the syphon—these two conditions determine another condition which establishes compensation *at one point of elevation* of the barometrical column, above or below which the error of temperature is variable, *plus* or *minus*; second, any system of compensation outside of the syphon, will necessarily involve mechanical complexities liable to disturbance by slight causes.

These additions will of course involve additional items of cost, which, if bestowed upon the compensation of the tube itself, would permanently obviate all difficulties.

The question of compensation will now be examined, requiring only the comprehension and aid of the artisan that it may be wrought into a *practical* solution, at once available for the purposes of the meteorologist.

Proceeding upon the assumption that the maximum elevation of the mercury in the barometer is below the limit of 32 inches, let there be supposed a syphon of iron or steel free from flaws, the long arm of which has an elevation of 32 inches, the short arm an elevation of 6 or more inches, their diameters being any convenient unit and equal.[1] The communication between the two arms of the syphon may be a small orifice (say 0·03 inch in diameter) through the base, large enough to permit free passage for the mercury, and small enough to admit of handling the syphon in the necessary manipulations of erection without endangering loss of its contents by a sudden movement. The contents of the passage through the base may be disregarded in the calculations on which the compensation depends. Now, regard the long arm and the passage through the base of the syphon as having been filled with mercury at the temperature of 32° F.

The elements which enter into the compensation of the syphon require to be considered, viz: a unit of volume of mercury at 32° F., becomes at 212°, 1·018018 (Guyot's Tables)$=$A; a linear unit of iron at 32°, becomes at 212°, 1·0012534363 (Hassler)$=$B.

The theoretical elevation of a barometrical column of 32 inches by a change of temperature from 32° to 212° F., will be 32 A. The excess over its former elevation may be expressed by the terms 32 (A$-$1). In the syphon, the iron tube expands by the rate B in each of its three cubic dimensions, two of which (involved in the square of the diameter) *tend to diminish* the elevation of the mercury by enlarging its sectional area, so that the elevation of the column might be expressed by $32\frac{A}{B^2}$, referring to a tube closed at its base and open at top; the excess over its original elevation may be expressed by the terms $32\left(\frac{A}{B^2}-1\right)$. This last expression is less than the unit of excess of elevation due to temperature $=32(A-1)$ which it is necessary should be attained in the long arm of the syphon, to establish compensation, (presuming that it is attainable as a *theoretical* consideration). The decimal proportion of the actual excess of elevation of the mercury in the tubes to the *required* excess of elevation may be obtained from the expression $\frac{32\left(\frac{A}{B^2}-1\right)}{32(A-1}$. In the general application of this expression to the computation of the compensation of the syphon, the expression is $\frac{32\left(\frac{B^2}{A}-1\right)}{H(A-1)}$, in which H is made successively equal to 32, 31, 30, 29, &c., as far as it may be necessary to proceed. The difference in value of the numerator and denominator of this fraction, represents the error of temperature for 212°. Regarding the expression $\frac{32\left(\frac{A}{B^2}-1\right)}{H(A-2)}$ as being but a fractional portion of the required unit of excess of elevation for temperature, it will be understood that the volume of mercury which it represents *may have its altitude increased by diminishing its sectional area.* Thus, if the expression be regarded as a unit of *elevation* but a fraction of sectional area, the square root of the expression becomes a diameter. This process modifies the form of the upper end of the long arm of the syphon, converting it to a truncated chamber of the form of a solid of revolution, the smaller end at top. The lines of vertical section of this solid are curves, the asymptote of which is the axis of the tube. The diameter of the conical chamber, beginning at top and measuring at intervals of half an inch, are in the following values, which relate to the original unit of diameter:

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[1] The minimum diameter should not be less than half an inch; the maximum diameter, on the other hand, need not exceed one inch. A compromise between cost and efficiency may suggest something between these extremes.

0·0	inch.	diameter,	0·926629
0·5	"	"	0·941456
1·0	"	"	0·957017
1·5	"	"	0·973879
2·0	"	"	0·990609
2·5	"	"	1·008785
3·0	"	"	1·028002
3·5	"	"	1·048359
4·0	"	"	1·069975
4·5	"	"	1·092992

The curve which defines the outlines of the chamber is so slight a departure from a right line (0·004433 in the limits of the available part of the cavity) that it may be regarded as a straight line in the calculations which define the proper length of the solid of revolution without involving sensible errors in the compensation. The solid of revolution becomes then a cone. This cone should embrace the same cubic space as an equal length of the original tube, and it must therefore have the same mean sectional area; make x = base of cone, d = truncated top (diameters), the value of x may be found from $\frac{x^2+dx+d^2}{3}=1$. $x=1·071618$. This value occurs between the diameters 1·069975 and 1·092992, and its location is determined by the proportion of its difference from the first of these two stated diameters, to their own difference. The length of the cone by this process is 4·035700 inches, a very small fraction too short (when the curvature is considered) for *perfect* compensation; but the resulting errors are inappreciable.

In the proposed compensated syphon, the interior capacity and mercurial contents are intended to be the same as if the tube were of uniform diameter. The object sought is to secure in the long arm of the syphon the theoretically required elevation due to temperature, so that no errors of temperature will be differentiated between the two arms of the syphon. The conical chamber will accomplish this object by imparting the required elevation at the expense of diameter. But this modification of the upper end of the long arm changes its relation to the previously cylindrical short arm of the syphon, and requires that the short arm should be an *inverted fac simile* of the conical chamber, so that under the influence of barometrical changes, a given portion of the column of mercury transferred from the contents of one arm to the contents of the other shall not undergo any change of vertical dimensions. In effect, the full cubic contents of the conical vacuum chamber will occupy the *same conical space* (at the temperature 32°) at all elevations of the barometrical column, the vacancy in the vacuum chamber being equalled by the space occupied by mercury in the short arm of the syphon.

The tendency of the surface of the mercury to assume a spherical form with increased elevation may require a trifling adjustment. Another adjustment may also be necessary on account of the tendency of the mercury to seek that position in a conical space which least diminishes its sphericity.

At the temperature 32° F. the lower limit of utility of the syphon will be reached with a barometrical pressure of 23·929 inches, the upper limit being 32 inches. The point at which the tube would be self-compensating, if cylindrical, corresponds to the pressure 27·483489 inches at 32°. Above that point the error has the same sign as in the ordinary barometer; below that point the sign of the error is inverted.

The point of self-compensation for a cylindrical tube corresponds to the section of mean diameter, or rather, diameter of mean sectional area of the conical chambers.

The following table exhibits the temperature errors of a *cylindrical* syphon of 32 inches range, for 212°.

23 inches,	Error, 0·081	28 inches,	Error, 0·099
24 "	" 0·063	29 "	" 0·027
25 "	" 0·045	30 "	" 0·045
26 "	" 0·027	31 "	" 0·063
27 "	" 0·009	32 "	" 0·082

Mohawk, N. Y.

Art. XXIV.—*On a new Illuminator for Opaque objects under high powers of the Microscope;* by H. L. Smith, of Kenyon College.

In attempting to study the structure of the diatomaceous frustule, I found it impossible to view it with high powers as an opaque object, by any means hitherto devised. In a valuable paper on the scales of the Podura, (Mic. Jour., N. S., vol. ii, p. 86), Mr. Richard Beck has stated that there is no difficulty in viewing *them* as an opaque object with the ⅛th in. objective and condensers rightly placed. Any illumination of Diatoms thus obtained is almost useless, from the great obliquity of the light, and with powers higher than the ⅛th in. is quite impossible.

Mr. Ross's ingenious arrangement, suggested by Mr. Brooke, of a plain reflector, flush with the front surface of the objective, and receiving light from a truncated ellipsoidal reflector below, is so exceedingly difficult to use, and only with a special mounting of the object, that it has never been generally adopted. Mr. Wenham's method is entirely inapplicable to Diatoms, inasmuch as it depends upon the total reflection of the light from the under surface of the glass cover of a mounted object, and in such case the Diatoms, from their transparency, and the near coincidence of refractive index of silex with that of the mounting fluid, throw back but a feeble light and are nearly invisible. The use of the well known collimating eye-piece suggested to me the idea of making the objective its own condenser, and upon communicating this idea to Mr. Wales, already well known for the excellence of his objectives, he at once sent me a trial instrument. This first illuminator proved so far successful that I was induced to persevere, and with his assistance an "Illuminator" has been constructed which gives entire satisfaction, and answers admirably with all objectives from a $\frac{4}{10}$th to a $\frac{1}{50}$th.

It must be borne in mind that there are certain difficulties to be overcome in this mode of illumination, the chief of which is the reflection of the light from the posterior surface of the back combination of the objective. All the difficulties are now surmounted, and there is no trouble in viewing Diatoms, or other objects, mounted dry, and uncovered, with the *highest* powers of the microscope, and with abundant illumination; and this without any trouble in mounting the object on little discs or pins, but using the ordinary 3 in. by 1 in. slide.

As I do not here intend describing the instrument in detail, I will only say that it consists essentially of a rectangular brass box, having the "Society screw" at the top, to attach it to any microscope tube, and at the bottom, to receive any objective; and so constructed that it can be placed in any position with regard to the light. A brass draw, moved by screw and milled head, slides into the box, and carries a reflector of silver, also movable on its own axis by means of small milled heads; the forward edge of the reflector is curved, and it is concave, having a focus of about six inches. By means of the screw, the curved edge of this reflector can, when adjusted at an angle near 45°, be pushed more or less over the opening at the back of the objective. Opposite to the reflector, and attached to one side of the box, is a revolving circle of diaphragms, of great use in regulating the light, so as to exclude all fog or glare; the apertures vary from $\frac{3}{20}$ths to $\frac{1}{20}$th of an inch.

I had supposed that the idea of making the objective its own illuminator was original, until very recently, when I found that Dr. Riddell had proposed to place Shadbolt's annular ring behind one of the lenses of the objective, and to reflect light, received up by the side of the lens, back down through it.[1] I need not say that this method is entirely impracticable; it would require a very peculiar mounting of the objective, and also of the object, to allow the rays to pass up into the ring in order to be again transmitted down through the objective. Moreover, parallel rays would scarcely answer, as the working distance of the objective is not exactly the distance at which it would condense parallel rays reflected through it. This, however, might be remedied. It is hardly desirable to receive the illumination from all sides. A unilateral light reveals the structure much more clearly. I attempted, at first, to employ as the reflector, a disc of the thin glass, or two or three of them, used as covers of microscopic objects. A very good illumination, without sensible interference with definition, when the glasses are clean and well selected, may thus be obtained; and the reflection, from the posterior surface of the objective lens, may be entirely removed by a Nichol's prism over the eye-piece; but by an illumination produced in this way the field has a peculiar flatness, no shadows are perceived; there is the same difference that one experiences in viewing the mountains and cavities in the moon when she is in quadrature and when she is fully illuminated. Indeed, I have found that nothing can compare with the unilateral illumination from the silver reflector rightly managed. A fixed reflector attached to the objective, has, I believe, been recently proposed by one of the New York opticians as an improvement; but it is very obvious this is far inferior to the adjustable one, which not only answers for different objectives, but, from the very fact of being movable, enables the light to be condensed in the most perfect manner just where it is wanted, and, with the slightest touch, removes all fog and glare. I think it will sufficiently appear that the method I have proposed and carried into effect, is in reality novel, and no modification of Dr. Riddell's, as has been stated. It would be strange if the idea of a reflector behind the objective had not occurred to this ingenious and veteran microscopist; but if so he seems never to have carried it out in practice. In the annual address of the President of the Microscopical Society of London, Feb. 11, 1857, four years after he proposed the annular ring, we find the following: "This is a problem," the illumination of opaque objects under the *highest* powers of the microscope, "the solution of which has been attempted by numerous adepts in manipulation with only very partial success." He then speaks of Mr. Wenham's method as by far the best. If then, Dr. Riddell, or any one else, had previously succeeded in accomplishing so desirable an object, why has it been kept secret? It is not the one who may have had vague ideas as to the possibility of accomplishing certain results who is to be considered as the real benefactor; rather he who puts it in tangible shape before the world, and, surmounting the difficulties of practice, makes the idea a reality. I make these latter remarks because I have heard; with some surprise, that it has been stated, by an optician of deserved celebrity, in a meeting of the Microscopical Society of New York, that the "Illuminator," as constructed by Mr. Wales, is only a poor modification of Dr. Riddell's method.

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[1] See this Journal, [2], xv, 69.

As the "Illuminator" is already in the hands of many, I append a few simple directions as to its use. The objective must be adjusted for an uncovered object, though I find few are rightly marked. An ordinary paper-covered slide, with bits of gold leaf on it, answers admirably as an object to adjust the light. The Illuminator being screwed on to the tube, and the circle of diaphragms placed facing the light, (I find the ordinary coal-oil lamp with flat flame to answer admirably, the flat side being toward the reflector), turn the reflector at an angle of about 45°, and allow the light to enter the largest aperture of the diaphragm. By means of the screw, push the reflector forward nearly as far as it will go. Turn the reflector on the axis of the tube, and on its own axis, until the light, which may be placed 10 or 12 inches to the left of the microscope, and directly opposite the circle of diaphragms, is reflected down on the paper-covered slide, the tube of the microscope being racked up to about the position it will occupy when the objective is screwed on and in focus. The light thus reflected down, should appear just at the curved edge of the reflector, in the axis of the tube, when looking through the tube, the eye-piece being removed. Now screw on the objective, and before replacing the eye-piece bring it into focus. The field will appear brilliantly illuminated, as in using a lens with a lieberkuhn; if not, a slight movement of the reflector, or diaphragm, or light, will quickly accomplish this. Put in the eye-piece and adjust for focus; if the field is not clearly illuminated, say with $\frac{1}{8}$ in. objective, a little fingering of the reflector, or diaphragm, will suffice to effect this. The screw which moves the draw and reflector may now be withdrawn, uncovering all but about a quarter or one half of one side of the posterior lens of the objective; and, if care has been taken to properly adjust the diaphragm and reflector, a most brilliantly illuminated field, free from all fog and glare, will reveal objects with a beauty and clearness inconceivable by those who have never used high powers of the microscope upon opaque objects. The most common objects appear with new and hitherto unsuspected beauty, brilliant not only with their own proper colors, but reflecting iridescent tints from the thin membranes.

The diatoms are especially beautiful, and no one can view, without a sense of profound reverence and unspeakable emotion, the elegant structure of Arachnoidiscus and Heliopelta; of Surirella or Pinnularia.

In thus accomplishing the illumination of opaque objects under the *highest* powers of the microscope, a powerful aid to investigation is furnished, which, I doubt not, will be rightly appreciated.

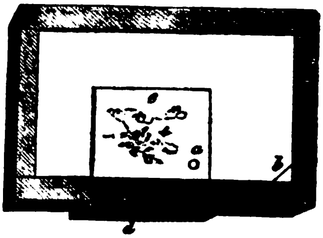
An inexperienced microscopist may find some difficulty at first, but a few trials will ensure success, and when properly used, there is no want of light with the $\frac{1}{12}$th or $\frac{1}{16}$th even with the B or C eye-piece.

ART. XXV.—*On a new Growing Slide for the Microscope;* by H. L. SMITH, Kenyon College.

IN studying the growth and conjugation of the Diatomaceæ, I have felt the want of some means of keeping them alive for a long time under the microscope; and have devised for this purpose, the slide to be described, which appears fully to meet all requisitions; and, as it can be readily made by any tolerably expert microscopist, it will, I am certain, be considered a valuable addition to microscopical apparatus.

The whole slide, as I have constructed it, is a trifle more than $\frac{1}{8}$th of an inch in thickness. It consists of two rectangular glass plates 3×2 in., and about $\frac{1}{25}$ in. thick, separated by thin strips of glass of the same thickness, cemented to the interior opposed faces, as shown in the figure.

This closed cell, ultimately destined to be filled with water, is not of such thickness as to prevent the use of the achromatic condenser, a very important requisite. The glass I use is such as is employed for the small cheap looking glasses, and easily obtained.



The upper plate has a small hole, *a*, drilled through it. This is effected by means of the ordinary writing diamond, and the sharp edge of a broken steel brooch or small rat-tail file. A hole can be drilled through glass of this thickness in a few minutes. One corner of the upper glass is removed, as at *b*, and a small strip of glass cemented at *c* serves to prevent the thin glass cover placed over the object from sliding. Another strip of glass is cemented on the lower side of the cell at *d*, but not extending as far as the removed part at *b*. The object of this is to prevent the water in the cell from being removed by capillary attraction, in case the slide in the neighborhood of *b* should be a little wetted. This strip is not, however, absolutely necessary.

To use the slide, fill the space between the two plates with clean water, introduced at *b*, by means of a pipette, and also place a drop on *a* to remove the air. The object being put on the top of the slide and wetted is now to be covered with a large square of thin glass, *e*, at the same time covering the hole *a*. The slide can now be placed upright, or in any position no water can escape. It is, in fact, only a new application of the old principle of the bird-fountain. As the water evaporates from under the cover more is supplied through the hole *a*, and from time to time an air bubble enters at *b*; thus, a constant circulation is maintained. A cell of the size named will need replenishing only about once in three days, and this is readily effected without disturbing the object. I have been enabled to make observations by means of this slide, which it would have been very difficult, if not impossible, to have made without it.

I had intended to have reserved the description of this and some other microscopic apparatus until I had completed my investigations upon the growth of the Diatoms, in order to publish all together; but new and unexpected developments will so delay this publication, that I have deemed it best to describe the slide at once, that others might have the benefit of it as well as myself.

Note.—I consider it an act of justice to a young artist, to mention the very remarkable performance of a $\frac{4}{10}$th objective lately received from Mr. Wales.[1] This objective is furnished with two backs, one for direct, the other for oblique light. This mode of correction, which arose from some correspondence between Mr.

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[1] The address is W. Wales & Co., Fort Lee, Bergen Co., New Jersey.

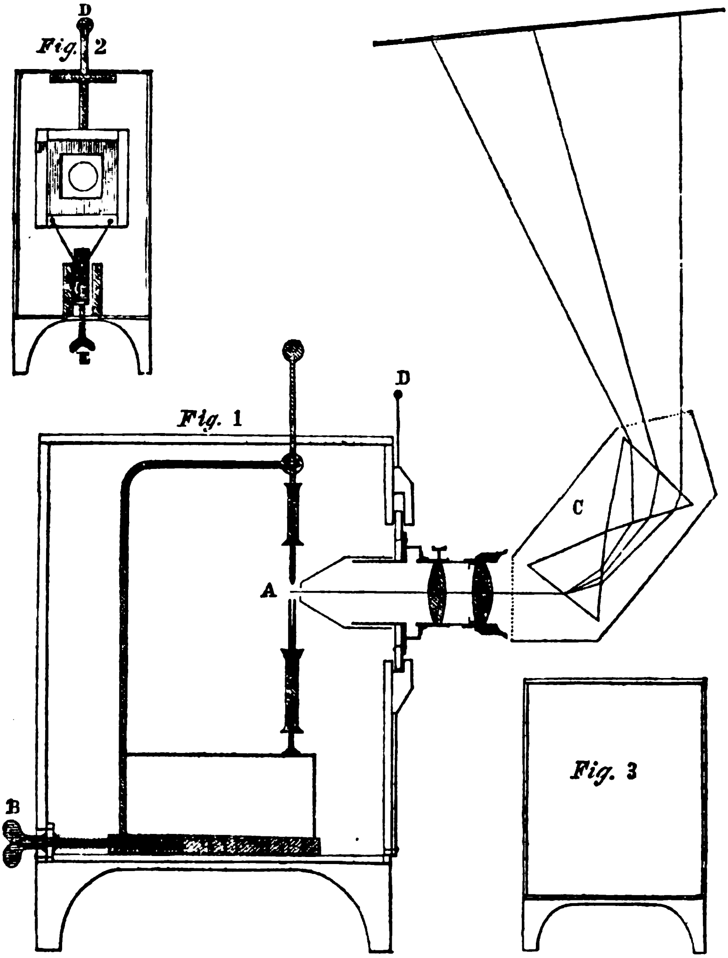
Wales and myself, appears to be more effective than that of a double front, as adopted by Mr. Tolles. The glass in question has a mean aperture of 115° with the direct light back, and of 120° with the oblique. The definition with the former, upon Diatoms and Confervæ, is truly magnificent; and upon the prepared Diatoms it is far superior to a fine Smith & Beck $\frac{1}{8}$th, not only with direct, but with oblique light. With the oblique back the definition is equally fine when using direct light, but the objective being now a little under-corrected it is not as pleasant. With oblique light, however, especially very oblique light, it is amazingly sharp and clear and most beautifully corrected. Thus, I have no difficulty in resolving with it the Hyalodiscus Californicus, of Bailey, or Grammatophora subtilissima, of Greenport, as well as the delicate Cuban Amphipleura, now well known to many American microscopists, through specimens distributed by Mr. W. Sullivant and myself, and which I have never been able to resolve with any English objective under an $\frac{1}{8}$th. The working distance of this objective is more than $\frac{1}{25}$th of an inch!

The resolutions above named were effected by the concave mirror of the microscope-stand, and not by any special contrivances for oblique illumination. It is, however, for their beautiful performance with direct light, I most value Mr. Wales's objectives. I have a $\frac{1}{5}$th, which, on Podura, excels anything I have yet seen of like power in exquisite definition. Both this and the $\frac{4}{10}$th objective bear the highest eye-pieces without loss of definition or want of light. I need hardly say that a $\frac{4}{10}$th which thus defines and resolves objects, considered as fair tests for an $\frac{1}{8}$th but a year or two ago, is a wonderful specimen of optical skill.

Art. XXVI.—*On the Projection of the Spectra of the Metals;* by Prof. Josiah P. Cooke, Jr.

Spectrum analysis has become, in all its bearings, a subject of so much importance both in chemistry and physics, that a simple and efficient method of exhibiting the phenomena to an audience is an important need to all teachers of physical science. Having tried several plans without obtaining the definiteness or brilliancy required, I finally devised the following, which has been completely successful. By placing my lantern at the distance of from 50 to 60 feet from the curtain, I obtain a spectrum from 20 to 25 feet in length and about 18 inches in breadth, with all the brilliancy that can be desired and without any diffused light, which greatly impairs the beauty and distinctness of the phenomena as they have been hitherto exhibited. My apparatus may be best described under three heads: First, the electric regulator; secondly, the lantern with the adjustable slit and lenses; and, lastly, the prisms for dispersing the light.

The light is obtained by means of a powerful galvanic battery. I have found that fifty Bunsen cells of medium size (about 7 inches high) were quite sufficient. The metal whose spectrum is required, is volatilized in the voltaic arc and almost



any of the numerous electric regulators may be used for the experiment. I used for a long time a Deleuil regulator, and in fig. 1 a section is shown of this regulator in connection with the rest of the apparatus. I now use, however, a regulator better adapted to the purpose, made by J. Duboscq, of Paris. The negative pole, which is the lowest, is formed of a small cylinder of hard coke, having a small cavity at the top to receive the metal. The positive pole is formed of a coke pencil such as is used in most electric lamps. The poles are so arranged that they can be raised or lowered at will, and the milled heads, which regulate the motion, are placed for convenience at the base of the instrument. The coke cylinder forming the negative pole is fastened to the rim of a circular brass disk, which also supports four other similar cylinders. So that, by turning the disk, one after the other may be brought under the positive pole, and different metals volatilized in the voltaic arc without further adjustment.

The electric regulator is placed within a dark lantern, as shown in fig. 1, resting on a movable platform, which can be moved backward or forward by the screw B. The lantern is made of wood like a square box, see figs. 1, 2 and 3, with doors on two opposite sides as large as the box will permit. The upper part of each door is made of wood and hung on hinges in the usual way; but from this is suspended a thick cloth curtain, which, while covering the lower part of the doorway, enables the experimenter to adjust the poles without opening the lantern. The wooden doors, moreover, are glazed with very deeply colored glass in double plates of blue over red through which the experimenter can see the carbon poles when ignited by the galvanic current.

The electric regulator is placed in the lantern so that the carbon poles shall be directly behind and within half an inch of an adjustable slit, which should be about half an inch high and $\frac{1}{16}$th to $\frac{1}{8}$th of an inch wide. This slit forms a part of a brass cap which slides on one end of a brass tube, while into the other end is screwed a camera tube such as is used by photographers for taking portraits. The lenses of the combination which I use are three inches in diameter, and the focal length is about 5½ inches. This size cannot be advantageously exceeded, but the shorter the focal length of the combination the better. The slit is placed at the focus of the combination, and the position of the lenses with reference to the slit is adjusted by means of the rack and pinion usually attached to camera tubes. The brass tube with the slit and lenses just described is fastened to a thin board, which slips into a movable wooden frame on the front of the lantern, (see fig. 2), a portion of the tube with the slit projecting into the box, as shown in fig. 1. The object of the movable frame is to adjust rapidly the position of the slit with reference to the voltaic arc, so as to keep the slit constantly in front of the luminous flame, which, as is well known, shifts its position from one side to the other of the carbon poles in a most irregular manner. The means of adjustment just referred to are shown in fig. 2.

The frame F is connected by iron bars with a pivot which turns in the wooden block G. Moreover, to the top of the frame there is attached a flat iron bar, which ends in the handle D, a stout steel spring attached to the front of the box serving to keep the bar in place although permitting a slight lateral motion. By moving then the handle D the slit with the lenses may be moved to one side or the other, as the position of the flame may require. Finally, the bar G slides between two grooved wooden bars, and may be moved by the screw E. Thus the frame may be raised or lowered, and the slit kept constantly midway between the carbon poles.

It remains now to describe the prisms, by which the light is dispersed. I use for the purpose two large sulphid of carbon prisms having a refractory angle of 60°. They were made from a large and thick glass tube three inches in diameter, with faces of ordinary plate glass cemented to the cells with a mixture of glue and molasses. They are securely fastened in a wooden box, which is made of such a shape that when the light passes at the angle of least deviation the rays will enter the box perpendicular to one side, and will leave it perpendicular to another. The box is closed except at the two ends, in which circular apertures are made three inches in diameter, and these can be closed with covers when the prisms are not in use. A section of the box and prisms is shown at C, fig. 1. It is here drawn on the same plane as the section of the lantern, but when in use it is at right angles to this plane.

Having a knowledge of the various parts of the apparatus, the method of using it can be readily understood. The lantern having been placed on an elevating stand with a revolving table[1] at a distance of 50 or 60 feet from a curtain or white wall, the prism box is placed at one side on a similar table, and adjusted so that, while the axis of one of the circular openings is perpendicular, or nearly so, to the screen, the axis of the other opening will coincide with the axis of the lenses when the lantern is turned on the revolving table through the required angle; see fig. 1. The lantern being now directed to the screen, the electric regulator having been adjusted so that the point of contact of the carbon poles is in the axis of the lenses and about two inches behind the slit, and the galvanic circuit having been closed so as to produce the electric light, the focal distance of the lenses is so adjusted as to form a distinct image of the slit on the screen. We then turn the lantern on the revolving table so that the light shall pass through the prism, and we have at once the projection of the spectrum in all its beauty over 20 feet in length and 18 inches or more in breadth. To produce now the spectrum of a metal we first separate the poles, and then bring them, by the aid of the screw B, within half an inch of the slit. Having now placed a small piece of the metal selected for the experiment in the cavity of the coke-cylinder, we again bring the poles in contact and slowly separate them as the heat increases, until the light emitted by the ignited carbon is wholly intercepted by the edges of the slit and the lenses are illuminated only by the ignited vapor between the poles. We then have the spectrum of the metal, which continues in perfect purity so long as the space between the poles remains filled with the metallic vapor. When, however, the supply diminishes, either in consequence of the consumption of the metal, or on account of the failing strength of the battery, the bright bands, which distinguish the elements of the air, will appear at the same time with those of the metal. The metals which I have found best adapted to these experiments, are copper, zinc, brass (which gives the bands of copper and zinc together), mercury, thalium, sodium, lithium, potassium, cadmium, antimony, lead, gold and silver; and of these the first seven give the most characteristic and brilliant results. I use them all in the metallic state with the exception of lithium, which I use as carbonate, moistening the salt before placing it on the pole. It is important to have a separate coke cylinder for each metal, as otherwise, unless the cavity is most carefully cleaned out between the experiments—and for this there generally is no time—the spectra will be confused by the reappearance of the bands of the metals previously used.

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[1] The stands used by photographers are well adapted for the purpose.

These experiments can be varied by projecting on the screen the image of the carbon poles with the stream of glowing vapor between them, and thus showing the color of the light before it is decomposed; and for this purpose it is only necessary to direct the lenses to the screen, and, having removed the brass cap, which carries the slit, to re-adjust the focal distance of the lenses.

The reversal of the sodium band can also be readily shown. For this purpose we arrange the apparatus so as to produce a continuous spectrum, as first described. We then interpose between the poles and the slit the flame of a Bunsen lamp, and in this flame we insert a small spoon containing metallic sodium. A dark line soon appears crossing the yellow portion of the spectrum in the position of the sodium band. It is convenient to have a small shelf in the box, to support the lamp, so hung on hinges that it will drop out of the way when its support is removed.

Again, the formation of dark lines crossing the spectrum, similar in appearance to the solar lines, can be shown by interposing between the lenses and the prism a vessel with parallel glass-sides containing hyponitric acid or iodine vapor. Such a vessel is easily made from a piece of glass tubing—three inches in diameter and four inches long—by cementing plates of glass to the open ends and drilling a hole through the sides, into which a glass stopper may be fitted.

Finally the apparatus may be with a little additional expense so constructed that it can also be used for projecting photographic transparencies after the principle of the magic lantern. Small photographs on glass may thus be used in place of diagrams and the great geological features of our globe, the glaciers for example, may in this way be brought before the eyes of an audience with almost all the vividness of the reality. The same method of illustration will be found of great value in teaching other sciences. For instance, the best way of giving an idea of the dark lines of the solar spectrum is to take a photograph of the more refrangible portion and project the image on the wall. Such photographic transparencies are easily made; but as few teachers have the means or time for such work, it would be well if some professional photographer would turn his attention to this department of his art. If the instrument here described is to be used as a magic lantern it is necessary to have, besides the regulator above described, a second adapted for giving continuous light. We have used for some time the regulator now made by J. Duboscq of Paris, after the plan of Foucault, and we find that it works very well.

Most of the apparatus here described is so simple that it can be made by any good mechanic and for this reason we have entered into more detail than would otherwise be necessary. The lenses and other accessories must of course be purchased. The apparatus can also be ordered from E. S. Ritchie & Co. of Boston.

Cambridge, August 8th, 1865.

ART. XXVII.—*On the use of the Bisulphate of Soda as a substitute for the Bisulphate of Potash in the decomposition of minerals, especially the Aluminous minerals;* by J. LAWRENCE SMITH, Professor of Chem., University of Louisville.

IN referring to the more recent works on analytical chemistry, I perceive that the bisulphate of potash is still used to the almost utter exclusion of bisulphate of soda in rendering certain minerals soluble; and it is still recommended as the proper agent to fuse with aluminous minerals, as corundum, emery, &c.

This subject occupied my attention to a considerable extent when engaged in the preparation of two memoirs on the geology and mineralogy of emery presented to the French Academy of Science in 1850,[1] as well as in some investigations I am now making on the emery from Chester, Mass. In the above researches I had a large number of corundums and emeries to analyze. The powdered minerals were fused with the bisulphate of potash in the usual way, and I found no difficulty in decomposing the minerals; but unfortunately during the operation a double salt of potash and alumina is formed which is almost insoluble in water, or in the acids; and it is only by a solution of potash that it is first decomposed and afterward redissolved. There are many disadvantages and delays attendant upon this method, which experience soon exhibits—as the constant deposition of alum if the solution is not kept quite dilute. I therefore experimented with the bisulphate of soda, knowing that the double salt of alumina and soda was quite soluble, and my results were everything that could be desired; for while the soda salt gives a decomposition at least as complete as the potash salt, the melted mass is very soluble in water, and in the future operations of the analyses there is no embarrassment from a deposit of alum. The manner of employing the bisulphate of soda in the analysis of emery will be referred to in a future article on the emery of Chester, Mass.

Preparation of the Bisulphate of Soda.—The ordinary commercial article is not sufficiently pure for use, and I prepare it from pure carbonate of soda or sulphate of soda that has been purified by recrystallization. In either instance pure sulphuric acid is added in excess to the salt in a large platinum capsule, and heated over a flame until the melted mass, when taken up on the end of a glass rod, solidifies quite firmly. The mass is then allowed to cool; moving it over the sides of the capsule will facilitate this operation. When cool it is readily detached from the capsule, is then broken up and put into a glass stoppered bottle. So far as my experience has yet gone, in almost every instance where we have been in the habit of using bisulphate of potash, the bisulphate of soda can be substituted.

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[1] See this Journal, vol. x, 1850, and vol. xi, 1851.

ART. XXVIII.—*Altitudes of Shooting Stars observed on the night of Nov. 13–14th, 1863, at Washington, Haverford College, Germantown, Philadelphia, and other places.* Computed by H. A. NEWTON.

A BRIEF notice of observations made upon shooting stars on the night of Nov. 13–14th, 1863, was given in this Journal, [2] xxxvii, 141–145. A collation of the data thus accumulated shows that seventy-eight meteors were observed at two or more places so that we can compute with more or less accuracy their paths. The following table gives the altitudes above the earth's surface in statute miles (one mile =1609 meters) of the meteors at their first appearance, and at their disappearance. In the second column is the hour of the day, and in the fifth column are numbers designed to express the measure of confidence which I have in the determinations of the altitudes of the middle points of the paths. These numbers range from 1 to 10, and depend upon the probable accuracy of the observations and the positions of the paths with respect to the observers.

More than half of these altitudes were computed from observations made by the assistants in the United States Naval Observatory at Washington, and by a party under the charge of Prof. Gummere at Haverford College near Philadelphia. Frequent use has been made, however, of observations at Washington by the party from the Coast Survey Office under charge of Mr. Schott, of those at Germantown by Mr. Marsh, at Philadelphia by Mr. Vail, at West Town, Pa., by Mr. Hoopes, and elsewhere by other persons. The Connecticut Academy of Arts and Sciences, at whose instance the observations were made, intend to publish them in detail. It has been thought best however to publish this table in advance in order that the information which it contains may be in the possession of those who desire to observe the meteors on the return of the anniversary, the morning of Nov. 14th prox. There were on that morning in 1863 more shooting stars than usual, and there was a decided radiation from the sickle in Leo. We have some reason to expect a still greater increase this year.

For several numbers of the table the altitudes are not given. The observations were not in those cases entirely reliable, and only the altitudes of the middle points of the visible paths were computed. They were as follows:

No.	10	mid. alt.	143	miles,	w't	3	No.	37	mid. alt.	50	miles,	w't	1
"	13	"	113	"	"	1	"	51	"	37	"	"	–
"	14	"	69	"	"	–	"	58	"	54	"	"	6
"	19	"	72	"	"	3	"	61	"	135	"	"	2
"	30	"	72	"	"	3							

Table of altitudes (in statute miles) of shooting stars observed at Washington, Haverford, &c., on the night of Nov. 13–14th, 1868.

No.	Hour.	1st alt.	2d alt.	Weight	No.	Hour.	1st alt.	2d alt.	Weight.
1	9·2	57	49	8	40	14·0	61	43	3
2	9·4	65	30	5	41	14·2	91	60	8
3	10·8	86	36	2	42	14·3	94	64	5
4	12·0	93	46	5	43	14·4	87	20	2
5	12·0	79	68	8	44	14·4	203	126	2
6	12·0	87	73	7	45	14·5	114	80	7
7	12·2	121	97	5	46	14·5	93	45	6
8	12·3	86	78	3	47	14·5	113	67	9
9	12·5	104	62	9	48	14·6	200	130	1
10	12·5			3	49	14·8	57	28	7
11	12·5	145	110	4	50	14·8	156	90	6
12	12·6	85	20	6	51	14·9			
13	12·7			1	52	14·9	49	37	3
14	12·7				53	14·9	184	115	2
15	12·8	109	82	7	54	15·0	141	93	4
16	12·9	151	113	4	55	15·1	112	72	10
17	12·9	102	70	5	56	15·1	90	46	3
18	13·0	83	58	8	57	15·1	50	23	3
19	13·0			3	58	15·1			6
20	13·0	51	40	7	59	15·2	105	49	6
21	13·0	113	83	4	60	15·2	71	47	10
22	13·0	98	60	4	61	15·2			2
23	13·1	142	110	7	62	15·3	79	41	5
24	13·2	121	53	7	63	15·3	188	110	3
25	13·4	96	56	2	64	15·3	82	55	4
26	13·5	90	72	9	65	15·3	112	82	6
27	13·5	94	66	7	66	15·4	94	39	7
28	13·5	102	62	6	67	15·4	99	69	10
29	13·6	136	80	5	68	15·5	101	50	10
30	13·6			3	69	15·5	90	66	8
31	13·6	75	43	9	70	15·5	79	51	7
32	13·7	59	45	9	71	15·7	92	52	8
33	13·8	83	47	9	72	15·8	174	68	4
34	13·8	90	62	4	73	15·8	106	61	4
35	13·8	104	64	4	74	16·0	104	66	7
36	13·8	68	52	3	75	16·0	106	50	9
37	13·9			1	76	16·1	120	72	8
38	13·9	92	45	10	77	16·2	79	66	7
39	14·0	72	50	2	78	16·4	80	42	8

If the several altitudes in the third column of the table be multiplied by the corresponding *weights*, and the sum of the products be divided by the sum of the multipliers, we have the mean altitude of the shooting stars at first appearance, that is, 96·2 miles, or 154·9 kilometers. In like manner we find for the mean altitude at extinction 60·8 miles, or 97·8 kilometers. The mean therefore for the middle points is 78·5 miles, or 126·4 kilometers. The nine special cases give about the same result.

If the altitudes be taken in seven groups, the several means for the middle points will be 81·1, 82·4, 73·4, 74·9, 80·9, 78·0, and 79·2 miles. These numbers indicate that there is no essential difference of altitude for different hours of the night.

The altitudes given in the table are represented to the eye in the first division of the diagram on p. 253, by a method first employed, I believe, by Mr. A. S. Herschel. The distances of the two extremities of each line from the base line, A B, represent respectively the altitudes above the earth's surface of the corresponding shooting star at its appearance and disappearance. The length of the line represents therefore the amount of descent, not the length of the path. The dotted lines stand for those meteors of which the heights of only the middle points are computed. At the end of this division of the diagram is a heavy line that represents the mean of all the paths as computed above.

In the second division of the diagram is given in like manner a representation of the paths of 39 meteors observed at New Haven, Hartford, Williamstown, Wolcottville, Albany, &c., on the night of Aug. 10–11th, 1863, (this Journal, [2] xxxvi, 303). The last heavy mark represents the mean. The mean of the first altitudes is 69·9 miles, or 112·4 kil., and that of the last altitudes is 56·0 miles, or 90·1 kil., that of the middle point 62·9 miles. The computations of the observations made at that time are incomplete. The results thus far obtained, and here represented, agree well with previous determinations. Thus the mean altitude of the middle points of meteor paths deduced from the table in this Journal [2], xxxviii, 136, is found to be not quite 60 miles, (this Journal [2], xxxix, 194). Mr. A. S. Herschel from the same table, and from observations since published, finds a mean altitude for appearance 73 miles, for disappearance 52 miles, (Ast. Soc. Notices, March 10th, 1865). The large majority of the paths from which these results are obtained belong to August meteors.

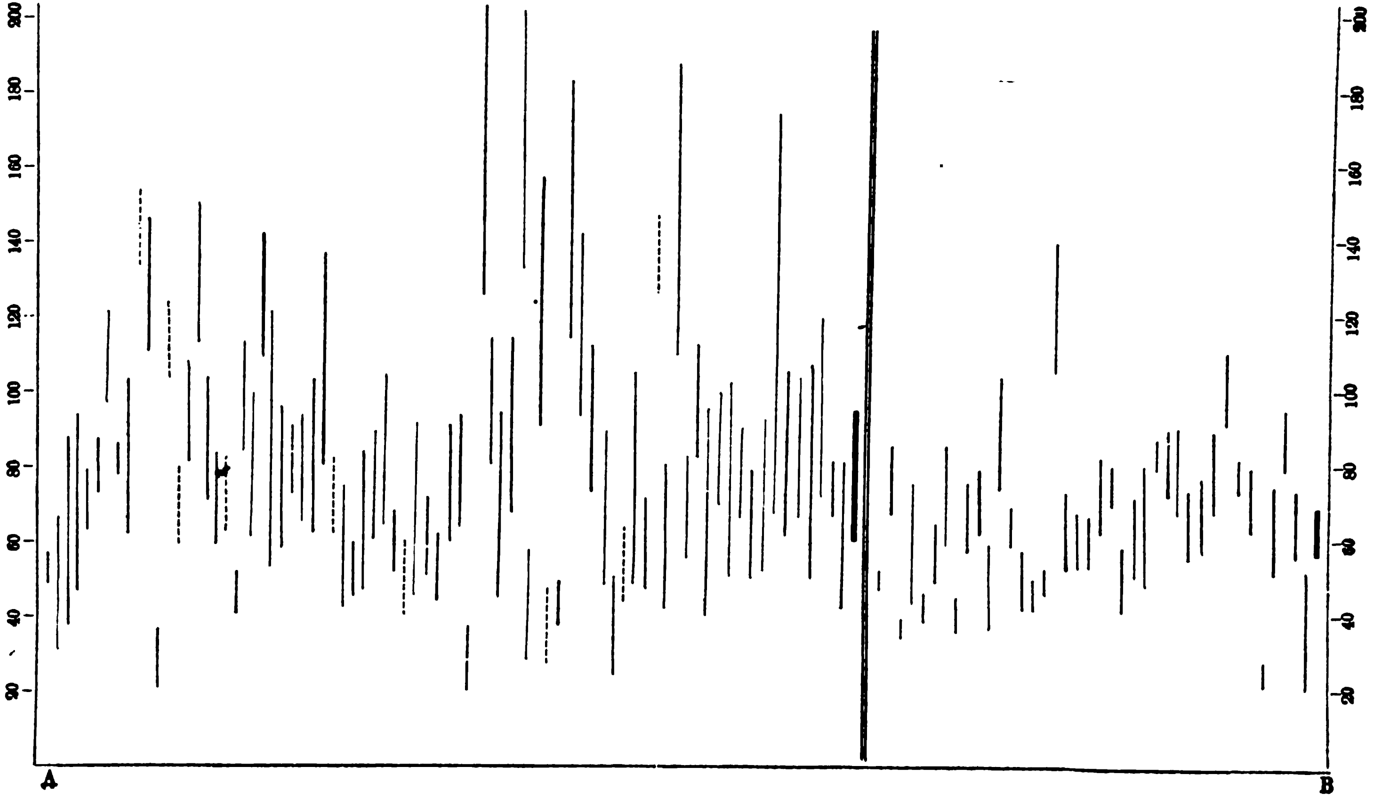
It appears, then, that the region in which the November meteors appear and disappear, is 15 or 20 miles higher than the corresponding region for the August meteors. If the decrease of density of the atmosphere at this elevation follows the same law as near the earth's surface, the air in the latter region is forty or fifty times as dense as in the former.

The most plausible explanation of this remarkable fact is, that the two groups of bodies differ in their chemical and mechanical constitution; the November group being more inflammable than that of August.

It is altogether unlikely that any of the meteors became visible at a greater altitude than 125 or 150 miles. The facility for error in observing is very considerable. It seems impossible however to explain in this way the large difference between the means of the computed altitudes of the two groups.

Altitudes of shooting stars, Nov. 13–14th, 1863.

Alt. of shooting stars, Aug. 10–11th, 1863.



Art. XXIX.—*Remarks on Gravitation, and its relation to a supposed Universal Force;* by Henry F. Walling.

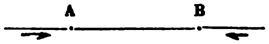
The determination of the co-relation between gravity and the other known physical forces has been the object of numerous delicate experiments and investigations by many eminent physicists, but this object has not yet been attained. There is besides an apparent conflict between the law of conservation of forces and the inherent attraction theory of gravitation.

It has occurred to me that the latter difficulty may be overcome by the simple supposition of a primary universal force, with which the co-relation of the other forces may hereafter be established.

This universal force may be supposed to fill all space, either associated with the universal ether, or independent, and to be exerted along every possible line in both of its directions, forming rays of force passing through every point in all directions. Inert or gross matter will be acted upon by it, and the composition of all the rays of force which act upon a given body will impart motion to it in the direction of the resultant force, passing through its center of gravity.

Let us now suppose a single atom of matter to exist in space. We shall readily perceive that there will be no resultant force since the atom is acted upon in every direction alike, each impinging ray being counteracted by its opposite ray, producing an equilibrium, in which we may suppose the force of each impinging ray to be decombined or developed from the atom by its counteracting ray so that the general store of force remains unchanged.

But let there be two atoms at a given distance from each other, as A and B in the figure, which we will suppose to be indefinitely small, so that only one line of force rays may be considered as passing through the two atoms. This line will be occupied by two forces in opposite directions as represented by the arrows. It will be seen that each atom intercepts the ray which passes through it before reaching the other, and thus a portion of the counteracting power of the ray is removed, so that an equivalent resultant force acts upon each atom in the direction of the other. This action continues during each successive instant producing uniformly accelerated motions of both atoms toward each other.

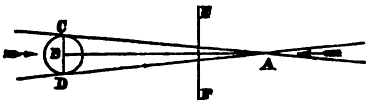


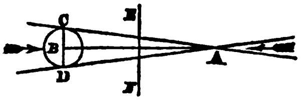
Let us suppose one of the atoms A to be doubled in quantity of inertia, B remaining as before; then the gravitating force of each toward the other will be doubled, for A having twice the combining capacity it had before, and being otherwise under similar conditions, will absorb twice as much force, and B having a double amount of counteracting force removed, will also combine with twice as much force as before, during each successive instant of time, so that B will pass over twice the distance that A does in the same time, each having a uniformly accelerated motion toward the other.

We may consider the entire amount of force traversing any given line to be infinite, and it follows that the successive abstractions of definite amounts from it will not appreciably diminish its proportions to finite quantities. Any number of atoms, therefore, possessing equal quantities of inertia, placed along a given line, and having equal amounts of counteracting force removed, will virtually combine with equal amounts of force from the same ray in equal times.

It evidently follows that any two bodies of matter placed at a given distance from each other, and acted upon by a given number of parallel force rays in the two opposite directions, will, during each successive moment, acquire equal amounts of force impelling them toward each other, proportional to the products of the units of inertia of the two bodies, the square of the velocity of each at any given instant being proportional to the mass of the other.

The proportion of the inverse squares of the distances readily follows, when we consider the relations of one body to each of the several atoms of the other. The rays which act upon the body in the direction of the atom, form a pencil centering at the atom and the counteracting rays form another equal pencil from the same center in the opposite direction. The aggregate quantity of power produced by each pencil at different distances will be proportional to the area of a figure which the rays passing through a given section of the body, cutting its center of gravity, will project upon a plane at a fixed distance from the atom, and parallel to the given section. This may be seen by inspecting the figures, in which A represents the atom, B the





body, C D the given section of B, and E F the parallel plane, at a fixed distance from A. The area of the projected figure upon E F will of course be in inverse proportion to the square of the distance A B, and since the same proportion holds between either body and each several atom of the other, it holds good between the two bodies referred to their centers of gravity.

A growing dislike to the doctrine of inherent attraction, has existed since the days of Newton, who shared the feeling as may be seen in the following quotation made by Faraday in connection with some recent remarks of his own, showing the want of harmony between this doctrine and the conservation theory.

"That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance, through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent, acting constantly according to certain laws; but whether this agent be material or immaterial I have left to the consideration of my reader."

The idea of an inherent power of matter enabling it to pull distant and disconnected matter toward it, seems somewhat analogous to the old idea of an inherent suction power in the piston of a pump, by which it was supposed to raise the water below it.

The apparent annihilation of power in the equilibrium of forces in a statical condition, and the deficiency in the total power of a resultant force, made up of forces in different directions compounded, may be accounted for by supposing the eliminated forces to be mutually decombined from the body acted upon, and added to the general store of universal force. This takes place during each successive instant of the time in which the opposing forces continue to act. When an entire or partial removal of a given number of counteracting or modifying forces takes place, a force which would exactly counteract the resultant of the removed forces is converted from "potential" into "actual energy," and produces corresponding motion. The changes in the velocity and consequent power of motion of the heavenly bodies are therefore due to the variable amounts of force alternately drawn from, and restored to, the great reservoir of universal force.

The constant mutual transmission of the radiant forces, including light, radiant heat, &c., which apparently takes place from the surface of all bodies, across vacant spaces, or through transparent or diathermanous mediums, can perhaps be explained by supposing the universal force rays to be modified by the molecular actions going on within the bodies, which produce vibrations of such a nature, that at the surface of the bodies small amounts of counteracting force are alternately added to, and then partially withdrawn from, the universal force rays, in minutely brief periods of time, endowing the developed or decombined rays with the power of impressing vibrations upon the surface atoms of any matter which they may encounter, by determining the resultant force which will take effect upon them. These vibrations are equal in duration and intensity to those by which they are generated, though perhaps opposite in direction, and result in an augmentation of the centrifugal or expansive motions of the atoms, which is communicable from atom to atom and convertible into "modes of motion." The differences in the constituent parts of rays of light and heat, developed by decomposition, polarization, &c., and due to differences in the duration, direction, &c., of the generating vibrations, are thus transmitted and reproduced with rigorous integrity, being equal in duration, direction and intensity to the generating vibrations, and in quantity or aggregate effect, inversely proportional to the squares of the distances.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On a new and very powerful thermo-electric battery.*—In a communication to the Vienna Academy dated the 16th of March of the present year, S. **Marcus** has described a new thermo-electric battery which possesses extraordinary interest both in a theoretical and practical point of view. The properties of the new battery are as follows:

(1.) The electro-motive force of one of the new elements is equal to $\frac{1}{25}$ of that of a Bunsen's element of zinc and carbon, and its internal resistance is equal to 0·4 of a meter of normal wire.

(2.) Six such elements are sufficient to decompose acidulated water.

(3.) A battery of 125 elements evolved in one minute 25 cubic centimeters of mixed oxygen and hydrogen, although the decomposition took place under disadvantageous circumstances, as the internal resistance of the battery was much greater than that of the voltameter in the circuit.

(4.) A platinum wire of $\frac{1}{2}$ millimeter in thickness introduced into the circuit melted.

(5.) Thirty elements develop in an electro-magnet a lifting power of 150 pounds.

(6.) The current is generated by warming only one of the contact-sides of the elements and cooling the other by means of water of the ordinary temperature.

As positive metal in these batteries, Marcus employs an alloy of 10 parts of copper, 6 of zinc and 6 of nickel. The addition of one part of cobalt increases the electro-motive force. For the negative metal he uses an alloy of 12 parts of antimony, 5 of zinc and 1 of bismuth. The electro-motive force of the alloy is increased by repeated fusion. In place of these alloys a particular kind of German silver, known as Alpacca, may be used with the same negative metal; or, as the positive metal, an alloy of 65 parts of copper and 31 of zinc, and, as the negative metal, an alloy of 12 parts of antimony and 5 parts of zinc. The bars are not soldered but screwed together. The mechanical arrangement is such that only the positive metal is directly heated, the negative metal being warmed by conduction; the former melts at about 1200° C., the latter at about 600° C.

An interesting fact in relation to the transformation of heat into electricity in the thermo-electric battery, is that the water which serves to cool one of the contact sides of each element, becomes very slowly warmer so long as the circuit remains closed, but is heated pretty rapidly when the circuit is open. The alloys employed in this battery fulfill several conditions essential to the production of powerful electrical currents by heat. These conditions are, that the metals employed should be as far as possible from each other in the thermo-electric series; that they should permit great differences of temperature so as to avoid the necessity of using ice; that they should not be expensive, and that the insulating material should resist a high temperature and possess sufficient solidity and elasticity. The thermo-electric battery in question was constructed with reference to the use of a gas flame. The single element consists of bars of unequal dimensions, the positive bar being 7″ long, 7‴ broad, and $\frac{1}{2}$‴ thick; the negative 6″ long, 7‴ broad, and 6‴ thick. Marcus puts together 32 elements in such a manner that all the positive bars are on one side and all the negative bars upon the other and have thus the form of a grating. The battery consists of two such gratings which are screwed together in the form of a roof and strengthened by an iron bar, mica being used as the insulator. The under sides of the elements are cooled by a vessel of water. The whole battery has a length of two feet, with a breadth of six inches and a height of six inches. Marcus has constructed a furnace which is calculated for a battery of 768 elements, which would correspond to a Bunsen's battery of 30 pairs and consume 240 lbs. of coal per day. The Vienna Academy, recognizing the importance of the discovery, has voted to the inventor the sum of 2500 gulden —the invention to be public property.—*Pogg. Ann.*, cxxiv, 629, April, 1865. W. G.

[NOTE.—The importance of Marcus's invention in a technical point of view can hardly be overestimated, since it promises to furnish the cheapest method of obtaining an intense light for light houses and public buildings, and even holds out a prospect, perhaps not remote, of applications in domestic economy.

It must be remembered that the step taken by Marcus is, after all, a first step in the right direction. Bunsen, E. Becquerel, and Stefan, have shewn that there are thermo-electric combinations of much higher electromotive force than those employed by Marcus, although the internal resistance is too great to permit of their use in constructing large batteries. If the progress of science should make us acquainted with metallic alloys which, when combined and arranged as thermo-electric elements, develop electro-motive forces as high as $\frac{1}{10}$th of that of a Bunsen cell, the thermo-electric battery will again become a new instrument. In this connection we suggest that the thermo-electric relations of the highly crystalline alloy of iron, manganese and carbon, known as "spiegeleisen," (that from the Franklinite of New Jersey for example,) deserve a careful study. The possession of a galvanic battery in which coal is consumed in place of zinc and acids, can hardly fail to revive an interest in electro-magnetic motive engines, like that of Page, even if only for cases in which comparatively little power is required, since our best steam engines do not yield 10 per cent of the work which the consumption of the coal is capable of doing.—W. G.]

2. *On some thermo-electric elements of great electro-motive power.*—Stefan has examined a variety of mineral substances with relation to their thermo-electric power at high temperatures. The mineral to be examined was placed upon one end of a strip of copper while the end of a copper wire rested upon the mineral, the whole being pressed together to insure contact. The wire and copper strip were connected with a galvanometer of great resistance and the copper strip was then heated by a spirit-lamp. In examining the mutual relations of the minerals a copper strip was placed between them, wires attached to the free ends of the fragments of mineral and the whole pressed together by a wooden press. The free end of the copper strip was then heated and the heat conducted to the minerals. In the following enumeration of the elements employed the positive element is always placed first, and the number appended signifies how many of the elements give an electro-motive force equal to that of Daniell's cell.

(1.) Foliated copper pyrites—copper; 26.
(2.) Compact " " ; 9.
(3.) Pyrolusite—copper; 13.
(4.) Compact copper pyrites—foliated copper pyrites; 14.
(5.) Copper—crystallized cobalt pyrites; 26.
(6.) Granular cobalt pyrites—copper; 78.
(7.) Copper—iron pyrites; 15·7.
(8.) Compact copper pyrites—iron pyrites; 6.
(9.) Foliated copper pyrites— " ; 9·8.
(10.) Copper—erubescite; 14.
(11.) Fine bleischweif—copper; 9·8.
(12.) Coarse " —copper; 9.
(13.) Galena in large crystals—copper; 9·8.
(14.) Bleischweif—erubescite; 5·5.

The great influence of structure upon the thermo-electric relations is seen in Nos. 1, 2 and 4, and still more in 5 and 6. A mass of cubical crystals of galena was at some points negative, at others positive, to copper. The element No. 14 has the greatest electro-motive force yet observed in thermo-electric series; but the substances employed are all bad conductors. The author considers—and we think very justly—the above results as of great importance for the physics of the earth, and proposes to continue the subject. In a note to Stefan's paper, Poggendorff calls attention to an observation of Marbach made in 1857, according to which crystals of iron pyrites ($Fe\,S_2$) and of cobaltine ($Co\,S_2 + Co\,As_2$) which cannot be distinguished either in crystalline form or in composition are divided, so far as their thermo-electric relations are concerned, into two groups. Calling the two forms of the minerals α and β, Marbach gives the following series, reckoning from negative to positive: Iron pyrites α, cobaltine α, bismuth, german silver, platinum, lead, copper, brass, silver, cadmium, iron, antimony, cobaltine β, iron pyrites β.—*Pogg. Ann.*, cxxiv, 632. W. G.

3. *On the wave length of the blue iridium line.*—J. Müller has determined the wave length of the blue line characteristic of the new metal indium, and finds as a mean of two determinations $\lambda = 0{\cdot}000455^{mm}$.—*Pogg. Ann.*, cxxiv, 637. W. G.

4. *On the absorption spectrum of Didymium, Erbium and Terbium.* —**Delafontaine**, who maintains the existence of erbium and terbium as distinct elements, has compared the absorption spectra of these two metals with that of didymium, the spectrum of which was first studied by Gladstone. The absorption bands in the spectra of erbium and terbium were first observed by Bahr. The didymium spectrum has been also studied by Rood and by Erdmann, both of whom detected several dark bands not noticed by Gladstone. Erbium gives usually five lines or bands, and eight when in the form of a thick syrup. Er^a remains after all the other lines have vanished. Terbium is characterized by three bands of metal, of which only two are very distinct. The third, or Tr^c, is difficult to recognize, and perhaps does not belong to the metal at all. Upon a scale on which $\mathrm{Na}=27$, $\mathrm{Li}^a=10$ and $\mathrm{Tl}=43$, the author found

$\mathrm{Di}^h = 9$
$i = 19$
$a = 28\text{–}32$
$b = 48\text{–}50$
$d = 54\text{–}55$
$e = 66\text{–}67$
$g = 70$
$f = 73\text{–}75$
$c = 85\text{–}91$

$\mathrm{Tr}^a = 30\text{–}32$
$b = 48\text{–}50$
$c = 85\text{–}89$

$\mathrm{Er}^f = 9$
$d = 14$
$g = 16$
$h = 42$
$e = 44$
$a = 48\text{–}50$
$b = 65$
$c = 85\text{–}90$

From this it appears that two of the bands are common to all three elements, (Di^b, Tr^b, Er^a and Di^c, Tr^c, Er^c), and also that Er^f coincides with Di^h.—*Pogg. Ann.*, cxxiv, 635. W. G.

5. *Polisimmetria dei Cristalli; Relazioni tra la geminazione dei Cristalli ed il loro ingrandimento;* par **Arcangelo Scacchi**. 120, 28 pp. 4to, with 4 plates. Napoli, 1864.—By polysymmetry Professor Scacchi understands the quality pertaining to most substances of presenting distinct but closely related crystalline forms differing more or less in physical qualities. Dimorphism differs from polysymmetry in that the forms of a dimorphous substance are widely diverse, and fail of the close analogy which exists between Scacchi's polysymmetric forms. A hexagonally crystallized species, for example, as sulphate of potash, may, when formed, under some circumstances, have one lateral axis elongated, and a small variation of angles in the transverse zone, so as to become in fact trimetric though in planes and habit like a distorted hexagonal crystal; and this variation of form will appear also in the optical characters. Such variations have often been looked upon as mere distortions. But Scacchi shows that they are of frequent occurrence, and occur according to a distinct method and law; and he gives the name *polysymmetry* to the property. It was formerly supposed that the distinction of *system* of crystallization between species was a fundamental one, but this has for some years been admitted to be incorrect. Scacchi's observations illustrate this point most strikingly; since it is only by a small variation, and one which actually occurs in the same compound, that a hexagonal prism passes to a trimetric, or a monometric to a dimetric, or an orthometric to a clinometric. The acid paratartrate of soda is one example Scacchi mentions of a substance crystallizing under two types, one of

them trimetric and the other triclinic. Orthoclase and albite constitute, as he observes, another example. Yet it is one which brings dimorphism and Scacchi's polysymmetry into close relation. Professor Scacchi presents a large number of facts illustrating his views, and discusses also the origin of the phenomena observed. In his second memoir, Professor Scacchi gives an account of some important observations bearing on the origin of twin crystals.

6. *Memoire sur l'Emploi du Microscope polarisant, et sur l'étude des propriétés optiques biréfringentes propres à déterminer le système cristallin dans les Cristaux naturels ou artificiels;* par M. Des Cloizeaux. 60 pp. 8vo, with two plates. 1864. Paris. (Dunod, éditeur; Quai des Augustins, 49).—No one has labored more effectually in optical mineralogy than Mr. Des Cloizeaux. The science bears evidence throughout of progress due to his investigations. For such optical researches a polarizing microscope is very often essential, owing to the smallness of the crystals, or of the fragments of crystals, which may require study. In the memoir above mentioned Mr. Des Cloizeaux describes the microscope which he has found most satisfactory for his purposes, and gives the method of using it for the various kinds of observations. The author promised in the first volume of his mineralogy (published in 1862) that this memoir should appear as an Appendix to the *second;* but the uncertainty with regard to the publication of the latter has led to his issuing it separately.

7. *Zirconium.*—Zirconium has been the subject of researches by Mr. Troost, which were recently presented to the Academy of Sciences at Paris. The results go to show that zirconium acts the same part in the Carbon series of elements that antimony does in the Nitrogen; and that this element constitutes the passage between silicium and aluminium, thus sustaining the view of St. Claire Deville who places in a common group, carbon, boron, silicium, zirconium and aluminium. Crystals of zirconium are monoclinic rhombic prisms of 93°, inclined 103° to the basal plane. —*Les Mondes*, July 20.

II. MINERALOGY AND GEOLOGY.

1. *On the Sand hills of Cape Henry in Virginia;* by B. Henry Latrobe, Esq. (Transactions Am. Ph. Society, vol. iv, p. 439, and Bruce's Mineralogical Journal, No. iv, 1814).[1]—From the falls of the great rivers of Virginia over the out-runnings of the granite strata, the general level of the land gradually approaches the level of the ocean. At the falls it is elevated from 150 to 200 feet above the tide; on the sea-shore at Cape Henry, the original coast rises not more than 15 feet above high-water mark.

That the whole of this extensive country, from the falls to the coast, is factitious, and of Neptunian origin, appears far from being hypothetical; and the fossil teeth and bones, which accompany this memoir, and which with many hundred more, were dug out of a well at Richmond from the depth of 71 feet, prove that the deposition of the super-strata is not of a date sufficiently removed to have destroyed the soft and almost cartilaginous part of the joints, or to have injured the enamel of the teeth. The Neptunian theory of geogeny, has now very generally taken place of the old volcanic system, and, as far as conjecture and hypothesis can forward science, it is certainly more generally applicable. But along the coast of Virginia,[1] a process is going forward, the result of which will be exactly similar, and in which water has no immediate share.

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[1] We reproduce this article, originally published fifty years since, because of its interest as affording data for estimating the changes in progress along portions of the Atlantic coast. We learn from Mr. J. E. Hilgard, of the Coast Survey, that the position of the lighthouse remains the same as when the observations were made.—Eds.

The shore, and the bed of the Atlantic near the shore, consist of a fine sand. The daily action of the flood tide carries a certain quantity of this sand above high water mark, which being dried by the sun and air, is carried further inland by the winds. The most violent winds on this coast blow from the points between the northwest and the east; and besides, a gentle easterly breeze prevails the whole summer, during some part of almost every day. This easterly wind, which is in fact a trade wind, is felt as high as Williamsburg. It is said to be felt, at this day, higher inland than formerly, and to be annually extending its influence; and it will no doubt, when the woods shall be more cleared away, blow health and coolness over a portion of lower Virginia, which is now considered as extremely unhealthy.

These easterly winds blowing during the driest and hottest season of the year, carry forward the greatest quantity of sand, and have amassed hills, which now extend about a mile from the beach. The natural level of the land, elevated little more than 10 feet above high-water mark, has a very gentle declivity to the east. It is now a swamp[2] of about five miles square (25 square miles). The soil below the surface is a white loamy sand, and if the water falling upon, or rising in it, had a free discharge to the ocean, it would probably be perfectly dry; this, however, the sand hills prevent, and the water is discharged into the sea to the southward, and into the mouth of the Chesapeake to the northward, by small creeks, which find vent from the westerly extremities of the swamp. Lynnhaven creek is the most considerable of these drains. The swamp, or as the neighboring inhabitants call it, the Desert, is overgrown with aquatic trees and shrubs; the gum, (*L. styraciflua*) the cypress (*Cup. disticha*) the maple (*Acer rubrum*) the tree improperly called the sycamore (*Platanus ocidentalis*) the Magnolia glauca, the wax myrtle (*Myrica cerifera*) and the reed, (*A. tecta*) are the principal. Of these many thousands are already buried in the sand, which overtops their summits, and threatens the whole forest with ruin. Their destruction is slow, but inevitable. Upon the extreme edge of the sand hills toward the swamp, the wind, opposed by the tops of the trees, forms an eddy; the sand carried along with it is precipitated, and runs down the bank into the swamp. Its slope is very accurately in an angle of 45°. By gradual accumulation, the hill climbs up their trunks, they wither slowly, and before they are entirely buried, they die. Most of them lose all their branches, and nothing but the trunk remains to be covered with sand, but some of the cypress retain life to the last.[4]

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[1] I speak only of the coast of Virginia at Cape Henry; for although I have the best reason to believe that the same natural process has produced all the sand banks, islands, and sand hills from the Delaware to Florida: I have only *examined* that part of the coast, which is the subject of the present memoir.

[2] By a swamp I exclusively mean a piece of ground, the surface of which is wet and soft, but which has a sound bottom. In this it differs from the Dismal Swamp, much of which is a *bog* or *morass*. Into the latter, a pole of any manageable length may be forced with great ease.

The Desert abounds in deer, bears, racoons, and opossums. Its skirts are more thickly peopled than the sterility of the soil would give reason to suppose; but the inexhaustible abundance of fish and oysters in the creeks, and the game, render it easy to support a family.

The lighthouse,[5] which was built about sixteen years ago, is an octangular truncated pyramid of eight sides, rising 90 feet to the light, and sunk 18 feet below the basement course. Within a few yards of the lighthouse is the keeper's dwelling, a wooden building of two stories. Both are surrounded by a platform of plank; and, without any such design in the architect, this platform has preserved both these buildings from being buried in the sand.

When the lighthouse was built, it was placed upon the highest sand hill at the Cape. Its distance from the beach may be six or seven hundred yards, and the elevation of its base above high water not less than 90 feet. At that time there was from the foot of the building, the most expanded view of the ocean, the Desert, the Chesapeake and its eastern shore. At present, a mound of sand surrounds them, which overtops the keeper's dwelling, and has buried his kitchen to the eaves. The platform, which was laid upon the former level of the sand, is an accurate standard from whence to ascertain its accumulation. The winds, meeting in their course the elevated tower of the light, form a perpetual whirl around it, which licks up the sand from the smooth surface of the timber, and heaps it around in the form of a basin. Where the platform ceases, the sand accumulates. The sandy rim, while it protects the keeper from the storms, renders his habitation one of the dreariest abodes imaginable. This rim is sometimes higher, at others lower, according to the direction and strength of the wind. Since the establishment of the light, the hills have risen about 20 feet in height (measuring from the platform) and have proceeded into the Desert about 350 yards, from a spot pointed out to me by the keeper. I stepped the distance as well as I could, while at every step I sunk up to my ancles into the sand. The height of the hill at the swamp, is between 70 and 80 feet perpendicularly. It is higher nearer the sea, the inner edge being rounded off, and I think at its highest point, it cannot be less than 100 feet above high-water mark. If the hills advance at an equal ratio for 20 or 30 years more, they will swallow up the whole swamp, and render the coast a desert indeed, for not a blade of grass finds nutriment upon the sand.

Should this event take place, and some future philosopher attend the digging of a well *in the high sandy country*, *on the coast of Virginia*, his curiosity would be excited by fossil wood, 100 feet below the surface.

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[4] That the swamp with its trees extended to the sea coast, perhaps *within* a century, is very evident from this circumstance: between the summit of the sand hills and the sea shore, and more especially on the Chesapeake side, the undecayed, though mostly *dead* bodies of trees still appear in great numbers. Being on the windward side of the sand hills, they have not been more than half buried. At the lighthouse there are none of the trees, but to the westward and southward are many.

[5] It is a good solid building of Rappahannoc freestone.

He would there discover a bed of vegetable and animal exuviæ, and going home, he might erect upon very plausible ground, a very good-looking hypothesis of a deluge, sweeping the whole upper country of its sand, and depositing it along the line of its conflict with the waves of the ocean.

2. *Volcanic Eruptions in Northern California and Oregon.*—Within the last few years there have been frequent reports of volcanic activity in or near the extinct volcanoes of Northern California, Oregon, and northward from that chain of high peaks. Thus, Mt. St. Helens in 1851, Lassen's Peak in 1856, Mt. Hood in 1864, are most familiar, the activity being shown by vast quantities of steam emitted. Various similar reports occur at times in the newspapers of the Pacific coast. The *Oregonian* of last April contains the following: "Mt. Baker, it is said, is rapidly sinking in. It is asserted that the mountain has fallen 1000 or 1500 feet, and that its summit, which was formerly a sharp point, is now much flattened. This peak has been for some time in a state of active eruption. Dense clouds of smoke have of late issued from it." Correspondents of the California papers speak of the same phenomenon, one of whom asserts that the emission of steam is immense, and that 1,200 feet of the summit has fallen in. We have no other data. W. H. B.

3. *Notice of Pot-holes near Poultney, Vermont;* by JOHN A. NICHOLS.—About two and a half miles south of this place, on the road to Granville, N. Y., the U. S. Slate Co. has just opened a new quarry. Slate quarries abound in the neighborhood, some of which have been worked for years; but in this new quarry seven pot-holes have already been found, and nothing of the kind has ever been found in any other quarry. The largest complete hole is elliptical in shape, its longest diameter being 11 ft. 6 in., its shortest 10 ft.; its depth is not known, as the earth and stones which filled it are not yet dug out. A smaller hole, which has been emptied, is 9 ft. deep and 5 ft. in diameter. Its sides are smoothly polished and it contained boulders, the largest weighing one and a half tons, the smallest not larger than a hen's egg, most of them beautifully rounded and polished. The slate is quarried in the hills bounding a valley through which runs a brook in a general southerly direction, emptying into a little stream called Granville river. The valley varies in width from a quarter to half a mile, and is quite flat; and the bounding hills rise from it rather abruptly to the height of 50 to 100 ft. The Rutland and Washington railroad, running north and south, passes through the valley. The U. S. Slate Co.'s quarry is on the western side of the valley, where the chain of hills suddenly makes a small sweep westward, giving the appearance of a small semi-circular bay, having a diameter of about 250 ft. This brings the quarry on the side of a bluff. The tops of the pot-holes are respectively 30, 25 and 10 ft. above the bed of the brook and about 800 ft. from it. There are no traces visible of the bed of the stream in which the pot-holes were made. The strata have a dip westwardly of about 30°. Poultney river, which forms part of the boundary line between New York and Vermont, turns abruptly near the point where it meets the valley of the brook, and flowing northward empties into Lake Champlain. The quarry is two and a half miles from the nearest point of Poultney river. The side of

the largest pot-hole has, in some places, a coating of carbonate of lime one-tenth of an inch thick, laid very evenly over the slate, and large lumps of limestone of the nature of stalactites have been found in the same hole.

I went over the ground in company with Mr. A. E. Knapp, one of the principal owners of the quarry, who furnished most of the measurements mentioned above.

Poultney, Vt., July, 1865.

4. *Observations on the Eocene Lignite Formation of the United States;* by T. A. Conrad. *Older Eocene or London Clay. Lignite Epoch.*—Some years ago I visited a marl deposit near Long Branch, Monmouth Co., N. J., in which casts of a few shells presented an Eocene character. Observing in Vanuxem's cabinet a specimen of what is now known to be *Aturia ziczac*, I described it in the Journal of the Academy of Natural Sciences, vol. i, 2d series, p. 129, and referred the marl, principally on account of the presence of this shell, to the Eocene era. I also described an imperfect cast of the same species as *Nautilus angustatus*, in Dana's Report of the Geology of the Exploring Expedition, which was found at Astoria in Oregon, in company with many shells which I mistook for Miocene forms; but a more extended acquaintance with Eocene types shows their older tertiary relations, and their matrix to be synchronous with the London Clay, of Sheppey, Highgate and Bracklesham. Professor Cook has lately sent me a box of specimens of similar age from Shark River, Monmouth Co., New Jersey, collected by Dr. Kneiskern. In company with *Aturia ziczac*, there are imperfect specimens of *Nautilus Lamarckii* Deshayes, another older Eocene form of the Paris Basin and of Belgium. Fruits also occur in this bed, referrible to the genera *Nipadites* and *Mimosites*, showing the tropical or semitropical climate of the era, and giving evidence of the intimate relations of the deposit to the Brandon and Mississippi Lignite strata. Indeed it seems clear that this Shark River marl was the bed of the oldest Eocene ocean, and that the flora of the Brandon and Southern Tertiary epoch flourished at the same time. The local, circumscribed character of the Brandon Lignite is attributed by Prof. Lesley, to its having filled a deep depression, thus escaping the denuding forces which swept all traces of it away over a wide region that it once covered. The locality at Mont Alto, near Chambersburg, described by Prof. Lesley, is doubtless a locally preserved fragment of a vast formation once deposited over the Appalachian slope to the very base of the mountain range, and occupying a large space in South Carolina, Georgia, Alabama and Mississippi, and in fact, extending to the Pacific as far north as Vancouver's Island. Dana's map of the Cretaceous epoch gives a general view of the United States at this time, supposing what was then ocean had become land and fresh water.

It is probable that the estuary deposits of Upper Missouri are the base of the older Eocene, and the fresh-water shells are the earliest tertiary types of this continent. The species of *Vivipara* resemble the Eocene forms of the Paris basin. According to Meek and Hayden these beds are more than 2000 feet thick.

Vanuxem was the first geologist who stated that a lignite bed is situated in South Carolina between the Cretaceous and Eocene strata, and Tuomey has since described several localities in that State and one in Clark Co., Alabama, represented by No. 6 of his Bashia Creek section; and No. 2 of the section represents the Marlborough and Buhrstone group, or second stage of the Eocene. In general, some doubt rests upon the identity of species by Tuomey, but the following list of shells contained in No. 2 is copied, with emendations, from his Report: *Ostrea Carolinensis* Con., *Venericardia planicosta*, *Protocardia Virginiana?* Con., *Volutilithes Tuomeyi* Con. This bed represents the dark-colored loose sand of Piscataway, over which, and next in succession, lies the Marlborough rock, which corresponds to the "great Carolinian bed" of Ruffin, and the "calcareous strata of the Charleston basin" of Tuomey. The sand bed and condition of its fossils, as well as the similarity of some of its species, reminds us of the Bracklesham Bay locality in England, and the superimposed rock of the Bognor beds.

Although the *Aturia ziczac* is the only fossil of Oregon known to be identical with the New Jersey Eocene, the vast distance between the localities will account for the variation; for the Continent was then as wide as from the Appalachian to the Rocky mountains, and seems to have been intersected by many rivers and fresh-water lakes, which have left an abundance of shells and mammalian remains entombed in the strata deposited by their waters. The Brandon fruits described by Hitchcock are all different from those of Shark River, but the conditions under which they flourished may account for this variation. They probably grew on high land, at some distance from the coast, whilst the station of the others was on low land along the shore, where Palms and Acacias scattered their fruit within reach of currents which swept them into the sea.

At present the marine beds of this era are found to lie close to the Atlantic, and in Oregon they skirt the shore; but estuary deposits were observed by Meek and Hayden in Upper Missouri. The Shark River marl is an indurated clay, with disseminated grains of green sand, which are often smooth and shining, and the shells are all in the form of casts, which are more or less distorted. Portions of this clay are indurated, making it as difficult to break as the hardest limestone. Its thickness is yet unknown. The *Aturia* of this locality is discoid, which is the result of pressure, whilst the Oregon forms are broader, and one specimen approximates to the normal form of the European shell.

Professor Harper describes a deposit on Chickasawhay river, Mississippi, which also is of similar geological age. "The *Nipadites* and *Cycadites* mixed with coniferous trees, and even oaks." "Stumps are seen rooted in the ground, as smooth and even as if not cut with an axe, but sawed with a sharp saw." "A little higher up, on the Chickasawhay river, occurs the most southern outcrop of the large Eocene marl stratum. Above the marl lies a stratum of hard limestone, which contains abundance of an *Ostrea* of large size." In this description I recognize the strata on Savannah river, where the lignite is overlaid by the "great Carolinian limestone" group, and succeeded by the *Ostrea Georgiana*, which is found as far west as Cape St. Lucas in Lower California.

The lignite bed underlies the bluff at Vicksburg, where we find—1. lignite; 2. ferruginous rock, with *Ostrea Georgiana* Conrad; 3. St. Stephen's limestone, or Orbitolite limestone, eighty feet; 4. Vicksburg group, with a new species of Orbitolite,—*N. supera* Conrad.

This formation appears at Cape Sable, near Annapolis, where, at about the water level, "under a stratum of sand, and resting upon an impermeable crust of ferruginous sandstone, lies imbedded in a layer of almost pure alumine, a forest of pine trees, thrown down by some ancient convulsion. The crust which forms the base of this aluminous layer is a little below the level of low tides and is of considerable hardness. The imbedded pines are converted into lignites more or less impregnated with sulphuret of iron. The central parts are generally transformed into pure metallic sulphuret, sometimes exhibiting in the hollow parts octahedral crystals of a yellowish metallic lustre and great hardness. The more remote the ligneous layers from the center, the less they are saturated with sulphuret of iron. The external rays, as well as the cortical layers, are generally pure lignite, some compact and black, others retaining the color and friability of rotten wood. In some instances their texture seems to have suffered but little alteration: the central system, concentric rays, the bark and knots being perfectly discernible; even *fruits* are occasionally found in a pretty good state of preservation as to form."[1]

The lignite is correctly placed in Morton's diagram as overlying the secondary marls. In Morton's paper the first published notice of the formation appeared, drawn up from the notes of Lardner Vanuxem, who was familiar with the strata in South Carolina. Deshayes states that he has found no species of organic remains common to Cretaceous and Eocene strata in Europe or Asia, and I have no doubt that the destruction of life was total over the whole surface of the globe at the close of the Cretaceous era. Deshayes, indeed, affirms that life has been five times destroyed and renewed in the past history of the earth. When we find evidence of surprising changes of level in the Eocene period, the limited nature of a mixed fauna is remarkable, for we would expect to find it much more extensive at the base of the Eocene. The bed of the Atlantic along the coast of the United States, from Cape May to the Gulf of Mexico, contains a mixture of recent and Miocene shells, which, if elevated above the sea level would present a group of shells consisting of recent and extinct species, so like in preservation that the fossil could not be distinguished from the recent forms, except by one conversant with all the Miocene shells.

Deshayes affirms of the Maestricht beds, "that there has been an accidental mixture of Cretaceous and Eocene; a degradation of a stratum of fossiliferous marl diluted in the bed of the tertiary sea at the time of the first deposit. The bed of the ocean, under our own eyes, shows an accidental mixture of this nature."

The Wilmington rock proves conclusively that this was the case in North Carolina. Eocene and Cretaceous fossils are there mingled in a breccia. When I first saw this rock in 1832, no fracture or excavation revealed its true character; but the external resemblance to the Timber Creek limestone of New Jersey, with its corallines, was striking. The mixture of Secondary and Tertiary species in this breccia, shows that a disturbance occurred in the bed of the Eocene ocean, which evidently from Tuomey's account, extended into South Carolina. No one, I suppose, will tell us that the *Venericardia planicosta* existed in the Cretaceous period, yet countless thousands may be observed at the base of the Eocene. It is true that in Europe a series of strata, termed Upper and Lower "Landenien" and "Heersien," are said to intervene between the chalk and Eocene; but one of the characteristic fossils of the Upper Landenien occurs in the Shark river beds,—the *Cyprina Morrisii* of Sowerby. It is therefore probable that the former system is merely an extension of the London clay. Certainly, in the United States, there is no such system as the "Heersien," whilst Lyell found, in the Belgium Lower "Landenien" grey marl, a perfect specimen of the *Terebratulina gracilis*, a well known chalk fossil,—together with *Ostrea (Exogyra) lateralis* Nyst. Lyell remarks, that the Lower "Landenien," at Folx les Caves, rests on the Maestricht chalk.

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[1] Durand, Journ. Philad. College of Pharmacy, v, 12, 1834.

There is an extensive bed of lignite in Europe of Eocene age, which Deshayes says forms a well determined horizon with the long series of "sables inférieures." "Above the lignite appears a bed of fresh-water and marine shells, the horizon of which I believe to be the same as that of the lignite formation of the United States. They reveal a singular state of the globe at the commencement of the Tertiary period, presenting a vast level region covered by a dense forest, in which palms and oaks grew side by side, interspersed with lakes and rivers, and long shallow bays of salt water penetrating to the interior of the continents. This state of the globe was exhibited in Europe and America at the same time, and the land was little elevated above the sea level, except that in America the Appalachian and Rocky mountain ranges stood out from the vast plain.

The Shark River fossils are few in number of species, and generally imperfect casts with small chalky portions of the shell occasionally remaining. A few of the bivalves have connected valves. About twenty-five species of shells and plants have been collected, of which I think six shells are identical with species of the London Clay and one of the Plastic Clay, *Cyprina Morrisii*.—*Proc. Acad. Nat. Sci.*, 1865, 70.

5. *On the Fossil Insects from Illinois, the Miamia and Hemeristia*, described in vol. xxxvii, of this Journal, at page 34; by SAMUEL H. SCUDDER. (From a letter to Professor DANA.)—In my study of the specimens of fossil insects from Illinois, allowed me through your kindness, I have observed new facts of interest, and arrived at some conclusions different from yours, which I take this opportunity to communicate to you.

The part in the *Miamia* which I believe represents the head is an obscurely defined portion in front which you regarded as the anterior femur and which I at first supposed it, until my attention was directed to the diminutive size of the prothorax (only half the width of the rest of the thorax); for a raptatorial, or any disproportionally large, fore-femur would require a prothorax of at least the size of the mesothorax or metathorax, bearing, as they do in this case, wings of such a feeble type. The right posterior border of the head is not represented in the drawing given, and is quite indistinct, but forms a regular outline of exact symmetry with the opposite. The middle and hind femora and tibiæ are quite distinct, but unfortunately we cannot see the basal terminations of the femora, and do not know how far they may be removed from their natural connection with the body; and since the tarsi are wanting we are unable to tell whether the tibiæ are docked, or whether we actually view the extremity, although the latter is probably the case.

The head appears to have an ovoidal outline, and on either side there are depressions—that on the left very distinct—which are apparently the positions of the elongate prominent eyes; and there are also faint indications of antennæ taking their rise at the usual point.

In the *Hemeristia*, with the exception of a fragment of a femur quite like that of *Miamia*, of which no representation is attempted to be given in your cut, we have only a portion of the four wings preserved. That we have more than a single wing is evident from the symmetry of the two sides of the fragments, and the regular intersection of the nervures from either side. In point of fact, we have in this case four completely overlapping wings in an attitude of repose, so perfectly transparent, and provided with such strong nervures and cross-veins, that those of each wing are visible in some portion of the fragment, forming thus a very confusing and almost inextricable mass of veins crossing and connected with one another in a most perplexing manner; moreover, the principal portions of the apex of the wings are broken off, the basal portions undistinguishable, and no portion of any one of the wings present in place except a very short piece of the anterior border of one, which fortunately is at the most important curve; neither have we, apart from the direction and position of the nervures themselves, any means of determining whether the upper or under surface of the insect is presented to us, the abdomen being absent, or at least invisible. It is nevertheless possible to decide that the upper surface of the insect is uppermost; that the right anterior wing overlaps the left anterior wing; and to reconstruct the entire anterior wing with but little doubt attaching to its exact form and the direction of the nervures, and to point out the principal and most characteristic differences between the neuration of the anterior and posterior wing.

The details I have presented in full in a paper read before the Boston Society of Natural History which will soon be published in their Memoirs.

From the facts which I have observed, I am enabled to determine with considerable precision the relation of these insects to the Neuroptera.

In the paper referred to I have discussed this question fully, and I will here give but a brief statement of my results. I am satisfied that the direction, mode of branching, and the amount and position of the space occupied by the principal nervures of the wings form most satisfactory bases for the distinction of families in this group of Neuroptera, characters upon which no systematist appears hitherto to have relied, and to which none have invited any attention, except it be Burmeister, whose treatment of it appears to have been partial, not altogether accurate, and exhibits a tendency to exaggerate the value of unimportant characters to the exclusion of those of more weighty significance. Heer, however, in his work on the fossil insects of Œningen &c., has distinguished the Termitina by characters of this sort in a proper manner, and it is to be regretted that he should not have extended his studies to other families.

From the wing-characteristics we learn that the Miamia and the Hemeristia belong to distinct families, and that neither of them can be referred to any family of known living tpyes, although resembling in certain features sometimes one, sometimes another, of these families; and it is most interesting, in this connection, to find that the families, from which these peculiarities are borrowed, belong, some to the Neuroptera proper, and some to the so-called Pseudoneuroptera (classed by some authors with the Orthoptera), so that the fossil forms unquestionably represent a synthetic Neuropterous type, uniting in one and the same group features of wing-structure found existing by themselves only in Neuropterous groups widely separated. We are strongly fortified in this view by the other portions of the insect in the Miamia. In the abdomen, mesothorax and metathorax, we are strongly reminded of Corydalis, one of the Sialina, a Neuropterous family; while in the head and prothorax, together with the flattened legs, we have features which are quite characteristeric of the Perlina, a member of the Pseudoneuroptera; their combination, however, we find only in the family to which the Miamia belongs. So far as the small fragments of femora and tibiæ in the Hemeristia tell us anything, they indicate a close affinity with the Miamia, so close indeed, that it appears highly probable that in characters apart from those drawn from the structure of the wing we shall find, as in Miamia, a relationship with the Neuropterous and Pseudoneuropterous types.

For these two synthetic families, I would propose the names of PALÆOPTERINA and HEMERISTINA.

In the *Palæopterina* we have a body rather broad and depressed, the head horizontal, the legs compressed, not long, the wings large, very regularly rounded, the two pairs equal, extending beyond the abdomen, and when at rest overlapping one another horizontally above it in a loose manner. In the wings the first nervure is continuous as in all other families except Odonata; the second impinges upon the third in the outer half of the wing; the third branches in the middle third of the wing at a very slight angle, the lower branch rebranching; the fourth, running in close contiguity to the previous one, branches near the division of the third; these four nervures occupy the anterior half of the wing; the fifth forks near but below the branching of the previous ones, its upper fork continually reforking, the branches so formed sometimes connected by distant cross-veins with one another, or with the lower branch of the fourth nervure; the sixth occupies about as much space as the fifth and is made up of a number of branches which fork near the base and fill the space with approximate nervules running parallel to the lower branch of the fifth. The wings are feeble and the nervures delicate, as in Ephemerina.

In the *Hemeristina* the first, second and third nervures run nearly parallel to one another throughout their course; the third sends out a branch at the extremity of the basal third of the wing at a considerable angle, which soon curves upward and runs nearly parallel to the main stem, sending out from its lower side several branches; these three nearly occupy the anterior half of the wing; the fourth, after running in contiguity with the third, diverges widely from it and forks below the branching of the third; the fifth, running parallel to the previous, forks very narrowly near the base, the upper fork again forking in a similar manner, and the lower emitting branches from its under side; the sixth is probably nearly as in the Palæopterina, though but little remains of it. The wings overlap one another very completely, are twice as broad in the middle as at the base, strong, the nervures prominent, and connected throughout by frequent and strong, generally straight, cross-veins. The legs are broad and compressed.

The characters here given are sufficient to distinguish these families from any others now known among the Neuroptera.

In concluding I take pleasure in apprising you that in the still older forms of insects that have been discovered by Mr. C. F. Hartt in the Devonian strata of New Brunswick are represented other hitherto unknown families of Neuroptera, some of which exhibit, in even more striking a manner than your specimens, combinations of structure borrowed from families of the two great sections. One is a gigantic member of the family Ephemerina; while the most interesting of all is a wing which appears to blend the peculiar structure of the stridulating apparatus of the male in some Orthoptera, with the general mode of neuration of the wings holding in the Neuroptera, carrying the synthesis one step farther back.

Boston Society of Natural History, Jan. 30, 1865.

6. *Paleontology of the Upper Missouri.* A Report upon collections made principally by the Expeditions under the command of Lieut. G. K. Warren, U. S. Top. Engrs. in 1855 and 1856. *Invertebrates*, by F. B. MEEK and F. V. HAYDEN, M.D.; Part I, 136 pp. 4to, with five plates; 1865. (Smithsonian Contributions to Knowledge; accepted for publication May, 1864.)—To Messrs. Meek and Hayden we are indebted for a large part of all our accurate knowledge of the geology and paleontology of the eastern slope of the Rocky Mountains. Both have explored portions of the region, but Hayden much the most extensively; while the determination of the fossils collected and the working up of the paleontology is mainly the labor of Mr. Meek. The volume on the paleontology, just issued under the auspices of the Smithsonian Institution, takes up the fossils of, (1) the Potsdam or Primordial period, (2) the Carboniferous, (3) the Permian, and (4) the Jurassic. A second part of larger size, will contain the Cretaceous fossils, and a third the Tertiary. The work is not a mere description of fossils with figures; but besides full descriptions, it contains a thorough discussion of the zoological characters and affinities of the species, their synonymy, and their stratigraphical and geographical relations. The synonymy has become greatly confused, and particularly so for this western region, through the hasty work of explorers; and it is of great importance to the science that the subject should have been so thoroughly followed out through all its intricacies. The plates are handsomely lithographed from the careful drawings of Mr. Meek. The country owes much to the Smithsonian Institution for the publication of works illustrating American history; and those relating to its ancient progress through the geological ages are not the least valuable among them.

7. *Archæopteryx.*—On one of the two slabs from Solenhofen containing remains of the Archæopteryx, there is "a crescent shaped protuberance" which is pronounced by Mr. Evans, in an article in the Natural

History Review for June, to have been due to the remains of the *Cranium* of the Archæopteryx, and even the form of the brain cavity and position of the brain—both ornithic in character, are supposed to be made out. There is also on the principal slab a small V-shaped object, the longer limb of which is 1½ inches long, made partly of mineralized bone and partly of the same bones that occur on the counterpart slab. Along side of it there are impressions of four teeth. These teeth may be the teeth of the Archæopteryx, *if not* of some other animal whose remains lie buried in the same rock. H. von Mayer has examined the teeth and observes that he has not before seen teeth of the same kind in the lithographic stone; and that they are most like the teeth of his family of *Acrosaurus.*

8. *Documents sur les tremblements de terre et les phenomènes volcaniques dans l'Archipel des Kouriles et au Kamtschatka;* par M. Alexis Perrey. 166 pp., large 8vo. (From the publications of La Soc. Imp. d'Agriculture, d'Hist. Nat. et des Arts utiles de Lyon, July 17, 1863).—Prof. Perrey gives in this memoir a very full review of all that is known with respect to the volcanos and earthquakes of the Kurile Islands and Kamtschatka. He quotes at length from a memoir by K. von Dittmar, in Petermann's Geogr. Mittheilungen, 1860, on the latter region, giving descriptions of 12 active and 22 extinct volcanos in eastern Kamtschatka and 5 extinct in western, besides many hot springs. Dittmar states, in conclusion, that volcanic activity commences to the north near the parallel of 62°, but is apparent there only in hot springs. The first volcanos occur between the parallels of 58° and 57°, and these are extinct. 57° is the beginning of the long line of volcanos (the "Aléoutes") which continues to the southern extremity of the peninsula, and extends on, in the Kuriles, even to Japan. The active vents are most thickly grouped between 54° and 55°, and the hot springs about the 53d parallel. The solfataras and the deposits of sulphur occur along the whole volcanic line from 62° to southern Kamtschatka, being situated in the old craters or near hot springs. On the western side of the peninsula there are only a few volcanos, and these are extinct. Prof. Perrey cites from other memoirs numerous details of interest connected with the eruptions of the volcanos, and the earthquakes of the peninsula.

9. *Extraits de Geologie, pour les années,* 1862–1863; par M. Delesse, Ingenieur en chef des Mines. 150 pp. 8vo., Paris. (From the Annales des Mines, vol. vi, 1864).—In this review of progress in geological science, which has been continued now for some years by Mr. Delesse, the author does not attempt to include paleontology and stratigraphical geology; but, being prepared for the Annales des Mines, the Government Mining Journal of France, it enters fully into the subjects of *lithology*—that is, the nature and composition of rocks and rock materials, mineral waters, alluvium, meteorites, etc., dwelling at length on metalliferous rocks and the various mining regions recently developed and their metallic veins; and also *dynamical* geology, including whatever relates to the alteration and decomposition of rocks or their ores or other mineral constituents, to metamorphism, and to the origin of rocks and the earth's general features. This Annual Report will be found of great interest to both the theoretical and mining geologist.

10. *Earthquake in the Mississippi Valley.*—On the 17th of last August, at 9 o'clock A. M., an earthquake was felt in the Mississippi valley, at Holly Springs, Oxford, and Grenada in Mississippi, Memphis and Lagrange in Tennessee, St. Louis in Missouri, and Cairo in Illinois. How far beyond these limits it extended we have not yet learned. At Memphis the shock lasted about ten seconds, and was severe enough to tumble down chimnies, upset loose articles, and cause the people to run out of their houses. It is reported to have been most severe at Holly Springs, Mississippi, and Lagrange, Tennessee.

11. *Geological Survey of Nevada.*—Mr. R. H. STRETCH, of Virginia City, we are informed, has been appointed State Geologist of Nevada, in conformity with the provisions of the Act passed last winter by the Legislature of this new State.

III. BOTANY AND ZOOLOGY.

1. *The Tennessee Yellow-Wood* (*Cladrastis lutea*).—One of the very handsomest of our ornamental trees has this summer flowered finely in the south of England, as we learn from the Gardener's Chronicle,—the tree "completely covered with long drooping branches of pure white flowers, many of them nearly eighteen inches in length and from ten to twelve inches wide at the shoulders. The foliage, being of a lively green, contrasted favorably with the pure white blossoms. It is also nicely scented." An unusually warm summer has for once brought this tree well into blossom in England. Even here it blossoms copiously only every other year; this year it did so to perfection, owing to the heat and dryness of last season and the very favorable spring of the present year. The fine large tree in the Cambridge Botanic Garden was, as it were, veiled with white, and many of the graceful pendent clusters were fully two feet in length. A. G.

2. *Welwitschia mirabilis*, HOOK., fil.—Some account of this very strange vegetable, and of the interesting memoir, in the *Linnæan Transactions*, in which Dr. Hooker made it known to the botanical world, was given in our vol. xxxvi, for Nov. 1863. Through the great kindness of Dr. Hooker, the Herbarium of Harvard University has recently received a fine old trunk of this *tree*,—if we may so call a woody plant which, though perhaps a century old, is only a foot or two in height, and as broad as it is high, and never had any other foliage than the primary pair of leaves, the cotyledons,—accompanied by the cones (for it is a sort of coniferous tree), both dry and in alcohol. A. G.

3. *On the Movements and Habits of Climbing Plants*; by CHARLES DARWIN.—This is a long paper read before the Linnæan Society in February last, and published in its *Journal*, where it fills 120 pages of the double number, 33 and 34, issued in June. The investigations which it records were made, we believe, during a period when the author's ordinary scientific labors were interrupted by illness,—as was no less the case with respect to his former papers on Dimorphous and Trimorphous Flowers and his volume on the Fertilization of Orchids by the aid of Insects. Of these works and of the present,—side-issues as they are,—it may fairly be said, that they show a genius for biological investigation, and

a power of turning common materials and ordinary observations to high scientific account, which, if equalled, have not been surpassed since the days of Hunter and Charles Bell. This will be the opinion equally, we suppose, of those who favor and of those who dislike Mr. Darwin's theory of the gradual transformation of specific forms through natural selection, upon which, indeed, all these collateral researches have a bearing, direct or incidental. In the present case the bearing is obvious. The gradual acquisition by certain plants of advantageous peculiarities is inferred from the gradation of forms and functions. Properties and powers which are latent or feebly developed in most plants are taken advantage of by some, made specially useful, and enhanced from generation to generation. Tendril-bearing plants,—the most specialized in structure and the most exquisitely adapted to the end in view,—are supposed to have been derived from leaf-climbers, and these in turn from simple twiners.

The author states that he was led to this subject by a brief note, communicated to the American Academy in the summer of 1858, (and reprinted in this Journal,) in which the writer of the present notice recorded his observation of the coiling of certain tendrils by a visible movement promptly following an extraneous irritation. Mr. Darwin's observations were more than half completed before he became aware that the spontaneous revolution of the stems and of some tendrils of climbing plants had been observed and recorded almost 40 years ago, and nearly at the same time, by Palm and by von Mohl, and had been the subject of two memoirs by Dutrochet, published more than twenty years ago. But the mode in which the free and growing end of a stem sweeps around seems not to have been previously well made out, having been more or less confounded with the torsion of the axis which many twining stems, such as the Hop and the Morning Glory, are apt to undergo. It is plain to see, however, that many stems which revolve do not twist at all; and those that do never could twist on their axis at every revolution without speedy destruction,—indeed usually do not twist until they have ceased revolving. Every one must have noticed that the growing extremity of a Hop, Convolvulus, or other twiner, when unsupported, hangs over or stretches out horizontally to one side. But it is not so well known that this outstretched portion, while at the proper age, is continually sweeping round, in circles widening as it grows, and always in the same direction, in search of some object round which to twine. The Hop revolves with the sun; the Convolvulus, Bean (*Phaseolus*) &c., against the sun, that is, in the same directions that they twine. Two or three internodes are usually revolving at the same time. Mr. Darwin observed thirty-seven revolutions in one internode of a Hop,—the first revolution made in about 24 hours, the second in 9 hours, the third and the following ones up to the eighth in a little over 3 hours each. "The shoot had now grown 3½ inches in length, and carried at its extremity a young internode an inch in length, which showed slight changes in its curvature. The next or ninth revolution was effected in 2 hours and 30 minutes. From this time forward the revolutions were easily observed. The thirty-sixth revolution was performed at the usual rate; so was the last or thirty-seventh, but it was not quite completed; for the internode abruptly became upright, and, after moving to the center became motionless. I tied a weight to its upper end, so as slightly to bow it, and thus to detect any movement; but there was none. Some time before the last revolution the lower part of the internode had ceased to move. . . . . It moved during five days; but the more rapid movement after the third revolution lasted during three days and twenty hours. The regular revolutions from the 9th to the 36th inclusive, were performed at the average rate of 2 h. 31 m. The weather was cold, and this affected the temperature of the room, especially during the night, and consequently retarded a little the rate of movement. . . . . After the seventeenth revolution the internode had grown from 1¾ to 6 inches in length, and carried an internode 1⅞ inch long, which was just perceptibly moving; and this carried a very minute ultimate internode. After the 21st revolution the penultimate internode was 2½ inches long, and probably revolved in a period of about three hours. At the 27th revolution our lower internode was 8⅜, the penultimate 3½, and the ultimate 2½ inches in length; and the inclination of the whole shoot was such that a circle 19 inches in diameter was swept by it. When the movement ceased the lower internode was 9 and the penultimate 6 inches in length; so that, from the 27th to the 37th revolutions inclusive, three internodes were at the same time revolving."—(pp. 3, 4.)

The shoots of many climbers sweep their circles more rapidly than the Hop,—the common Pole Bean (*Phaseolus vulgaris*) in rather less than two hours, Convolvuluses of various species in the same time or rather less; while more woody stems naturally move more slowly, some requiring from 24 to 50 hours for each revolution. But the thickness or texture of the shoot does not govern the rate, many slender shoots moving slower than some stout ones, and some lignescent quicker than other purely herbaceous ones. The movement appears to be accelerated, up to a certain point, by raising the temperature, or rather is retarded by lowering it; but while the conditions are nearly the same, the rate is often remarkably uniform. The quickest rate of revolution of a proper stem observed by Mr. Darwin was that of a *Scyphranthus*, in 77 minutes. When the light comes from one side, the semicircle towards the light is usually described in less time, often in less than half the time, of that from the light. The tendency of young stems to turn toward the light is here active as usual, but is overcome by a superior force. The end of the shoot describes circles or broad ellipses, or else, from insufficient power or mechanical disadvantages, narrow ellipses, semicircles, or irregular figures. A horizontal shoot of considerable length will thus be found, not unfrequently, to sway from side to side in a semicircular course, while the extreme internodes are making complete revolutions.

A striking illustration of the amount of space that may be swept over is afforded by a case in which Mr. Darwin allowed the top of a *Ceropegia* to grow out almost horizontally to the length of 31 inches,—three long internodes terminated by two short ones. The whole revolved at rates between 5¼ and 6¾ hours for each revolution, the tip sweeping a circle of above five feet in diameter and 16 feet in circumference, travelling therefore at the rate of at least 32 inches per hour. "It was an interesting spectacle to watch the long shoot sweeping, night and day, this grand circle, in search of some object around which to twine."

As to the nature of this revolving movement, Mr. Darwin clearly shows that it is not a torsion of the axis, but a successive bending [similar to that by which ordinary stems bend toward the light], the direction of which is constantly and uniformly changing. "If a colored streak be painted (this was done with a large number of twining plants) along, we will say, the convex line of surface, this colored streak will, after a time depending on the rate of revolution, be found to lie along one side of the bow, then along the concave side, then on the opposite side, and, lastly, again on the opposite convex surface. This clearly proves that the internodes, during the revolving movement, become bowed in every direction. The movement, is, in fact, a continuous self-bowing of the whole shoot, successively directed to all points of the compass." It is an automatic movement, of the same character as those which these and other parts of plants effect in changing position or direction, sometimes slowly and sometimes with a visible motion. The movement may be likened in one case to that of the hour-hand or the minute-hand of a clock, in the other to the second-hand, but in both is as truly a vital movement as is the contraction of an involuntary muscle. It must be effected—as Mr. Darwin recognizes—either by the contraction of the cells on the concave side, or by the turgescence and elongation of those on the convex side of the internode, or by both,—probably the former, as various facts go to show; but questions of that kind are not investigated in the present essay.

No differences in this regard are observable in the behavior of exogenous or endogenous stems, or even of those of climbing Ferns. *Lygodium scandens*, according to Darwin, revolves like other twiners; it completes its revolutions in six hours, or on a very hot day in five (moving against the sun, which is much the commoner case); this is about the average rate of Phænogamous twiners, like which it comports itself in all respects. Our own *L. palmatum*, we find, revolves in the same way, in about four hours, the temperature being 75° Fahr.

The power of revolving depends, of course, upon the general health and vigor of the plant, and upon the age of the shoot, is retarded by lowering the temperature, is interrupted by any considerable disturbance, such as exposure to cold or to much jarring; carrying the plant from one place to another, or cutting off a shoot and placing it in water, stops the movement for a time, just as it does the more vivid automatic movement of *Desmodium gyrans*. But each internode is so independent that cutting off an upper one does not affect the revolutions of the one beneath. Twining stems are far from being insensible to the action of light (as Mohl supposed), the half-revolution towards the light being not uncommonly twice faster than that from it; but as the rate of revolution by day and by night is nearly the same, the one half of the circuit is accelerated just as much as the other is retarded. This influence of the light is quite remarkable when we consider the slenderness of most revolving internodes, the small surfaces they expose, and that their leaves are little developed.

The design, as we must term it, of this revolving of the end of twining stems is obvious, and usually effectual. Such stems, even when no supporting object is within their reach, will reach each other, and by twining together make a mutual support, from which, as they lengthen, they may reach yet farther. The connection of the revolving with twining is obvious, though the latter is not a necessary consequence; for many stems revolve which do not twine, but climb in some other way.

"When at last the [revolving] shoot meets with a support, the motion at the point of contact is necessarily arrested, but the free projecting part goes on revolving. Almost immediately another and upper point of the shoot is brought into contact with the support and is arrested; and so onward to the extremity of the shoot; and thus it winds round its support. When the shoot follows the sun in its revolving course, it winds itself round the support from right to left, the support being supposed to stand in front of the beholder; when the shoot revolves in an opposite direction, the line of winding is reversed. As each internode loses from age its power of revolving, it loses its power of spirally twining round a support. If a man swings a rope round his head, and the end hits a stick, it will coil round the stick according to the direction of the swinging rope; so it is with twining plants, the continued contraction or turgescence of the cells along the free part of the shoot replacing the momentum of each atom of the free end of the rope.

"All the authors, except von Mohl, who have discussed the spiral twining of plants maintain that such plants have a natural tendency to grow spirally. Mohl believes (S. 112) that twining stems have a dull kind of irritability, so that they bend toward any object which they touch. Even before reading Mohl's interesting treatise, this view seemed to me so probable that I tested it in every way that I could, but always with negative results. I rubbed many shoots much harder than is necessary to excite movement in any tendril or in any foot-stalk of a leaf-climber, but without result. I then tied a very light forked twig to a shoot of a Hop, a *Ceropegia*, *Sphærostema*, and *Adhatoda*, so that the fork pressed on one side alone of the shoot and revolved with it; I purposely selected some very slow revolvers, as it seemed most likely that these would profit from possessing irritability; but in no case was any effect produced. Moreover, when a shoot winds round a support, the movement is always slower, as we shall immediately see, than whilst it revolves freely and touches nothing. Hence I conclude that twining stems are not irritable; and indeed it is not probable that it should be so, as nature always economizes her means, and irritability would be superfluous. Nevertheless I do not wish to assert that they are never irritable; for the growing axis of the leaf-climbing, but not spirally twining, *Lophospermum scandens* is, as we shall hereafter see, certainly irritable; but this case gives me confidence that ordinary twiners do not possess this quality, for directly after putting a stick to the *Lophospermum*, I saw that it behaved differently from any true twiner or any other leaf-climber.

"The belief that twiners have a natural tendency to grow spirally probably arose from their assuming this form when wound round a support, and from the extremity, even whilst remaining free, sometimes assuming this same form. The free internodes of vigorously growing plants, when they cease to revolve, become straight, and show no tendency to be spiral; but when any shoot has nearly ceased to grow, or when the plant is unhealthy, the extremity does occasionally become spiral. I have seen this in a remarkable degree with the ends of the shoots of the *Stauntonia* and of the allied *Akebia*, which became closely wound up spirally, just like a tendril, especially after the small, ill-formed leaves had perished. The explanation of this fact is, I believe, that the lower parts of such terminal internodes very gradually and successively lose their power of movement, whilst the portions just above move onward, and in their turn become motionless; and this ends in forming an irregular spire.

"When a revolving shoot strikes a stick, it winds round it rather more slowly than it revolves. For instance, a shoot of the *Ceropegia* took 9 h. 30 m. to make one complete spire round a stick, whilst it revolved in 6 h.; *Aristolochia gigas* revolved in about 5 h., but took 9 h. 15 m. to complete its spire. This, I presume, is due to the continued disturbance of the moving force by its arrestment at each successive point; we shall hereafter see that even shaking a plant retards the revolving movement. The terminal internodes of a long, much-inclined, revolving shoot of the *Ceropegia*, after they had wound round a stick, always slipped up it, so as to render the spire more open than it was at first; and this was evidently due to the force which caused the revolutions being now almost freed from the constraint of gravity, and allowed to act freely. With the *Wisteria*, on the other hand, a long, horizontal shoot wound itself at first, in a very close spire, which remained unchanged; but subsequently, as the shoot grew, it made a much more open spire. With all the many plants which were allowed freely to ascend a support, the terminal internodes made at first a close spire; and this, during windy weather, well served to keep the shoots in contact with their support; but as the penultimate internodes grew in length, they pushed themselves up for a considerable space (ascertained by colored marks on the shoot and on the support) round the stick, and the spire became more open.

"If a stick which has arrested a revolving shoot, but has not as yet been wound round, be suddenly taken away, the shoot generally springs forward, showing that it has continued to press against the stick. If the stick, shortly after having been wound round, be withdrawn, the shoot retains for a time its spiral form, then straightens itself, and again commences to revolve. The long, much-inclined shoot of the *Ceropegia* previously alluded to offered some curious peculiarities. The lower and older internodes, which continued to revolve, had become so stiff that they were incapable, on repeated trials, of twining round a thin stick, showing that the power of movement was retained after flexibility had been lost. I then moved the stick to a greater distance, so that it was struck by a point 2½ inches from the extremity of the penultimate internode; and it was then neatly wound round by this part and by the ultimate internode. After leaving the spirally wound shoot for eleven hours, I quietly withdrew the stick, and in the course of the day the curled part straightened itself and re-commenced revolving; but the lower and not curled portion of the penultimate internode did not move, a sort of hinge separating the moving and the motionless part of the same internode. After a few days, however, I found that the lower part of this internode had likewise recovered its revolving power. These several facts show, that, in the arrested portion of a revolving shoot, the power of movement is not immediately lost, and that when temporarily lost it can be recovered. When a shoot has remained for a considerable time wound round its support, it permanently retains its spiral form even when the support is removed.

"When a stick was placed so as to arrest the lower and rigid internodes of the *Ceropegia* at the distance at first of 15 and then of 21 inches from the center of revolution, the shoot slowly and gradually slid up the stick, so as to become more and more highly inclined; and then, after an interval sufficient to have allowed of a semirevolution, it suddenly bounded from the stick and fell over to the opposite side, to its ordinary slight inclination. It now recommenced revolving in its usual course, so that after a semi-revolution it again came into contact with the stick, again slid up it, and again bounded from it. This movement of the shoot had a very odd appearance, as if it were disgusted with its failure but resolved to try again. We shall, I think, understand this movement by considering the former illustration of the sapling, in which the contracting surface was supposed to creep from the southern, by the eastern, to the northern, and thence back again by the western side to the southern face, successively bowing the sapling in all directions. Now with the *Ceropegia*, the stick being placed a very little to the east of due south of the plant, the eastern contraction could produce no effect beyond pressing the rigid internode against the stick; but as soon as the contraction on the northern face began, it would slowly drag the shoot up the stick; and then, as soon as the western contraction had well begun, the shoot would be drawn from the stick, and its weight, coinciding with the northwestern contraction, would cause it suddenly to fall to the opposite side with its proper slightly inclined positions; and the ordinary revolving movement would go on. I have described this case because it first made me understand the order in which the contracting or turgescent cells of revolving shoots must act.

"The view just given further explains, as I believe, a fact observed by von Mohl (S. 135), namely, that a revolving shoot, though it will twine round an object as thin as a thread, cannot do so round a thick support. I placed some long revolving shoots of a *Wisteria* close to a post between 5 and 6 inches in diameter, but they could not, though aided by me in many ways, wind round it. This apparently is owing to the flexure of the shoot, when winding round an object so gently curved as this post, not being sufficient to hold the shoot to its place when the contracting force creeps round to the opposite surface of the shoot; so that it is at each revolution withdrawn from its support."—(pp. 9–13, *passim.*)

The successive shifting of the contracting side of the shoot, which explains the revolution or bowing in turn in every direction, no less explains the twining round a proper support, leaving however some idiosyncrases unexplained. Some tendrils and some petioles of leaf-climbing plants equally possess this revolving power; but their usefulness depends mainly upon additional and more special endowments,—mainly upon the power of directly responding by curvature to the contact, more or less prolonged, of an extraneous body.

Of *Leaf-climbers*, no instance is more familiar than that of *Clematis* or Virgin's Bower. Little more was known of them than that they climbed by curling their petioles (common or partial) around neighboring objects. Mr. Darwin made observations upon eight species of *Clematis*, seven of *Tropæolum*, the common species of *Maurandia*, *Lophospermum*, *Fumaria*, &c., as also upon *Gloriosa* and *Flagellaria*, which climb by a tendril-like production of the tip of the leaf. From the summary it appears that plants which belong to eight families are known to have clasping petioles, and those of four families climb by the tips of their leaves. In almost all of them the young internodes revolve, in some of them as extensively as in twining plants,—the movement being plainly serviceable in bringing the petioles or the tips of the leaves into contact with surrounding objects. Those whose shoots revolve most freely are also capable of twining spirally around a support; but when the stem twines (as in *Clematis Sieboldii* and *calycina*, but not in *C. Viticella*), it has the peculiarity of winding first in one direction for two or three turns, and then in the opposite direction. The petioles are principally efficient in these plants, and that by means of an endowment which is not shown to belong to twining stems, with one or two exceptions. That is, the petioles or their divisions are sensitive to the contact of an extraneous body, contracting on the side touched so as to curve or coil around it. That the footstalk is directly sensitive to the touch, just as tendrils are, Mr. Darwin proved by lightly rubbing them with a twig for a few times, when in the course of some hours it bends to the rubbed side, afterwards becoming straight again; or by leaving the body in contact it is permanently clasped by the footstalk. So sensitive are some footstalks that "a loop of thread weighing a quarter of a grain caused them to bend; a loop weighing one-eighth of a grain sometimes acted, and sometimes not." In one instance, in *Clematis Flammula*, even the sixteenth part of a grain caused a petiole to bend through nearly 90 degrees. With rare exceptions only the young petioles are sensitive. Take the cultivated *Clematis Viticella* for an illustration of the mode in which the leaves do the work of climbing.

"The leaves are of large size. There are three pairs of lateral leaflets and a terminal one, all borne by rather long petioles. The main petiole bends a little, angularly, downwards at each point where a pair of leaflets arises, and the petiole of the terminal leaflet is bent downward at right angles; hence the whole petiole, with its rectangularly bent extremity, acts as a hook. This, with the lateral petioles directed a little upward, forms an excellent grappling apparatus by which the leaves readily become entangled with surrounding objects. If they catch nothing, the whole petiole ultimately grows straight. Both the medial and lateral petioles are sensitive; and the three branches, into which the basi-lateral petioles are generally subdivided, likewise are sensitive. The basal portion of the main petiole, between the stem and the first pair of leaflets, is less sensitive than the remainder, but it will clasp a stick when in contact. On the other hand, the inferior surface of the rectangularly bent terminal portion (carrying the terminal leaflet), which forms the inner side of the end of the hook, is the most sensitive part; and this portion is manifestly best adapted to catch distant supports. To show the difference in sensibility, I gently placed loops of string of the same weight (in one instance weighing ·82 of a grain) on the several lateral and on the terminal sub-petioles; in a few hours the latter were bent, but after 24 h. no effect was produced on any of the lateral petioles. Again, a terminal sub-petiole placed in contact with a thin stick became sensibly curved in 45 m., and in 1 h. 10 m. had moved through ninety degrees, whereas a lateral petiole did not become sensibly curved until 3 h. 30 m. had elapsed. In this latter case, and in all other such cases, if the sticks be taken away, the petioles continue to move during many hours afterward; so they do after a slight rubbing; but ultimately, if the flexure has not been very great or long-continued, they become, after about a day's interval, straight again."—(p. 31.)

In numerous cases, notably in *Solanum jasminoides*, the petiole when clasped increases very greatly in thickness and rigidity, undergoing a change in its woody structure by which the fibro-vascular bundles, originally semilunar in cross-section, develop into a closed ring, like that of an exogenous stem.

Lophospermum scandens of the gardens climbs, like its allies *Maurandia* and *Rhodochiton*, by clasping petioles; but in this plant, alone, the young internodes are also sensitive to the touch.

"When a petiole clasps a stick, it draws the base of the internode against it; and then the internode itself bends toward the stick, which is thus caught between the stem and the petiole as by a pair of pincers. The internode straightens itself again, excepting the part in contact with the stick. Young internodes alone are sensitive, and these are sensitive on all sides along their whole length. I made fifteen trials by lightly rubbing two or three times with a thin twig several internodes; and in about 2 h., but in one case in 3 h., all became bent: they became straight again in about 4 h., subsequently. An internode, which was rubbed as much as six or seven times with a twig, became just perceptibly curved in 1 h. 15 m., and subsequently in 3 h. the curvature increased much; the internode became straight again in the course of the night. I rubbed some internodes one day on one side, and the next day on the opposite side or at right angles; and the curvature was always toward the rubbed side."

Here, then, is one case in which the sensibility of a stem is manifest, and is turned to useful account. The peduncles of the allied *Maurandia semperflorens* are also sensitive and flexuous, although Mr. Darwin insists that they are useless for climbing. That some stems should be sensitive might have been expected; for tendrils of axial nature (e. qr. of *Passiflora gracilis*) are not less sensitive than those of foliar nature, as of *Leguminosæ*, *Cucurbitaceæ* and *Cobæa*. And if twining stems in general are not endowed with "a dull kind of irritability," as Mohl conjectured, it may well be because the equally wonderful automatic revolving movement leaves no need for it. In general, the most striking cases of automatic movement belong to leaves or their homologues.

The distinction can be only somewhat arbitrarily drawn between Leaf-climbers,—especially those with small or undeveloped leaflets, or where the tip of the leaf forms a hook or tendril-like projection,—and Tendril-climbers. The tendril, however, whether answering to leaf or stem, is the more specialized organ, adapted only for climbing, and endowed in different plants with very various and some highly remarkable powers. To this subject Mr. Darwin has devoted more than half of his essay. An analysis of it must be deferred, for want of space.

Near the close of the essay, under Hook-climbers, Mr. Darwin remarks that:—

"Even some of the climbing Roses will ascend the walls of a tall house, if covered with a trellis; how this is effected I know not; for the young shoots of one such Rose, when placed in a pot in a window, bent irregularly toward the light during the day and from it during the night, like any other plant; so that it is not easy to understand how the shoots can get under a trellis close to a wall."

Now we have had occasion to observe that the strong summer-shoots of Michigan Rose (*Rosa setigera* Mx., *R. rubifolia* R. Br.), trained on a latticed wall, are strongly disposed to push into dark crevices and away from the light; they would, many of them, pretty surely place themselves under the trellis, and the lateral shoots of the next spring would emerge as they seek the light. We suspect this is also true of the Sweet Brier.

A. G.

4. *Gradation from "Individual Peculiarities" to Species in Insects.*—The following are the concluding paragraphs of a paper by Dr. B. D. Walsh "*On Phytophagic Varieties and Phytophagic Species.*" The name *phytophagic* is given to those otherwise identical insects which differ, as varieties or species, according to the species of plant they feed upon. "When certain unimportant characters in the insect are correlated with the food-plant, while at the same time there is no sufficient reason to doubt that the two varieties freely intercross," the forms are called *phytophagic varieties*. When, from the lack of intermediate forms, intercrossing may be inferred not to take place, they are called *phytophagic species*. Dr. Walsh sums up his conclusions thus:

"From the facts referred to above and those recorded by me elsewhere, we may construct the following almost unbroken series, from the first dawnings of the Phytophagic Variety to the full development of the Phytophagic Species.

1st. Difference of food, even when the food-plant belongs to widely distinct botanical families, is accompanied by no difference whatever, either in the larva, pupa or imago state.—*Attacus Cecropia* Lin., *Dryocampa imperialis* Drury, *Lachnus Caryæ* Harris, (*Proc. Ent. Soc. Phil.* I, p. 303), and hundreds of other species.

2nd. Difference of food is accompanied by a marked difference in the color of the silk-producing secretions.—*Bombyx Mori* Lin., the common silkworm.

3rd. Difference of food is accompanied by a tendency toward the obliteration of the normal dark markings in the imago.—*Haltica alternata* Illig.

4th. Difference of food is accompanied by marked, but not perfectly constant, colorational differences in the larva, but none whatever in the ♂ ♀ imago.—*Datana Ministra* Drury.

5th. Difference of food is accompanied by a marked and perfectly constant difference in the size of the imago.—*Chrysomela scalaris* Lec.

6th. Difference of food is accompanied by a marked difference in the chemical properties of gall-producing secretions, the external characters of the ♂ ♀ imago remaining identical.—*Cynips q. spongifica* O. S. and *C. q. inanis* O. S.

7th. Difference of food is accompanied by a slight, but constant change in the coloration of the abdomen of the ♂ ♀ imago, and by a very slight change in the chemical properties of the gall-producing secretions, the galls of the two insects, though typically somewhat distinct, being connected by intermediate grades in the case of the latter.—*Cynips q. punctata* Bassett and *C. q. Podagræ* Walsh.

8th. Difference of food is accompanied by one marked and perfectly constant colorational difference, and others which are not perfectly constant, in the larva, but none whatever in the ♂ ♀ imago.—*Halesidota tessellaris* Sm. Abb. and *H. Antiphola* Walsh.

9th. Difference of food is accompanied by several slight but constant structural differences in the ♂ imago, but none whatever in the ♀ imago.—*Clytus Robiniæ* Forst. and *Cl. pictus* Drury.

10th. Difference of food is accompanied by a slight but constant structural difference in both ♂ and ♀ imago.—1. *Tingis Tiliæ* n. sp. and *T. amorphæ* n. sp. 2. (Doubtful.) *Diapheromera femorata* Say and *D. Velii* n. sp.

11th. (Doubtful.) Difference of food is accompanied by very strong structural and colorational differences in the larva and in all probability by a constant structural difference of generic value in the ♀ imago, the ♂ imagos being to all external appearances identical, and the two insects belonging to different genera.—*Sphingicampa distigma* ♂ ♀ Walsh and *Dryocampa bicolor* ♂ Harris.

12th. Difference of food is accompanied by marked and constant differences, either colorational, or structural, or both, in the larva, pupa and imago states.—*Halesidota tessellaris* Sm. Abb. and *H. Caryæ* Harris, and hundreds of species belonging to the same genus and commonly considered as distinct species.

The constitution of the human mind is such, that the same evidence carries with it very different degrees of weight, when presented to different intellects. Others will no doubt draw different conclusions from the facts catalogued above; but for my own part, as on the most careful consideration I am unable to draw any definite line in the above series, and to say with certainty that here end the Varieties and here begin the Species, I am therefore irresistibly led to believe, that the former gradually strengthen and become developed into the latter, and that the difference between them is merely one of mode and degree."

5. *Illustrated Catalogue of the Museum of Comparative Zoology at Harvard College.* No. 1, *Ophiuridæ and Astrophytidæ;* by THEODORE LYMAN. 200 pp., large 8vo, with two colored plates. Cambridge, 1865.—The Museum of Comparative Zoology, under the directorship of Professor Agassiz, at Cambridge, has already become a vast collection in some departments of zoology, and is on the rapid increase. The idea of connecting with a catalogue of the Museum the publication of occasional memoirs upon the species here gathered, has for some time been in contemplation, and the first number has just been issued with the above

title. The style of publication selected for the series is all, as regards paper, type and arrangement, that the eye could desire, and this first memoir, by Mr. Lyman, makes a most excellent beginning of the series. The Ophiuridæ and related Asterioids have been for some years his special department of study. The memoir contains detailed descriptions of the species from the shores of North and South America and Greenland, that are now in the Museum and also in the collections of the Smithsonian Institution, including in all 105 species, of which 26 are new; and it is illustrated by two beautiful colored plates, on which seven of the species are represented. It contains also an extended bibliography, and a table of the known species of Ophiuridæ and Astrophytidæ.

IV. ASTRONOMY AND METEOROLOGY.

1. *Shooting Stars seen at Hinsdale, Mass., in August*, 1865.—The following observations were made on the days specified below, the writer's station being at Hinsdale, Mass., about W. long. 73°, N. lat. 42½°. During all the observations the atmosphere was pure and unclouded, but the moonlight interfered with all but the brighter paths. The area of observation was centered considerably north of Perseus, but reached generally to Auriga. It was not, however, more than four-fifths of the field ordinarily due to a single observer.

Aug. 9th.—From $2^h\ 20^m$ to $3^h\ 45^m$ A. M., an interval of $1^h\ 25^m$, ten meteors were seen, of which nine were closely conformable to a radiant centering in A. R. 47°, N. P. D. 33°.

Aug. 10th.—Between $2^h\ 25^m$ A. M. and $3^h\ 50^m$, an interval of $1^h\ 25^m$, nineteen meteors were seen, of which *all were conformable* to a radiant centering in A.R. 42°, N. P. D. 34°; but elongated some 4°, either way, across the meridians.

Aug. 11th and 12th.—The sky was wholly obscured by clouds.

Aug. 13th.—Between $10^h\ 45^m$ P. M., and $0^h\ 45^m$ of Aug. 13th, an interval of 2^h, sixteen meteors were seen, of which twelve were conformable closely to a radiant point in A. R. 52°, N. P. D. 32°. Two-thirds of the whole number were seen in the first three quarters of an hour.

Aug. 14th.—Between $0^h\ 10^m$ A. M. to $1^h\ 35^m$, an interval of $1^h\ 25^m$, twenty meteors were seen, generally slow in angular velocity and wholly destitute of a *regimen* to their lines of direction, except that three of the earliest and one of the latest conformed to an area of several degrees around A. R. 68° N. P. D. 18°. Three of the flights were very long but *unstable* in motion—say 30° of arc traversed in 2½ to 3 seconds of time.

It seems to be clear from the foregoing that the proper August meteors in this instance, appeared as early, at least, as the first morning hours of the 9th, and were not discernable later, at least, than 11 P. M. of Aug. 12th. In other words, the duration of the phenomenon did not exceed four days, and probably did not quite cover that duration.

Notwithstanding the impracticability of observing on the important mornings of the 11th and 12th, a shifting of the radiants, as above specified, was unmistakable. A. C. T.

2. *Shooting Stars seen at New Haven in August*, 1865.—On the night of Aug. 9–10th I saw, during half an hour, ending about 2^h A. M., ten shooting stars, about two-thirds of them moving from Perseus. The

sky was clear, but the bright moon, then two days past the quarter, very greatly interfered with observation.

The next night was the one on which the greatest display was looked for. A numerous party was ready to watch, but could do nothing because of the clouds.

On the night of the 11–12th, during one hour ending about midnight, I saw fourteen. The sky was again clear, but the moon interfered. Not more than half radiated from Perseus. But for the light of the moon the number visible on this night, as well as on the night of Aug. 9–10th, would have been no doubt twice as many, probably three times as many, as were actually visible.

On the night of Aug. 15–16th, C. Tomlinson, M.D., Messrs. A. Van Name, J. Avery, G. L. Woodhull, and A. W. Gates, Col. B. S. Pardee and myself, watched from the roof of the New Haven House, during three hours, from five minutes past eleven till five minutes past three o'clock. All but myself were constantly watching the sky, and saw during the first hour 55 meteors, during the second hour 56, and during the third hour 61. About half of my time was occupied in keeping the record. Of twenty-five seen by me six were not seen by others, so that we saw in all 178 meteors, or about one each minute. Besides these, however, there were at least seventeen instances in which some one of the party thought he saw a shooting star, but was not confident of it. The moon rose at 12^h 40^m, and from that time the number seen was apparently diminished a little by its light. A fog which was rising toward the end of the watch also interfered. Shortly after two o'clock the fog closed entirely over us.

The special object of the watch was to determine what proportion of the meteors visible at one place would be seen by one observer; in other words, by what factor the number seen during any time by one person should be multiplied to obtain the whole number visible at the place. The conclusion, as derived from this night's work, is that this factor is at least 4·5, and cannot safely be assumed as greater than 5. The details of the observations, and the discussion, will be given hereafter.

During the night there was a slight aurora, and just before the moon rose there was a bright auroral cloud, or compact cluster of short streamers in the northeast. It was probably illumined by the moon, as suggested below by Prof. Twining. H. A. N.

3. *Auroral Phenomena of August* 3, 1865.—On the night of August 2d–3d, there was witnessed at New Haven a display of the Aurora Borealis, of great brilliancy notwithstanding the light of the moon—then nearly full. An intelligent witness states that, just before 11 o'clock P. M. on the 2d, he saw a nebulous column or streamer of white luminosity, ascending from far down in the east-of-south high up toward a point north of the zenith,—and simultaneously with it a similar column in the northwest rising toward the same high altitude. The columns were but a degree or two broad, and did not, while this witness saw them, unite to form a complete arch. When he ceased looking—at about a quarter past 11 o'clock—they were undiminished in extent and brightness. They were said, by others who witnessed the same, to have faded not long after.

When the writer first observed them, at about 1h 45m A.M. on the 3d, the auroral lights were spread in a continuous and luminous sheet over more than half the sky through west, north and east,—and southward on the meridian to about N. P. D. 70°. This luminous sheet was composed of streamers in irregular groups, not so dense in nebulosity, nor so great in linear extent, as the writer has often witnessed; but unusually abundant, and forming a very *stable* corona at A.R. 341° N.P.D. 64½°,—this being a little east of the meridian and, as usual, at the magnetic pole or near it. The entire assemblage of streamers down to within say 18° of the horizon was pervaded by a rush upward of auroral waves in a constant and quite regular succession. Two to four of these waves might be seen following one another, but with interrupted courses, up toward the corona. Their brightness at the corona was not less than in any other part, and I estimated their medium angular motion at about 90° in a second. The streamers were moving from west to east—contrary to the usual direction of this movement, and it constitutes the second instance only in which the writer remembers to have witnessed such an anomaly. This movement, as naturally suggested by the phenomenon itself when first noticed many years ago, is a result of the relative direction of electrical currents along the streamers and the like currents of the earth and atmosphere. At 2h 30m a cluster 23° high in the N.E. by E., was observed to have about 2° a minute of this lateral movement. It was more or less rapid in other parts, in obedience to the laws of perspective.

At 2h 40m A.M. the phenomena had faded, and clouds in extended masses obscured the view. These increased until toward the first appearance of twilight; but through the rifts and clear spaces in the N. and N.N.W. the bright banks of auroral vapor were still seen at a low altitude. Also along the eastern sky there was left a clear belt from the horizon upward in which clusters of short streamers were visible, and, rarely, a faint auroral wave.

It had occurred during one of those grand auroral developments which the writer witnessed between 1838 and 1848 from his place of residence at Middlebury, Vermont, that, in one instance, at a very early morning hour, he was viewing an assemblage of enormous streamers which arose from the north, when he noticed that an upper segment of each streamer became of a rosy hue, very vivid, and extending down to about the same altitude in all. This he *suspected* to be the effect of sunlight striking the tops of those streamers at a height of several hundred miles—as it must have been at that hour—above the earth's surface. But, in the great number and variety of appearances which solicited notice, he omitted to make such memoranda of time, altitude, and positions, as would have enabled him afterwards to test the plausibility of this explanation. Therefore the phenomenon has rested without particular notice; but on the morning of the 3d inst. it received a remarkable *reiteration*, and the writer now ventures to commend this particular of observation to others who may have opportunity to make it, whether by sunlight or moonlight. It not only offers a method of determining approximate heights, but it may also determine, in certain respects, the physical properties of the material in the Northern Lights which possesses luminosity. At 3h 30m and onward there was, in the vicinity of Orion and above it, a limited and solitary but distinct cluster of short and yellowish white streamers, in about 5° S. of E., and 17° high. This was earnestly attended to, and, at 3^h 40^m—as noticed by *feeling* the minute hand of the watch—its upper part began to be intensified in color with a speedy progress downward and northward which disclosed an increased extension in those directions, and north of the cluster a considerable extent of fainter streamers. The yellow had now become not merely intensified suddenly in hue but changed in quality, or imagined to be so. What I would specially notice is that the color of the faint streamers, although now as bright as the aforementioned cluster had been at first, appeared not of the same nebulous yellow but *glistening* in effect, although far less brilliant than the cluster had itself become. Soon the latter turned to a clear but dilute red, like the same commonly witnessed in auroras, but which appeared now, for the first time, in this. This rosy color propagated itself, as the glistening yellow had done before, and soon characterized the whole, and so continued until the twilight overpowered it.

The observations of time, altitude and azimuth in this instance are not clear enough for exactness, but they give—after allowing for atmospheric refraction—about fifty miles for the average height of the cluster above described.

This of the 2d–3d inst., was, by reason of its extent, duration and completeness, one of the class of grand auroras—although far less brilliant and variegated than several which have been seen at this place since 1830. In but one instance however—and that one but poorly developed—did it embrace any of those fan-like curtains or assemblages of streamers which sweep down to a definite altitude, for the time, and having a termination so abrupt and so correspondent in all that the *suspicion* may be allowed (and the suggestion of it pardoned) that this abrupt development takes place at the definite upper boundary of the atmosphere proper—at the transition, or *rebroussement*, which it would seem must there exist, from a lower to the secondary atmosphere, or, if that supposition is preferred, to the simply ethereal spaces.[1] A. C. T.

New Haven, Conn., August 8th.

4. *Comparative intensity of the light reflected from the Moon and Venus;* by Mr. CHACORNAC.—On the 20th of June last, at 3^h A. M., the Moon and Venus were in conjunction in the latitude of Lyons, France, so that both bodies could be seen in the same field of vision, and use was made of the opportunity to compare the light received from them. The surfaces taken for comparison were those affording rays at the same angle of incidence; and on the Moon, the region was that lying between the craters Rocca and Eirchstadt, over the very brilliant surface which lies to the southeast of Grimaldi. It was found that the light from this brightest part of the Moon was only a tenth of that reflected by the surface of Venus.—*Comptes Rend.*, lviii; *Les Mondes*, July 20.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Magnesium.*—Magnesium light contains an extraordinary proportion of ultra-violet or chemical rays, this part of the spectrum between the extreme violet and the extreme red being six times as large as usual,

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[1] As suggested in this Journal, xxvii, 19.

and it is particularly efficient for producing fluorescent and photographic effects. Very remarkable fluorescence may be obtained by exposing to the light a paste made of powdered platino-cyanid of barium and gum arabic.—*Les Mondes, June* 22.

2. *The Rumford Premium.*—The American Academy of Arts and Sciences, at its annual meeting on the 23d of May last, voted: "That the Rumford Premium [a Gold Medal] be awarded to Professor Daniel Treadwell, for improvements in the management of heat, embodied in his investigations and inventions relating to the construction of cannon of large calibre, and of great strength and endurance."

3. *The Prince Albert Medal.*—This gold medal, founded by the Society of Arts in honor of the memory of its late President, his Royal Highness Prince Albert, has been given this year to the French Emperor, Napoleon, for his encouragement of the arts, manufactures and commerce, his patronage of the fine arts, etc.

4. *Exploration of the Urals.*—Von Helmersen, of St. Petersburg, has been commissioned by the Russian Government to explore the central parts of the Urals, with special reference to the discovery of coal, which some geologists of distinction have thought to exist there.

5. *Mineral Waters.*—Scouteten, in a paper read before the Academy of Sciences at Paris in July, endeavors to demonstrate that the medicinal value of mineral waters is due mainly to their electrical condition.

6. *Association for the Advancement of Science in France.*—This association held an extraordinary session at Marseilles, in May, commencing on Friday the 26th, on the occasion of the inauguration of the Observatory there, at which Le Verrier was present.—*Les Mondes, June* 22.

7. *A Central Meteorological Bureau in Prussia* was instituted on the 1st of May. It will be connected with the principal ports of Prussia, and will make regular announcements of all meteorological phenomena, storms, etc., to the bureau. The atmospheric bulletins of the principal cities of Europe, containing especially whatever relates to the progress of storms, will be placarded in the principal seaports.

8. *Goeppert on the Diamond.*—Goeppert has published his essay on the organic nature of the diamond. He shows that it cannot be of igneous origin; for it turns black when highly heated. Moreover it contains sometimes, besides other crystals, germs of Fungi, and fibres of vegetable structures higher in organization.

OBITUARY.

SILBERMANN, Curator of the Museum of the Conservatoire des Arts et Metier, died in July, at Paris, in his fifty-ninth year.

Dr. SAMUEL P. WOODWARD, F.G.S., a member of the Council of the Geological Society, died at Herne Bay, on the 11th of July, at the age of forty-three. Dr. Woodward was assistant in the department of geology and mineralogy in the British Museum. He is the author of various scientific papers, and between the years 1851 and 1856, published his excellent work "A Manual of Recent and Fossil Shells."

Sir JOHN WILLIAM LUBBOCK, Bart., died in Kent in June last, in his sixty-third year. He was one of the Vice Presidents of the Royal Society.

Sir WILLIAM J. HOOKER, the eminent botanist, died in August.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *A treatise on the Assaying of Lead, Copper, Silver, Gold and Mercury from the German of* BODEMANN *and* KERL, *translated by* W. A. GOODYEAR, Ph.B. 12 mo, pp. 214, with plates; published for the Berzelius Trust Association, by John Wiley & Son. New York, 1865.—BODEMANN'S *Anleitung zur Berg- und Hütten-männischen Probierkunst*, the 2nd edition edited by BRUNO KERL, is well known as a work on assaying, which for accuracy of statement and fullness of detail is unsurpassed if not unequaled. The present translation of a most important part of this book is a very welcome addition to the literature of assaying in the English language. Assayers, who have heretofore been confined to the vagaries of Mitchell, as well as of most other English and American works on this subject, will take great satisfaction in studying the clear exposition of the theory of the processes, and the careful directions for the practical operations in assaying given in this book. Mr. Goodyear has endeavored to adhere strictly to the text in making his translation, and we think in some instances he has sacrificed clearness, and even accuracy, in too closely holding to the German idiom; but, notwithstanding, the translator deserves the thanks of chemists and assayers for this contribution to Laboratory literature. The book is published by the Berzelius Trust Association, a society of students connected with the Sheffield Scientific School of Yale College. G. J. B.

2. *The Declaration of Students of the Natural and Physical Sciences.* 30 pp., 8vo.—This pamphlet, issued recently in England, contains a declaration of regret that science is sometimes misused for throwing doubts upon the "truth and authenticity of the Holy Scriptures," followed by more than seven hundred signatures. Its object is excellent, and it is gratifying to find the names of such men as Sir David Brewster, Sir John Richardson and J. H. Balfour in the long catalogue. Yet the pamphlet is not satisfactory, because we are sure that there are large numbers of men of science in Great Britain, equally confident of the truth of the Bible, whose names are not included. They have objected to giving their names, as English journals have intimated, because there was something of reproach implied against others, who, they knew, whatever their views respecting the Bible, were honest searchers for truth; and because also, as they thought, this division of men of science into seemingly hostile ranks was calculated to operate against that independence in investigation which is essential to real progress in science.

For ourselves we regard such efforts to sustain the Bible unnecessary. They may give courage to some wavering minds. But, on the other hand, they exhibit a fear of men which is unbecoming, and which, at the same time, is injurious to the cause they would sustain. We would rather speed on investigators of nature, in their various lines of research, feeling confident that whatever error be now mingled with the truth will thereby be the sooner eliminated; and that when thus eliminated, Nature and the Bible will manifest all the more convincingly their oneness of authorship.

3. *Smithsonian Report for* 1863.—This volume contains, in addition to the Report on the operations, expenditures and condition of the Smith-

sonian Institution for 1863, and the Report of the Assistant Secretary on the Museum, the following important papers:—

Lectures on the Principles of Linguistic Science, by Prof. W. D. Whitney.

Memoir of C. F. Beautemps-Beaupré, by M. Elie de Beaumont.

Origin and History of the Royal Society of London, prepared by C. A. Alexander.

Modern Theory of Chemical Types, by Dr. Charles M. Wetherill.

Researches on the Phenomena which Accompany the Propagation of Electricity in Highly Rarefied Elastic Fluids, by Prof. A. de la Rive.

Report on the Proceedings of the Society of Physics and Natural History of Geneva, from July, 1862, to June 1863, by Prof. Marcet.

Experimental and Theoretical Researches on the Figures of Equilibrium of a Liquid Mass withdrawn from the Action of Gravity, &c., by Prof. J. Plateau, (39 wood-cuts).

History of Discovery Relative to Magnetism.

Recent Researches Relative to the Nebulæ, by Prof. Gautier.

Figure of the Earth, by Sr. Miguel Merino.

Aeronautic Voyages performed with a view to the advancement of Science, by Francis Arago.

Account of Balloon Ascensions, by James Glashier.

Account of the Aboriginal Inhabitants of the Californian Peninsula, by Baegert, translated by Prof. C. Rau.

Ethnology.—Account of Kjœkken-Mœdding in Nova Scotia, by J. M. Jones, of Halifax; Abstract of the Fifth Report of Dr. Keller on Lacustrian Settlements, by A. Morlot; Agricultural Implements of the North American Stone Period, by Charles Rau, (2 wood-cuts); Account of Ancient Fort and Burial Ground in Tompkins county, New York, by David Trowbridge; Account of Ancient Town in Minnesota, by O. H. Kelley; Account of Ancient Relics found in Missouri, by J. W. Foster; Account of a Mound in East Tennessee, by A. F. Danilsen.

Purple and Azure Dyeing, Ancient and Modern, translated from *Aus der Natur.*

Method of Preserving Lepidoptera, by Titian R. Peale, (4 wood-cuts).

Account of a Remarkable Accumulation of Bats, by M. Figanierre è Morago, Portuguese Minister to the United States.

Tables of Weights and Measures.

4. *Quartz Operators' Hand-Book.* Wheeler & Randall, San Francisco, 1865. 16mo. pp. 130.—This little manual is intended to give an outline of the various mechanical and chemical methods in use for treatment of gold and silver ores, especially those adopted in California and Nevada. In the space of forty small pages the authors treat of the blowpipe, chemical tests, the dry and humid assay, wasting, treatment of gold quartz by pan process, extraction of gold by chlorination (Plattner's method), extraction of gold by the Freiberg process, the platinum process—cupellation, Augustin process and Ziervogel's process, besides the patio, Patera, Veatch's and the pan processes for silver ores. This extreme brevity renders the first portion of the book of little value to any class of readers. The mechanical rules and formulæ, which fill much the larger part of the Hand Book, will be found

useful to all mill men and others concerned in the management of mining machinery. Mr. Wheeler is well known in California as a successful millwright and the inventor of the "Pan," or grinder, which bears his name, so generally used in gold and silver reduction works. Mr. Randall has modified this invention by moulding its grinding surfaces in the form of the tractory conoid. His mathematical discussion of the tractory and differently formed grinding surfaces given to plates in pans of various names, is original and ingenious. The practical result claimed by the author for his own system is expressed in the following recapitulation:—

Tractory conoidal plates of uniform hardness =1·7778.
Conical plates of uniform hardness =1·1042.
Plain circular plates of uniform hardness =·9877.
Randall's patent grinding plates =1·1752.

5. *The Scientific Review and Journal of the Inventor's Institute*, No. 5, July 1, 1865; 16. pp. sm. fol. Cassell, Pelter & Galpin, Ludgate Hill, London. Price 6d, stamped 7d.—A paper devoted mainly to practical science, but containing occasional articles on general principles in chemistry, physics and art.

6. Gesner, *Coal Oils*, (Bailliere, 1865).—This is a new edition of a work noticed in a previous number of this Journal, and contains many interesting details respecting the remarkable development of the Petroleum industry in the United States.

7. *Tours of a Chess Knight*, by S. S. Haldeman; Philadelphia, E. H. Butler & Co., 1864, 16mo., 90 pp., 114 figures; also Bibliography of the Chess Knight's Tour, (same author and publishers), 42 pp.—This little book treats of a problem, not so much of Chess as of the Geometry of position. The Chess Knight is required, without or with other conditions, to pass over the sixty-four squares of the chess board in succession. Euler thought the problem worthy of an Academic memoir of 27 quarto pages, and De Jeanisch, in his *Traité des Applications de l'Analyse Mathèmatique au Jeu des Échecs* (2 vols. 8vo.) has devoted to it all of the second volume and part of the first. The bibliography which forms the second part of the present work has over sixty references.

Essay on the Trees and Shrubs of the Ancients, being the substance of four lectures delivered before the University of Oxford, intended to be supplementary to those on Roman Husbandry; by C. Daubeny, M.D., F.R.S., Prof. Bot. etc., Oxford. London. (J. H. & J. Parker.)

The Natural History of the Precious Stones and of the Precious Metals, by C. W. King, M.A., author of "Antique Gems," etc. 1865. London. (Bell & Daldy.)

Saturn and its System, containing Discussions of the Motions (real and apparent) and telescopic appearance of the planet Saturn, its Satellites and Rings, etc., etc.; by Richard A. Proctor, B.A. Illustrated by 14 engravings on steel and copper. London. (Longman's.)

Pre-historic Times, as illustrated by Ancient Remains, and the Manners and Customs of Modern Savages: by John Lubbock, F.R.S. London. (Williams & Norgate.)

Lake-habitations and pre-historic Remains in the Turbaries and Marl-beds of Northern and Central Italy; by Bartolomeo Gastaldi. Translated from the Italian, and edited by C. H. Chambers. London. 1865. (Longman & Co.)

Ice-Caves of France and Switzerland, a narrative of Subterranean Exploration; by Rev. G. F. Browne. London. 1865. (Longman & Co.)

The Anthropological Treatises of J. F. Blumenbach, with Memoirs of him by Marx & Flourens, and an account of his Anthropological Museum by Prof. R. Wagner; and the Inaugural Dissertation of John Hunter, M.D., on the Varieties of

Man. Translated and edited by Thomas Bendyshe, M.A. London. 1865. (Published for the Anthropological Society by Longman & Co.)

Catalogus systematicus Dipterorum Europæ; von Dr. J. Schiner. Published in a small edition by the zoologisch-botanischen Gesellschaft of Vienna. Price 1 fl. 20 kr. Also, by the same Society, Nouveau Système des Blattaires, by Charles Brunner, with 13 copper plates, will soon be published.

L'Etudiant micrographe: Traité théorique et pratique du Microscope et des préparations; par Arthur Chevalier. With 200 figures in the text, and plates of 300 Infusoria. 2nd ed., containing applications on the study of normal and pathological histology, by G. Pouchet; on microscopic botany, by VanHeurk; on Diatoms, by A. de Brebisson. Paris. 1865. (Adrien Delahaye.)

Les metaux précieux considérés au point de vue economique, par M. Roswag; 400 pp., with 28 figures in the text and 16 colored plates. Paris. (Eugéne Lacroix). Dedicated to Quintino Sella, Mining Engineer and Minister of Finances in Italy.

Guide pratique d'Entomologie Agricole; par M. H. Gobin. Paris. (Eugene Lacroix.)

Verhandlungen der k. k. zoologischen-botanischen Gesellschaft in Wien. The 14th volume of this publication of the Zoologico-botanical Society of Vienna was issued in 1864. It contains numerous zoological and botanical papers, and 20 copper plates, mostly colored, and covers 920 pp. 8vo. Among the longer papers are a systematized list of the species of Lepiduptera, including all described species, by C. & R. Felder; a review of the names of fossils and living species of the genus Paludina *Lam.*, by Georg Ritter von Frauenfeld; a synonymic synopsis of the Phryganidæ by H. Hayen; Batrachological contributions by Dr. Franz Steindachner. There are also various miscellaneous zoological papers by Dr. Frauenfeld; one on the Cecidomyia destructor (wheat insect) of Say, by Prof. Haberlandt; a new system of the Diptera, by Dr. J. R. Schiner; and many others of interest, botanical as well as zoological.

Annals of the Lyceum Nat. Hist. of New York. Vol. VIII. Nos. 2, 3. 1864. —p. 4, Descriptions of New Species of Birds of the Families Tanagridæ, Cuculidæ and Trochilidæ with a Note on Panterpe insignis; *G. N. Lawrence.*—p. 46, Summary of a Meteorological Register for the year 1863 kept in the City of New York; *Prof. O. W. Morris.*—p. 50, On Ammobroma, a New Genus of Plants, allied to Corallophyllum and Pholisma; *J. Torrey.*—p. 57, Notes on Species of the Family Corbiculadæ with Figures; *T. Prime.*—p. 92, Catalogue of Birds collected at the Island of Sombrero, W. I., with observations by A. A. Julien; *G. N. Lawrence.*

Proceedings of the Amer. Phil. Soc., Vol. X, No. 73.—Page 18, Radical significance of Numerals; *P. E. Chase.*—p. 24, Heights of Auroras; *B. V. Marsh.*—p. 29, New Gold Crushing Machinery; *R. Briggs.*—p. 30, On some Indian Hieroglyphics at Safe Harbor; *S. C. Porter.*—p. 32, Relative Levels of Coal and Oil Regions; *P. W. Sheafer.*—p. 38, Petroleum of Kentucky, and Records of Borings in Pennsylvania; *J. P. Lesley.*—p. 69, A short Vocabulary of Well-established Copto-Egyptian Words for Convenient Use; *P. E. Chase.*—p. 97, Relations of the Magnetic Declination to Gravity; *P. E. Chase.*—p. 108, Effects of the late Tornado; *J. C. Cresson.*—p. 109, Venango County Oil Region; *R. Briggs.*—p. 110, Popular fallacies respecting Petroleum; *J. P. Lesley.*—p. 111, Relations of Gravity to the Vertical Inflection of the Magnetic Needle; *P. E. Chase.*—p. 121, Obituary Notice of Dr. Franklin Bache; *Geo. B. Wood,* M.D.—p. 137, On the Mythical Compounds of Bar; *J. P. Lesley.*

Proceedings of the Entomolog. Soc. Philadelphia, Vol. IV, No. 3, (concluding part of the volume).—p. 331, Contributions to the Natural History of the Cynipidæ of the United States, and of their Galls; *R. O. Sacken.*—p. 381, Descriptions of several new species of North American Coleoptera; *J. H. B. Bland.*—p. 385, Descriptions of some new species of Mutilla from California; *E. T. Cresson.*—p. 390, Notes on Papilio asterias and Saturnia Promethea hermaphrodites; *W. H. Edwards.*—p. 391, Compiled descriptions of North American Staphylinidæ; *J. H. B. Bland.*—p. 421, Catalogue of Hymenoptera in the Collection of the Entomological Society of Philadelphia, from Colorado Territory (continuation); *E. T. Cresson.*—p. 489, Lepidopterological Notes and Descriptions, No. 1; *A. R. Grote & C. S. Robinson.*

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ART. XXX.—*On the Origin of Prairies;* by JAMES D. DANA.

IN the articles on the Origin of Prairies recently communicated to this Journal by Prof. Winchell and Mr. Lesquereux,[1] presenting independent theories, the view that this origin is in any way due to the dryness of the climate is unnoticed except with the endeavor to set it aside.

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[1] See for Prof. Winchell's paper, xxxviii, p. 332, and for Mr. Lesquereux's, xxxix, 317, and xl, 23. Prof. Winchell supposes that the soil of the prairies is of lacustrine origin, produced during an epoch of a vast inland fresh-water sea following the glacial epoch; and that in the silt or "slime" of such a sea all pre-existing seed would disappear, so that the vegetation that would spring up over the recovered land would be only that which seeds transported from distant regions would afford, and that this "was more likely to be herbaceous than arboreal."

Mr. Lesquereux holds that the prairie land was recovered from the borders of lakes, rivers, and seas, (fresh or salt), by a process like that exemplified now on the borders of Lake Michigan and the Mississippi, Minnesota and other rivers, in which the region becomes, to a great extent, first a flat of shallow waters, and then a marsh or bog, and that such soil, or mould, owing to the nature of the moss, Confervas, Characeæ, etc., which grow in stagnant waters and contribute to it, becomes next, as a matter of course, covered with sedges and grass. This resulting mould, he observes on p. 318, partakes "as much of the nature of peat as of that of true humus;" it is "impregnated with a large proportion of ulmic acid, produced by the slow decomposition, mostly under water, of aquatic plants;" and "the acid of this soil, by its particularly antiseptic properties, promotes the growth of a group of plants mostly herbaceous," while the want of a free access of oxygen is unfavorable to trees. He adds, "of all the trees, the tamarac is the only species which, in our northern climate, can grow on a peaty ground."

This explanation is stated to cover almost all cases of natural prairies,—those "between the base of the Rocky Mountains and the Mississippi," the "Platas of the Paraguay and Madeyra," the "Pampas of Brazil," "the desert plains of the western salt lakes;" while in "the natural meadows of Holland," "the plains on the shores of the North Sea and the Baltic," "the vast steppes of the Caspian," etc., etc., there are "the same appearances, and the same results of a general identical action, modified only by local and mostly climatic circumstances."

The writer, in his Manual of Geology, makes, on the contrary, the degree of moisture the most influential of all causes that tend to determine either the presence of forests or absence of prairies. This idea, although not claimed as original with him,[1] is the result in his mind of original observations made in various parts of the world; and he proposes briefly to state some of the facts in its favor which he has noticed.

On arriving, in 1839, off the north side of the island of Tahiti, one of the first of the Pacific islands visited by the Exploring Expedition under Capt. Wilkes, it was a matter of much surprise that the land, so famed for its tropical fertility and beauty, was bare of trees over the lower ten or fifteen hundred feet of its slopes, the forest trees being confined—the valleys excepted—to regions above this limit. The explanation was at once suggested—that the bare backs forming the sloping surface that lay between the valleys were too dry for trees, in contrast with the region higher up, which was one of very frequent clouds, mists and rains. The existence of forests in the valleys stretching down below the limit accorded with the explanation; for there the declivities were dripping with moisture coming out from between the nearly horizontal layers of the rocks, besides being the courses of many streamlets from the moister region above, and the route of all its rills. Observations afterwards made on the ascent of one of the lofty summits of the island, and on other excursions, confirmed this inference. Over the bare slopes there were no marshy areas; and there was positive proof in the structure of the island, the dip of the rocks beneath being always seaward, and in the nature of the surface, that there never had been such areas; while on the upper slopes, where moisture was abundant, and the ground often boggy, trees were of luxuriant growth.

Similar facts were observed on other Pacific Islands. One of them, Upolu, of the Navigator group, "was covered throughout with forests; or rather it was one dense forest from the extreme east to the west end, and from the water's edge to the very summit of the most rugged peaks. The natives had spread their cocoanut groves and bread fruit trees along the shores, but in many places the line of forests remained yet unbroken. Shrubbery and sugar cane covered some parts of the lower declivities, but there was nowhere a spot of pasture land." Parallel with this condition "the island is in general well-watered.

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[1] Among authors who have advocated this view, Professor Guyot may here be mentioned; and since the preparation of this paper, the writer has learned that this eminent physical geographer has in hand a memoir on the connection between the distribution of forests over the world and that of moisture and the causes of the latter.

There is scarcely a day without low and heavy clouds about the summits of the mountains. Many streams of moderate size flow down both sides of the island to the sea, and numerous fountains gush out along the shores.[3]

Again at the Feejee Islands "luxuriant forests cover the elevated parts of the ridges where the frequent rains and more frequent mists and clouds afford them the necessary moisture," while lower down, "the slopes, especially to leeward, are covered with grass;" "the forest vegetation descends lower on the eastern declivities because they are well supplied with moisture from the trade winds—the rain of the southeastern side of Viti Lebu at least trebling that of the opposite side."[4]

The rocks at the Navigators and the Feejees are of volcanic or igneous origin, like those of Tahiti, and similar to the latter in composition and structure.

On the western coast of America the distribution of trees was found to sustain the same view, as the writer has already explained in this Journal.[5] Oregon, along the sea-border, over a breadth of 15 to 25 miles, is mostly covered with forests made up of large evergreens, some of the trees being 300 feet in height and 50 in girt; and the heavy sea-mists extend inland for about the same distance, covering the land the greater part of the time, even through the period of the summer droughts. And on the ascent of the Columbia, there is usually in summer a sudden transition from the mist region to that of clear brazen skies. Residing, in July 1841, at Astoria, within 20 miles of the mouth of the river, for three weeks we had hardly one day clear throughout; and for the following month, at Fort Vancouver, 100 miles from its mouth, we hardly saw a cloud the whole time, and the lofty peaks of St. Helen's and Hood were generally without a cloud to their very top, indicating the dryness of the climate over this interior region.

Beyond the forest border of Oregon, to the eastward, the plains and rolling hills, as those of the Willammet region, are bare of trees, excepting a sparse scattering of oaks, and lines of trees on the borders of the rivers, and along the slope between the upper and lower alluvial flat or terrace. But the higher hills, above one or two thousand feet in elevation, are largely forest-covered; for these, owing to their height and coolness, condense some of the moisture remaining in the sea-winds, and have, compared with the plains below, a moist soil, and the moisture which falls naturally descends along the surface. Farther east, beyond the Cascade Range of mountains, the dryness of the climate is more extreme, and the forests fail altogether except on the high mountains; and at a point on the Columbia river, about 250 miles from its mouth, there is the "last tree" on its banks—the region beyond being seemingly too dry even for the growth of the usual river-bank line of cottonwood.

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[3] Exploring Expedition Geological Report by the Author. (4to. 1849), p. 314.

[4] Same Report, pp. 339, 340.

[5] For details see same Report, and also this Journal, [2], vii, 387–394, 1849.

Thus the fact of a connection between the prevalence of moisture and forests, or of dryness and prairies, seemed to the writer most evident; and it appeared to be a causal connection.

To the southward, in the Sacramento region, and along the coast, a similar system of facts was, later, observed. The dryness of the climate, as is well known, increases southward; the average annual amount of rain along the coast of Oregon is 50 to 60 inches; at San Francisco, California, according to Gibbons, but 22 inches; and farther south on the coast, at the beginning of the California peninsula, but 10 inches. So the forest region of the sea-border narrows southward, failing on the lower lands before reaching San Francisco, and only to be found on the higher hills and mountains where cold and elevation secure moisture from the passing winds; and in the peninsula of Lower California, the mountains even are bare.

In the latitude of San Francisco, and farther south, the Sierra Nevada is generally bare below a line three to four thousand feet above the sea, except in the valleys; above this line there are nearly unbroken forests; and it is stated that 50 to 75 per cent more of rain falls on these mountains than on the Sacramento prairie plains at their western foot. The eastern slope of the Sierra is much drier than the western; and although forests extend nearly to the same line, they are less dense and consist of smaller trees.

The forests, as has been said, reach far down many of the valleys; for the stratification of the rocks leads often to an oozing out of subterranean waters; and sometimes these forests stretch out over the plains below, or form isolated patches in them. The Sacramento plains, which are a score or more of miles in width, are ordinarily covered with grass in scattered tufts, like those of the Willammet, and have in some parts similar "oak-openings." But in one spot, near Feather River, we observed ahead, with some surprise, a thickly-planted grove of oaks. The surprise was removed, on reaching the spot, by finding that the soil below was for some reason quite moist; sufficient to make the grass of the region green, although over the rest of the plain it was then (in November) as dry, and to all appearances as dead, as hay. As the moisture gradually diminished around the borders of the area, the trees were correspondingly more and more widely spaced, and the grass shaded off into the prevailing hay-color.

Other personal observations made abroad might here be mentioned. But we may pass to some well known facts nearer home which equally sustain the view that forests belong to moist climates, and prairies to those that are comparatively dry.

As America is the moist continent, and correspondingly the forest continent of the globe, as styled by Guyot, so North America itself graduates from forest to prairie land, or the reverse, in parallelism with the variations in moisture. In the United States forests originally prevailed with almost unbroken continuity over the parts which receive the greatest amount of rain with the least amount of evaporation, and prairies or semi-deserts where the amount is small. Over the eastern portion of the continent, from the Gulf states to Labrador, including the most of New England and New York, the Atlantic states, and all of Tennessee and Kentucky, the annual fall of rain is 40 to 45 inches or more; while in Michigan, the state which has afforded many of the facts appealed to by both Professor Winchell and Mr. Lesquereux, it is only 30 inches;[6] in a large part of Ohio, Indiana and Illinois, and in New York toward the St. Lawrence, it is 33 to 40; and beyond the Mississippi west of Kansas and Iowa, it is 25 to 15, and even 10 inches. Parallel with the increase of dryness, as all know, there is an increase in the dominance of prairie land; and the prairies, where the dryness is most extreme, graduate into semi-deserts. Such facts seem surely to prove that natural prairies have some kind of relation to dryness of climate, and that moisture has as much to do with the prevalence of forests.

Over the moister region of New York, and much of New England and other portions of the country, it is common to find not only the hills and upper alluvial plains (or upper terraces of the valleys) covered with forests, where they are still in a state of nature, but even the lower flats bordering the rivers. The writer spent his early life at Utica, N. Y., in the Mohawk valley; and facts sustaining this remark are well exhibited all along that region. Forests still cover some parts of the lower flats, although they have been mostly cut away for purposes of cultivation; and they continue north and south over the upper terraces and on over the high hills. The writer was through the valley the past summer, and observed forest patches on the lower flats (or, where the forests are gone, the old stumps,) at several places between Utica and Herkimer, over the Herkimer flats, between Herkimer and Little Falls, between Little Falls and Johnsville, and also east of Johnsville. These forest patches have generally been left where they occur because these places were too moist or swampy for cultivation, or required more draining than the value of the land seemed to warrant.

West of Utica, near Oriskany, there are large bogs which are so wet the year around that they are hardly penetrable unless when frozen over in winter; and still they are covered with forests.

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[6] See Blodget's Rain Chart, Army Meteorological Register. 4to, 1855.

The soil of these lower flats is often the finest of silt, such as naturally belongs to bogs and lakes; and varies from this on one side to peat, and on the other to sandy loam; and that of the upper terraces and hills varies from sandy loam to gravel and the coarsest and hardest of drift-material. Yet all these different kinds of soil are covered alike with forests. There are small lakes or ponds with their borders of rushes and sedge, swamps, peat bogs, and river bottoms, like those described by Mr. Lesquereux; but none of the swamps, peat bogs and river bottoms, with small exceptions, have become, or show a tendency to become prairies by natural change, however acid or unfavorable for trees the soil may be. The passage is in general from swamp to forest instead of from swamp to prairie.

Dr. Asa Gray, in a recent letter to the writer dated Sauquoit, N. Y., which is in the vicinity of the Mohawk Valley, observes that in the transformation of the bogs of that region (which in part have rush-bordered lakes at centre) to forest land, the tree that in his observation comes farthest out into the bog is uniformly the *Larch*, young trees growing on very shaky treacherous bog; but that, according to J. A. Paine, Jr., an able botanist of Utica, in the North Woods *Black Spruce* extends out as far as the larch. The next tree to the larch, and not far behind it, is the *Arbor-vitæ*, (Thuja occidentalis); the next *Black Ash;* the next *Red Maple*, (Acer rubrum); then, as it grows drier, *Birch;* then *Elm*, (Ulmus Americana). Alder, which runs out on the bog almost as far as the larch, is not here counted, it being regarded as a shrub. The "Cedar" (Arbor-vitæ) swamps, which are the remnants of the very extensive ones of Parishhill and Sangerfield, and from which the cedar has been cut out, and which are gradually drying, are already becoming filled with black ash, with a sprinkling of red maple, and especially of elm.

The great lake regions of Maine afford facts of similar import. I cite the following from observations made the last summer, at the suggestion of the writer, by Prof. A. E. Verrill, of Norway in Maine, (now of Yale College), and communicated by him (from Westport, N. Y., on Lake Champlain,) for this article:

> "The points which I had in view in my observations were the following: 1st, the succession of vegetation as a lake changes to a swamp, and then to dry land; 2nd, when swamps become permanently flooded what changes occur in the vegetation; 3d, when lakes or swamps are drained what effects are produced; 4th, whether natural meadows are ever produced in our climate, and under what circumstances will grass or trees flourish best.
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> There are in Maine abundant opportunities for studying almost every

change which can occur independent of climate in the vegetation of the lake margins, swamps, and meadows, both recent and ancient.

Near Norway I found a lake, one part of which has been gradually changing to a bog, and presenting all gradations from the open lake to a comparatively dry swamp. Approaching the bog, the water becomes shoal and the bottom is composed of black, soft vegetable mud of great depth, the surface of the mud supporting a variety of aquatic plants in patches. In still shoaler water clumps of sedges and rushes appear collecting around old stumps, driftwood, &c., and at the same point there are spots where various Ericaceous shrubs (such as Andromeda polifolia, Kalmia glauca, Cassandra calyculata, Ledum palustre, &c.). with the Alder (Alnus incana) begin to form dense patches of shrubbery bogland. Back of this these patches begin to blend, and other species are added to the shrubs, while the drift logs, &c., give support to an abundance of sphagnum and herbaceous plants, and now trees of Arbor-vitæ and Swamp-ash begin to find support, first taking root apparently on the old logs overgrown with vegetation and covered with mould, but the roots extend downward into the soft mud and water. Both these trees will grow where their roots, and even the bases of their trunks, are continually bathed in water. Farther back in the swamp these two trees become larger and more dense, and are mixed with fir, alder, larch, and an occasional white-pine and hemlock; the latter, however, only becomes common upon the drier borders of the swamp. The red maple also often grows in these swamps, in the firmer spots. Many other shrubs also occur, such as Rhodora Canadensis, Kalmia angustifolia, Vaccinum (several species), Myrica Gale, &c. The entire assemblage constitutes a dense forest with thick underbrush. Some of the Arbor-vitæ grow to a diameter of three feet, and the Ash to 15 or 18 inches, in the wettest parts.

The final condition of this kind of swamp may be seen in another place a few miles distant, where an extensive meadow has been formed by clearing a portion of such a swamp in a farther advanced stage, and partially draining it. This has been cleared about fifty years. It was formerly covered by a forest similar to that described, and some portions still remain in the original condition. Other parts that have been neglected have grown up to thickets of alder, white birch, American aspen, and other trees. The grassy portion covers 300 acres, or more, and in many parts is still quite wet and soft. The soil is composed of peat and muck which in many places has been penetrated to the depth of 6 or 8 feet, and is doubtless deeper in some parts. On some patches of low clayey soil, originally islands or low points of land, there are dense thickets of larch.

In the same region there is another extensive meadow surrounded by high, well defined banks of drift material, often terraced, and forming apparently the bottom of an ancient lake at the close of the Drift period. The lower parts of this basin are occupied by extensive swamps, similar to those already described, while other parts, composed of sandy soil and somewhat higher, were originally covered with a heavy forest of Pine. There is no evidence that grass ever grew upon any part of it until cleared by man.

I have observed some cases where lands have been flowed by means of dams at the outlets of lakes. In these cases, those trees, even of swamp-ash and arbor-vitæ, which have their trunks submerged to the depth of two or three feet are invariably killed after some time.

I have met with but two cases of lakes recently drained. In one of these, a heavy crop of grass was produced the first year, chiefly of "blue-joint," if I remember rightly, but in subsequent years the lake bottom grew up with great rapidity to a thicket of alder, willow, poplar, &c.

In the other case, coarse grass, sedges and other herbaceous plants prevailed for a year or two but very soon gave place to a rank growth of various forest trees, including alder, white birch, willow, poplar, red maple, &c.—the alder and willow growing most rapidly. I have no reason for supposing that the trees *did not vegetate the first year;* they became conspicuous as they grew higher than the grass and other herbaceous plants.

All of the phenomena observed in Maine also appear in this region.

On the coast of Labrador, and especially at Anticosti, very extensive peat-bogs occur under similar circumstances. On the southeastern side of Anticosti there is a bog bordering the coast for 70 or 80 miles, and often 7 or 8 miles wide, composed of peat of considerable thickness where exposed by the encroachment of the sea. In the midst of these bogs there are often small lakes or pools of stagnant water. The surface of the bogs or heaths is covered thickly by sphagnum and by a variety of Ericaceous shrubs. Vaccinum, (several species), Kalmia, Rhodora, Ledum, Rubus, Chamæmorus, &c., abound. Occasionally in the shallow pools and in wet depressions there are patches of sedges and coarse grasses; but dense clumps of alder, larch and other trees occur scattered over the surface, while immediately about the shores of the lakes there are usually dense thickets of these trees mingled with some spruce and white-birch (canoe birch). The last two trees form the chief part of the forests away from the bogs. In no case have I observed patches of grassy land occurring naturally of any considerable extent away from the immediate sea-shore. But the coast of Anticosti is everywhere fringed by a belt of coarse grass, (Elymus mollis, &c.,) mingled with various flowering plants. This belt sometimes widens into large patches at the mouths of streams.

The smaller low islands that are occasionally swept by the spray, usually support a dense growth of the same grasses and herbs, but the higher islands, as well as those of larger size, are covered by thickets of spruce, fir, larch and birch.

From these facts you can draw whatever conclusion you may think justifiable; but I think it evident that in our climate trees will invariably vegetate in the wettest soil and even displace grass where it has already become established."

The facts described by Prof. Verrill are so full and explicit in favor of the possibility of lakes, peat-lands and swamps, passing at times directly to forest land, that it is unnecessary to cite additional observations.

In this connection it is important to note some of the conditions essential to the self-propagation of trees. We mention a few well-known facts:

a. Elms, maples, and other shade trees drop their fruit profusely over a grass-covered lawn, and yet none take root. The grass prevents the seeds for the most part from reaching the soil; or if washed into it by rains, the pre-occupation by the grass hinders their development.

b. But along the sides of an earth-made path, and in shaded places, as near a fence or hedge, a crop of young plants will often spring up.

c. These young plants, as the writer has observed about his own grounds, die off during an ordinary summer's drought. Only those which spring up where the earth is kept moist by the shade and the position is one favorable for perpetuated moisture, and those which fall into the open and enriched soil of a garden, will stand the usual summer's drought and live.

d. The simple fact is that the roots of most trees are slender and rather sparingly branched, and the leaves also are few; and unless they have a chance to strike deep before a drought, down to depths where there is permanent moisture, and take considerable expansion at such depths, they are cut off by the dry season. Even if a peculiarly wet summer favors the seedlings so that they get a year's start, the drought of the following year is almost sure to cut them down.

In view of the facts we believe we are safe in deducing the following conclusions:

1. *A prevalence of moisture is connected directly with a prevalence of forests.*—This is exhibited in the distribution of forests over the surface of the continent; on mountains; down the valleys of mountains, where outcropping layers bring moisture to the surface; along the slopes dividing the alluvial terraces of many prairie regions, where moisture oozes out for the same reason—the stratification of the alluvium; over many prairie plains when they are unusually moist; and along the banks of rivers, which feel even the narrow fogs that may overlie the stream.

To avoid misapprehension of this statement it should be considered that this moist condition is not proportioned necessarily to the annual fall of rain and snow. For the mists which prevail throughout the year along some sea-coasts, and in some mountainous regions, may not drop half the amount of moisture in a year that descends through rain and snow in other regions, and still be vastly more favorable for forests. The mist keeps the ground always moist. The rains, on the contrary, may descend plentifully during half the year and give a large number of inches of fall, and yet be followed by a season of drought unfavorable to forests. Moreover, a large part of the waters of rivers flow off in the streams to distant regions; while the more gently distilling mists keep the ground thoroughly wet, and lose much less by running off. Hence, the regions of the greatest fall of moisture on a rain-chart do not necessarily correspond with those of the greatest extent of forests. The area of even "45 inches" over the United States is not necessarily, therefore, an area throughout of forests.

Again, the effects of the falls of moisture in the shape of rain mists, &c. depend much on the heat of the region, because of the consequent difference in evaporation. With a cool climate, like that of the coast of Oregon, there is comparatively little evaporation, and the moisture lies long in the soil. But the same moisture, from the same kind of mists, on a coast like that of Peru should produce much less effect. Forty inches of rain in a year about a mountainous region, or in a cool climate, would have far greater effect toward keeping the soil moist than in those low lands which lie in the same latitudes in the Mississippi valley. It has been stated that the amount of rain-fall in the Sierra Nevada is 50 to 75 p. c. more than in the Sacramento and Joachim plain on the east; but the effect of this moisture on vegetation is vastly greater than this on account of the cooler climate of the heights.

2. *In a moist region soils of all kinds not under water may become forest-covered, from the finest silt to the coarsest and loosest gravel, from the sourest marsh mud and drying peat swamp to the most fertile alluvium.*—This is shown through eastern North America, and has been illustrated by reference to facts in the States of New York and Maine.

Yet, where there is only a moderate supply of moisture, the different kinds of soil will manifest their differences in unlike productiveness in trees. The loose gravel is quick to feel droughts; the clayey soil much less so; the best vegetable mould still less so. The fact that trees will not grow over soils permanently submerged does not require us to admit that when the same region has become comparatively dry the soil is still unfit for trees. When, on the first recovery of the land, grass takes possession of the surface, forests, in moist regions, may soon spread and dispossess it.

3. *Grass-regions may encroach on forest-regions, or the reverse, according to the dryness or moistness of the country.*—Where the ground is permanently moist, the meadow seldom makes progress against the trees. Thus the line of forest which covers the slope lying between two terraces, so striking a feature in some prairie regions, holds its place notwithstanding the grass of the prairie plain above, and of that below it.

But if the old forest covers dry soil, the grass, or its precursors in other plants, may gradually make an undergrowth among the nearer trees, and thus commence encroachment. Droughts do not destroy the grass, while they do the seedlings of the trees; and when, under such conditions, the old trees die, they die without successors. The encroachment is the slow work of centuries, because the standing trees are not injured by such an undergrowth. They live out their life-time, if left to nature, and only when they die does the meadow-surface complete itself.

The forest, as has been explained, cannot ordinarily encroach on the prairie. But if a prairie, through any change of circumstances, becomes permanently wet, so decidedly so as to weaken the grass, but not a region of permanent water, then the forest has its chance for encroachment.

4. *If moistness, then, is especially favorable to the growth of forests, a change in the moistness of a region occasioned by geological events would be attended by a change in the adaptedness to such growth.*—The Champlain epoch of the Post-tertiary, when portions of the continent over the higher latitudes were much depressed, (in many parts 300 to 400 feet), and the more southern much less so, and when the great upper terrace flats of our lakes and rivers, often many miles in width, were made, was a time of warmer climate over the continents than the present, as the distribution of the terrestrial animal life of the era proves.[*]

It was, also, as the same terraces and the raised beaches prove, an era of wider expanse of lakes and rivers over the land. It was, therefore, in all probability, an era of moister climate over these regions than now. This being so, it was an era eminently favorable for the growth and extension of forests, when trees would have taken possession of the dry land not under water; that is, wherever it was *not too dry*, as even then was probably true of the far west. Now, after this Champlain epoch of depression, an elevation of the land began, bringing the continent up toward its present level, causing a change of climate to one of greater coolness and dryness, draining extensive regions that had been under water, and drying moist areas. Consequently, there would have been, from the beginning of this change, a tendency to a narrowing of the forest regions; and, with such a tendency, an actual narrowing would, in one region or another, have begun.

As the various parts of the continent would have differed hygrometrically during the Champlain epoch in the same way as now, the great dense forests of the continent, on this side of the Rocky Mountains, would have then, as now, covered the eastern portions, and the great treeless regions would have been beyond the Mississippi. Any subsequent extension of the *prairies* that accompanied the approach to the present era and condition would, therefore, have been from the west toward the east. The prairies would have gradually stretched their bare surfaces along the rivers, and made their winding way eastward among forest-clad hills, wherever the dryness of the soil most favored them. There would thus have been a slow yielding of the soil to plants that could hold their places in spite of droughts.

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[*] See the author's Manual of Geology, pages 547 to 567.

At the same time the new-made lands about drying bogs, lake shores, etc. would have taken their vegetation from the adjoining regions; the kind being determined by the dryness or moistness of the region through the year; by the nature of the vegetation that was most accessible with its seeds; by the character of the soil; or, if either the dryness of the climate, or its moistness, were great, very largely by this one element alone, the dryness determining the absence of trees in spite of all other conditions, and the moistness their presence. Accordingly, there are interminable forests in Eastern America, interminable prairies on the slopes of the Rocky Mountains, and in the region between an intermingling of forests and prairies.

The species of the Post-tertiary trees, as may be concluded from the observations on buried fragments, were probably identical for the most part, if not wholly, with those now existing, (as is true also of the species of invertebrate animal life). Our forests of modern trees date, beyond reasonable doubt, from this remote Post-tertiary era. Their distribution began then, and in accordance with the favoring conditions of the soil and climate. At the present time, we have the distribution of that era as it has become after being modified by the effects of subsequent changes of level and of climate.

But the modifications from these causes are, even now, far from being completed. As is well known, there are extensive forest regions where the soil and climate are so dry that if the trees were cut away they would not be able to re-establish themselves. A conformity between the distribution of trees and the climate is, consequently, only partially accomplished. Any cause that destroys the trees of a region, be it tempest, fire, or other agency, helps on this progress.

The prairies of the Willammet, and in part those of the Sacramento, and also many east of the Rocky Mountains, have now their "oak-openings," or surfaces scattered over with oaks, 15 to 20 or so, to an acre. But these trees may once have been in thick groves, as they are now clustered near Feather River, in the Sacramento region. (p. 296.)

A continent, or a large portion of it, cannot pass from a condition of greater to one of less moisture without ultimately exhibiting it in the distribution of its forests and prairies.

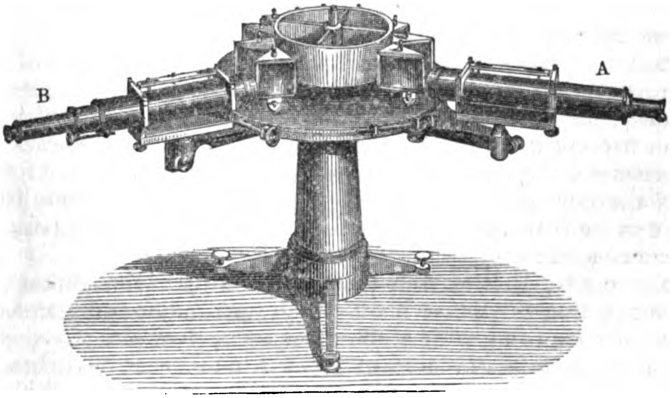
It is, therefore, no objection to the theory here propounded, that forests, when cut down, do not, if left to nature, always resume their places, but give way to meadows. The times since the original planting are changed, and meadows are on the increase from natural causes, as well as through the agency of man.

ART. XXXI.—*On the Construction of a Spectroscope with a number of prisms, by which the angle of minimum deviation for any ray may be accurately measured and its position in the solar spectrum determined;* by JOSIAH P. COOKE, Jr.

IN an extract from a letter of the author published in this Journal, vol. xxxvi, p. 266, a method of adjusting the prisms of a compound spectroscope was described, by which the adjustment for any portion of the spectrum could be obtained with great rapidity and accuracy. A further study of the subject has shown that the method of adjustment then only briefly described admits of the highest precision, and may be applied to the exact measurement of the angle of minimum deviation of the spectrum rays. It has been thus possible to apply the great dispersive power of the large Cambridge spectroscope in determining the relative position of the various spectrum lines, and to secure all the accuracy of which angular measurements are capable. The value of such measurements is obvious, and with the hope that this method will prove to be an assistance to investigators we propose to give in this paper a description of our instrument and of the manner of using it.

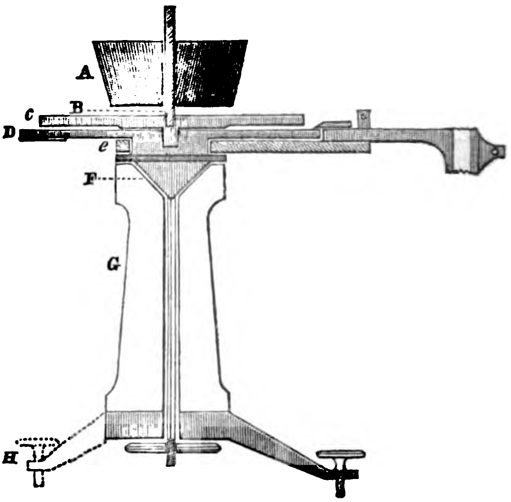
The general construction of the instrument is shown in fig. 1.

1.



The two telescopes are constructed in the usual way. The telescope A, which we shall call the collimator, has an adjustable slit placed exactly at the focus of the object glass. The small tube which carries the slit slides into the body of the telescope, but a rack and pinion motion would be preferable, so that when the focus is changed the slit will necessarily remain vertical. The rays of light diverging from the slit and rendered parallel by the object glass of the collimator next pass through a series of prisms adjusted around a conical wheel, which will be soon described, and are then received by the telescope B. The spectrum, which is formed at the focus of the object glass of this telescope, is examined with eye-pieces of different magnifying powers in the usual way. The object glasses of the telescopes are 2$\frac{1}{4}$ inches in diameter and have a focal length of 15$\frac{1}{8}$ inches. They rest in Y's and are provided with spirit levels and adjusting screws. The frames which hold the telescopes are supported on pivots turning in sockets at the ends of two arms connected with the body of the instrument and may be clamped in any position. The arm which carries the collimator is permanently attached to the main iron plate, but the arm, which carries the observing telescope may be moved around the plate.

2.

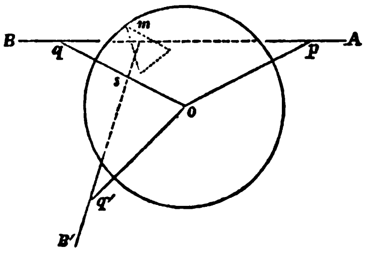


The details of the construction are shown in fig. 2, which is a section made through one of the legs of the tripod and the movable arm, the telescope with its frame and pivot having first been removed from the socket. (This figure as well as fig. 4 were drawn to the scale of one inch to a foot). The parts are as follows: H is an iron tripod with leveling screws; G is a hollow mahogany column with a conical cavity at the top; F is an iron cone which rests in the conical cavity, supporting the whole body of the instrument and connected by a long iron rod with a clamping screw beneath the tripod. By means of this arrangement the instrument may be turned as a whole in the horizontal plane and the collimator directed to the source of light. Above the iron cone and fastened to it securely is the main circular plate of the instrument, which is 18 inches in diameter and $\frac{1}{2}$ an inch in thickness. Near the outer edge of this plate is inserted a band of silver, which is graduated to 10″ of arc. On the under part of the plate there is a neck and at the center of the upper surface a socket, which are accurately centered with each other and with the graduated limb. Around the neck at E moves an iron collar, to which is attached the arm bearing the observing telescope. This moves, therefore, concentric with the graduated limb and bears a vernier by which the angular motion may be determined, reading to 10″. In the socket of the first plate rests the pivot of a second plate, C, which turns on the first and supports the prisms with the adjusting wheel A. The diameter of the upper plate is an inch less than that of the lower plate, so as to expose the graduated arc near the outer edge of the latter, and its upper surface is as flat and even as possible. Rising from the center of the upper plate there is a tall screw pivot of iron, B, on which turns a conical wheel, made also of iron. By this motion the wheel may be either raised or lowered. This wheel is an essential portion of the instrument, and we will next consider the theory of its use.

In the ordinary method of measuring the angle of minimum deviation with a Babinet's goniometer the prism is placed on a revolving plate at the center of the graduated circle, and so adjusted that the edge of the refracting angle shall be perpendicular to the plane of the circle, and its bisectrix parallel with a diameter of the circle. The axis of the collimator and observing telescopes, moreover, being parallel with a diameter of the circle, it is evident that, when the prism and telescope are turned into the position of minimum deviation, the vertex of the angle of minimum deviation will coincide with the center of the circle, and hence the arc intercepted between two radii of the circle parallel to the axes of the two telescopes will be the measure of the angle required. This angle is practically measured by first bringing into line of collimation the observing telescope and collimator, so that the image of the slit at the end of the collimator coincides with the vertical wire in the eye-piece of the telescope. The position of the vernier on the graduated arc is now noted. Then, having adjusted the prism, both the prism and the telescope are turned to the position of minimum deviation, and a coincidence established between the vertical wire and one of the lines of the spectrum. The vernier is now again read and the difference between the two readings is the angle of minimum deviation for the ray corresponding to that line.

It will be obvious however from fig. 3, that it is not necessary for the accuracy of this measure either that the prism should be placed at the center of the circle, or that the axes of the telescope should be parallel to one of its radii. If only the bisectrix of the refracting angle passes through the center of the circle, the prism

3.



may be placed on the outer rim of the plate, and if only the radial arm, which carries the observing telescope, moves concentric with the graduated arc, the axis of the telescopes themselves may make any angle with the radius whatever. Let O*p* and O*q* be two radii of the graduated circle. Let A*p* and B*q* represent the axes of the two telescopes in collimation and making any undetermined angles with the two radii. Place now a prism at *m* and turn the radial arm O*q* into the position O*q*′, but without changing the inclination of the axis of the telescope to the arm, and let B*m*B′ be the angle of minimum deviation. Since now the two triangles *qsm* and *q′so* are similar, it is evident that the angle B*m*B′ is equal to the angle *qoq*′, and is therefore measured by the arc intercepted between the radii O*q* and O*q*′.

In order now to apply this principle in the spectroscope the glass prisms were mounted permanently in brass frames. The frames rest on three brass pins which were adjusted by filing until the refracting edge of the prism was perpendicular to the iron place C fig. 2. Two brass pins were also attached to the back of each prism and the lengths of these so adjusted that, when the prisms are pushed against the conical wheel, the bisectrix of the refracting angle should coincide with a radius of the wheel. The last adjustment was made with the aid of a guage cut from a sheet of tinned iron fitting at the same time the periphery of the wheel and the face of the prism, which was applied alternately on either side. The angle of minimum deviation of the ray D was then measured for each prism in the following way:—

The prism having been placed on the plate with the pins applied to the periphery of the wheel, the collimator was turned on its pivot, and at the same time the plate C turned on its center, until on applying the eye at the open slit and looking through the object-glass towards the prism, the further back edge of the prism, seen through the glass of the prism, appeared to coincide with the nearer back edge seen directly. When this is the case, it is evident that the rays of light which reach the eye from the further back edge of the prism must pass through the glass parallel to the back edge of the prism, or, what amounts to the same thing, perpendicular to the bisectrix of the refracting angle, and when the light passes in this way the prism is at the angle of minimum deviation. When the prism was thus placed the collimator was turned slightly on its pivot until the axis of the telescope prolonged passed through the center of the prism-face, and was then securely clamped in this position. This adjustment may be made with sufficient accuracy by the unassisted eye. The prism having now been turned one side, the arm of the observing telescope was turned on its center, and at the same time the telescope turned on its pivot until it came into exact collimation with the collimator. In order to facilitate this adjustment the telescopes are provided with caps which cover the object-glasses of the telescopes with the exception of a narrow vertical slit at the center. The pivot of the observing telescope was next clamped and the caps having been removed the image of the slit seen through the observing telescope was brought into exact coincidence with the vertical wire and the position of the vernier noted. The prism was now brought back to its place by turning the upper plate of the instrument, and the observing telescope also turned until the position of minimum deviation for the ray D was attained and this well known double line brought to coincide with the vertical wire. The limb was then again read, and the difference of the two readings gave the angle of minimum deviation for the prism.

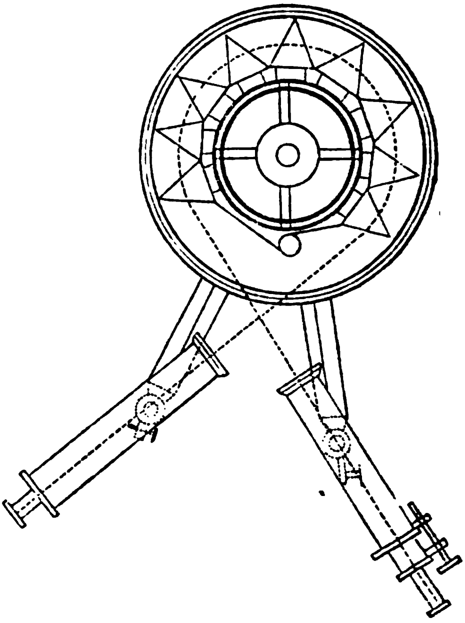
In order to show that this method of measurement is perfectly accurate, we give below the angles of minimum deviation of the nine prisms of the Cambridge spectroscope measured as just described, and in a parallel column the same angles measured in the old way with the prisms at the center of the plate. It will be seen that the differences are insignificant and within the limits of error of observation:

	Measured at center of plate.	Measured on edge of plate.	
No. 1,	29° 31′ 10″	29° 31′ 10″	
No. 2,	29° 29′ 10″	29° 29′ 10″	
No. 3;	29° 28′ 10″	29° 28′ 10″	
No. 4,	29° 37′ 0″	29° 36′ 40″	−20″
No. 5,	29° 28′ 30″	29° 28′ 40″	+10″
No. 6,	29° 36′ 30″	29° 36′ 10″	−20″
No. 7,	29° 28′ 10″	29° 28′ 10″	
No. 8,	29° 29′ 30″	29° 29′ 40″	+10″
No. 9,	29° 28′ 40″	29° 29′ 40″	
	267° 37′ 50″	267° 37′ 30″	−20″

Although the adjustments required may appear complicated, they can be made in far less time than it has taken to describe the method.

It is a well known fact that when a beam of homogeneous light passes through a prism at the angle of minimum deviation the incident and emerging pencils make the same angle with the faces of the prism, at which they respectfully enter and leave the glass. Hence a second prism like the first stands in the same relation to the emerging beam in which the first stands to the incident beam. If then, after the first prism has been adjusted at the angle of minimum deviation a second prism be applied against the wheel at the side of the first, by moving the prism slightly to one side or the other, it will be easy to find a position, in which this prism also is at the angle of minimum deviation, moving of course the prism on the plate without disturbing the position of the plate itself. In like manner other prisms may be added until the required number is obtained. (In the Cambridge spectroscope there are nine glass prisms of 45°, as shown in fig. 4.) This adjustment has only to be made, however, once for all, since when the prisms are adjusted they are fastened to a very thin, flexible brass ribbon, which, passing through a box at the back of each prism, is there secured by clamping screws. The ends of this ribbon, moreover, are attached to a small brass drum, which, being wound up by an ordinary clock spring, draws the chain of prisms tightly around the conical wheel, and keeps them always in place. By tracing the path of a ray of homogeneous light through a series of similar prisms, as is shown in fig. 4, it will be found that the path of the ray within the prisms is always tangent to the same circle, provided that it passes through all under the conditions of least deviation. Assuming, then, that the distances between the prisms are invariable, as they must be when the prisms are fastened to a brass ribbon as just described, it will be evident from a moment's reflection that the greater the refrangibility of the given ray the less must be the diameter of the circle, around which the prisms should be arranged in order that the ray may pass under the required conditions, and, knowing the dimensions of the prisms as well as the index of refraction and dispersive power of the glass, it is easy to calculate approximatively what the diameter should be in a given case. The dimensions of the conical wheel A, fig. 2, were thus determined—the largest diameter, $9\frac{1}{2}$ inches, corresponding to the extreme red, and the smallest diameter, $8\frac{1}{2}$ inches corresponding to the extreme violet rays of the solar spectrum. In order to facilitate the adjustment a series of lines were engraved encir-

Fig. 4.



cling the wheel at equal distances from each other and numbered from 1 to 12.

Having described the construction of the instrument it will now be easy to understand the method of using it. Let us suppose that the object is to measure the angle of minimum deviation of the blue ray of the strontium spectrum. By examining any chart of the spectra of the chemical elements it will be found that this line is situated roughly at somewhat less than two-thirds of the distance from A to H. If, then, we turn the conical wheel until the pins of the prisms all rest against the line marked 7, we shall have approximatively the true position. We then adjust the collimator with reference to the first prism exactly as before described for a single prism. Turning then the upper plate so as to remove the prisms out of range, we bring the observing telescope into collimation with the collimator, as also before described, when on reading off the limb we have the starting point for our measure. We next turn the plate and move the telescope until the spectrum appears in the field, and carefully bring the blue line to coincide with the vertical wire at the position of minimum deviation. We now raise or lower the conical wheel and notice if in this way the angular deviation is diminished, and leave the wheel in the position where the minimum is reached. It now only remains to again read the limb when the difference of the two readings subtracted from 360° will give the angular deviation required.

When near the position of minimum deviation a large motion of the conical wheel produces only a slight motion of the image, so that after a table has been made giving the position of the wheel for a few of the marked lines of the spectrum it is always possible to bring the wheel at starting to the desired point. Moreover, the fact that when near the position of minimum deviation the position of the image is affected so slightly by a small change in the position of the prisms, renders it possible to make all the adjustments required with exceeding rapidity and accuracy.

In order to test the accuracy of our method we have made several determinations of the minimum deviation of the line D, and although between each determination the whole apparatus was completely dismounted, the value obtained was in all cases 267° 37′ 50″.[1] It will be remembered that the sum of the angles measured on each prism separately at the center of the plate, as given on page 309, is precisely the same (267° 37′ 50″) and the sum of those measured on the edge of the plate (267° 37′ 30″) only differs from this by 20″.

When it is not important to use absolutely the whole aperture of the prisms it is not necessary to change the position of the collimator in passing from one part of the spectrum to another. If we adjust the collimator as above described, when the prisms rest against the middle circle on the wheel, the whole spectrum can be passed under review with great rapidity without any further change of the collimator, and each point seen under the condition of minimum deviation. When, however, on account of the feebleness of the light it is important to use the whole aperture of the prisms, a slight gain can be obtained by readjusting the collimator at the extreme points of the spectrum. In making the measurements described in this paper, an important advantage is gained in keeping the position of the collimator fixed; for if its position is changed, the point to which the angular measurements are referred is changed also, and must be determined anew. If however the arms, to which the telescopes are attached, are so arranged with a sliding motion that both the collimator and the observing telescope may be moved parallel to themselves without altering their relative angular position, the whole aperture of the prisms may at any time be used, and nevertheless all the measurements referred to the same point on the graduated limb.

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[1] As the mean of the two lines.

Besides the set of glass prisms the Cambridge spectroscope is also provided with a set of sulphid of carbon prisms, which have been previously described in this Journal. They are mounted on a separate plate with a separate wheel of the proper dimensions and are arranged in all respects like the glass prisms above described. The plates are provided with handles so that one battery of prisms may be quickly lifted off from the instrument and the other put in its place. But although the liquid prisms are valuable on account of their great dispersive power in bringing out faint lines, especially in the more luminous portions of the spectrum, yet their use is very restricted. One difficulty arises from the immensely rapid change of the index of refraction of sulphid of carbon with the slightest change of temperature. We have noticed within half an hour on a summer's day a change of. 8′ in the angle of minimum deviation of a single sulphid of carbon prism of 45°, although the temperature of the room had only in the meantime changed eight tenths of a Centigrade degree. The temperature of the prism undoubtedly changed much more than this; but when we remember that the variation thus produced would amount to over one degree for the nine prisms, it will be seen that they must be useless for any purposes of direct measurement. Again, sulphid of carbon is far less transparent than glass to the more refrangible rays of the spectrum, and lastly, a slight change of temperature in the observing chamber produces at once currents in the liquid, which are fatal to good definition. Nevertheless under the best conditions, we have found that sulphid of carbon prisms define as well or even better than glass. The instrument described in this paper, with the exception of the glass prisms, was made by Messrs. Clark & Sons of Cambridgeport, and we would here especially express our indebtedness to Mr. George Clark for his great ingenuity in planning and executing the mechanical details. We reserve for another article an account of the results of our measurements.

ART. XXXII.—*Experiments in Mechanical Polarity;* by PLINY EARLE CHASE, M.A., S.P.A.S.[1]

I HAVE already published three simple experiments in mechanical polarization (Proc. Amer. Philos. Soc., ix, 359), and I have endeavored in various ways to show that terrestrial magnetism is probably owing to an analogous polarization of aerial and aetherial currents. Finding that my hypothesis was sustained by some remarkable numerical coincidences, as well as by various points of correspondence between the assumed influence of gravitation-currents and the observed daily variations of declination and inclination, I sought for some practical demonstration of the truth of my inferences. I therefore endeavored to imitate the movements of the atmosphere by mechanical contrivances, and the results, which are in most respects such as I anticipated, and which are all susceptible of an easy explanation, are embodied in the following additional experiments.

4. By the use of fans, bellows and blowers of various kinds, either alone or in conjunction with directing discs, currents may be produced that will deflect the needle in any desired direction, in accordance with simple and evident mechanical laws.

5. I have tried iron, copper, wood, zinc, and pasteboard, and find, as I anticipated, that the material employed for producing or directing the artificial currents has no effect upon their mere *mechanical* action, but I have found the results most satisfactory when, in order to avoid the complication of induced magnetism or electricity, I employed non-conductors, such as wood and pasteboard. Upon subsequently repeating the experiments with different metals, the effects of the induced currents have been plainly shown.

6. Increasing the number of directing discs (provided they are all parallel), often modifies the intensity of current-influence, but does not appear otherwise to affect the result.

The most striking developments that I have yet hit upon, were obtained by the aid of a Gaussian modification of Faraday's apparatus for showing the electric currents developed in moving metallic bodies by terrestrial magnetism. It consists of a heavy copper ring (*a*), fixed to horizontal supports (*b*) (*c*), on which it can be rapidly rotated by a wheel (*d*) and pinion (*e*), the supports being relieved by friction-wheels (*f*). The outer diameter of the ring is 6 inches at each edge, and $6\frac{3}{16}$ inches in the middle; the inner diameter 5 inches; the thickness 1½ inches. The axle opposite to the gearing (*g*), is hollow, to allow the insertion of a brass rod (*h*), which supports a compass (*i*) horizontally in the interior of the ring. The compass is not shielded by a glass, and it is therefore easily affected by aerial currents. If the ring is made to revolve around the compass needle, the N. pole of the needle is deflected in a direction opposite to the motion of the top of the ring.

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[1] **From the Proceedings of the American Philosophical Society, July 21, 1865.**

For my special purpose, I replaced this copper ring by a wooden one of the same dimensions, and prepared a number of discs (*k*) (*l*), in the form of semicircles and circular segments, fitted so that they could be fixed, in different vertical positions, in the compass box, above the needle. Causing the wheel to rotate, with the axis variously placed, I tried the following experiments:—

A. Single disc, or multiple and parallel discs.

a. Axis in magnetic meridian.

7. When the disc was in the meridian, the currents produced a slight tendency in the needle[1] to move in the same direction as the upper part of the ring.

8. When the disc was in the equator, the slight tendency was opposite to that in Exp. 7, and the same as if it had been produced by induced electricity in the ring.

9. When the disc was inclined to the meridian, there was a marked polarity, perpendicular to the disc.

b. Axis in magnetic equator.

10. The disc being placed in the meridian, there was no disturbing current.

11. Placing the disc equatorially, the needle was still undisturbed.

12. But when the disc was inclined to the meridian, the current-polarity was parallel to the disc.

c. Axis 45° from magnetic meridian.

13. In nearly all positions of the disc, there appears to be a disposition in the needle to move *from* the axis of the ring. But when the disc is in or very near the meridian, there is a slight tendency towards the axis.

In order to imitate more closely the earth's principal thermal radiation-planes, which are all theoretically parallel with the

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[1] In speaking of the needle's tendency, I always refer to the motion of the N. pole.

thermal meridians and meet at the thermal pole, I constructed a compound disc (*m*), of two circular segments, inclined to each other at an angle of about 30°, with which I tried the following experiments:—

B. Spherical-angular disc.

a. Axis in magnetic meridian.

14. If the disc is so placed, with its opening towards the south, that its angle is bisected by the meridian, the current-polarity carries the needle in the same direction as the motion of the top of the ring.

15. If the opening of the disc is turned to the north, the apparatus remaining in other respects as in the last experiment, the current-polarity is reversed (as in Exp. 8).

16. Placing the disc equatorially, I am unable to discover any current-polarity.

17. Inclining the disc towards N.W. and S.E., the needle moves towards the east, whether the opening of the disc is towards the north or towards the south.

18. If the inclination is towards N.E. and S.W., the motion of the needle is westerly.

Experiments 14 to 18 are perhaps the most interesting and important of the entire series. Exp. 14 represents the direction of the gravitation currents that tend to restore the equilibrium which is continually disturbed by the thermal radiation of the northern hemisphere, while Exp. 15 represents the like direction in the southern hemisphere, and all the experiments demonstrate that *in the daily magnetic fluctuations, the motion of that end of the needle which is nearest to the equator should follow, precisely as observation shows that it does follow, the direction of the sun's meridional influence.*

b. Axis in magnetic equator.

19. When the disc is in the meridian, the current-polarity and the magnetic polarity coincide.

20. If the disc is in the equator, with the opening to the east, when the top of the ring moves towards the north the needle declines to the east.

21. But when the motion of the ring is towards the south, the current polarity is reversed, and the needle declines to the west.

22. The reversal of the disc, placing the opening to the west, likewise reverses the polarity, the declination being east when the ring moves south, and west when the ring moves north.

23. The disc being inclined to N.W. and S.E., the needle declines towards the west (the action in Exp. 17 being reversed).

24. Changing the inclination of the disc to N.E. and S.W., the declination changes to east (reverse of Exp. 18).

From Exp. 19 to 24 we may infer that *in consequence of the action of the trade winds, combined with the greater radiation of the* *northern hemisphere, there should be a constant eastward tendency of the magnetic declination, such as is indicated by the secular variation of the needle.*

c. Axis 45° from magnetic meridian.

25. When the disc is in or near the meridian, there is a slight tendency in the extremity of the needle which is under the opening of the disc, to follow the direction of the top of the ring. In all other positions the needle declines *from* the axis.

d. Axis variously inclined.

26. If the inclination of the axis to the meridian is less, or greater, than 45°, the results approximate respectively to those obtained when the axis is in the meridian, and when it is in the equator.

ART. XXXIII.—*Nitrate of Iron;* by JOHN M. ORDWAY.

OF the scattered accounts to be met with respecting the action of nitric acid on iron, few or none are worthy of entire confidence, because all are defective in the statement of conditions; and failing to make due reservations, they commonly set forth partial facts for general truths. In this way Thenard long ago fell short of correctness in each and every assertion when he said,[1] "Ou ne peut obtenir que du nitrate de fer vert et du nitrate de fer rouge; et encore le premier ne se forme-t-il qu'en prenant de l'acide à 5°, et le second, qu'en employant de l'acide au plus à 12° à 15°. Si l'acide étoit plus concentrée, une portion de l'oxide rouge se precipiteroit, et l'on n'en retrouveroit plus en dissolution que trés peu s'il avoit 36° à 40°." And some later observers have been little more successful in generalizing from insufficient data. It is singular that chemical writers in speaking of the varied reactions have hardly alluded to any other cause of the great diversities observed except difference in the strength of the acid used. Davy does indeed go so far as to make a rather indefinite mention of temperature, and describes the phenomenon thus:[2] "When very dilute nitric acid, that is, such as of specific gravity 1·16,[3] is made to oxydate iron without the assistance of heat, the solution gives out no gas for some time, and becomes dark olive-brown."[4] "When very dilute nitric acids are made to act upon iron, by the assistance of heat, nitrous oxyd is produced in considerable quantity mingled with nitrous gas and nitrogen." "In the beginning of a dissolution the nitrous gas generally predominates, in the middle nitrous oxyd, and at the end nitrogen."

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[1] Annales de Chimie, lvi, 75.

[2] Collected Works of Sir H. Davy, iii, 112.

[3] The 1·16 must be a misprint, for an acid of that strength can hardly be called "very dilute," and in fact nitric acid of sp. gr. 1·16,—except at very low temperatures,—acts rapidly on iron of any kind, and evolves gas abundantly.

[4] Collected Works, iii, 130.

It should be remembered that the terms *iron* and *nitric acid* are too generic to allow qualifiers and definers to be altogether dispensed with. It must also be borne in mind that a finely divided metal does not always behave exactly like compact lumps of the same; and that heating or cooling a few degrees sometimes suffices to change the order of affinities of substances in mutual contact. A sound philosophy would therefore at the outset suggest the inquiry whether the action of nitric acid on iron is not a function of several variables, such as the quality, the quantity, and the strength of the acid; the form, the kind, the state, and the amount of iron, and the quantity put in at a time; the initial temperature, and the range of temperature allowed during the action, pressure, light, rest or agitation, and the access or exclusion of air.

Having only the data afforded by a few hundred experiments, I cannot presume to enter into a systematic and thorough discussion of all possible causes of variation; but the narration of some of the principal trials will serve to show that most of these supposed influences are really operative.

Wrought iron turnings and weak acid were found sometimes to give off hydrogen copiously and produce a green protonitrate, while in other instances nitric oxyd was evolved and a red pernitrate was formed.

For some comparable trials, turnings were secured clean and all alike by catching them as they fell from the lathe in turning a shaft of common rolled iron. The acid used was a nearly colorless commercial nitric acid about free from chlorine and sulphuric acid. The following are the final approximating terms of convergent series of experiments made by varying the temperatures, all other things being equal:

1, *N.*—At 45° C., 100 g. of nitric acid of sp. gr. 1·03, with 5 g. of iron added all at once, gave off hydrogen continuously. The temperature during the action rose to 49°.

n.—At 46° C., the same materials gave hydrogen for a few moments and then, changing, extricated nitric oxyd to the end. The temperature rose to 50°.

2, *N.*—At 35° C., 100 g. of nitric acid of sp. gr. 1·04, and 5 g. of iron yielded hydrogen only and made a protonitrate. The heat rose to 45½°.

n.—At 36° C., the same quantities of the same materials gave nitric oxyd and produced a dark red solution.

3, *N.*—At 31° C., 100 g. of acid of sp. gr. 1·05 and 6 g. of iron gave off hydrogen only. The thermometer inserted rose to 47½°.

n.—At 32° C., 100 g. of acid 1·05 and 6 g. of turnings evolved nitric oxyd and heated to 47° C.

4, *N.*—At 16° C., 5 0g. of acid of sp. gr. 1·06 with 3 g. of iron evolved hydrogen throughout the action, though the temperature rose to 27°.

n.—At 17° C., 50 g. of acid and 3 g. of turnings gave off hydrogen for a moment, then stopped entirely, and then went on and evolved nitric oxyd to the end.

5, *N.*—Taking another kind of iron turnings, at 29° C. 60 g. of nitric acid of sp. gr. 1·04 and 3 g. of iron gave hydrogen and a protonitrate.

n.—At 30° C., the same quantities of the same materials gave hydrogen for a while, and then changing evolved nitric oxyd and made a pernitrate.

It is worthy of notice in these cases that the final action is determined by the initial temperature, and not by that which may be reached during the operation. We see too that when the temperature alone is varied, the dividing limit between entirely different reactions is pretty sharply defined; but with unlike qualities of iron, the limit is by no means the same for the same strength of acid. Indeed, in trying some sorts of the metal, there has been no production of hydrogen observed under any circumstances. And it may be that the failure of others to notice such a phenomenon is owing mainly to their chancing to operate with iron of the purer kinds. Priestley[*] reports having once obtained "explosive air" by warming iron with nitrate of iron; and this solitary instance appears to be the only one on record.

The essential peculiarity of hydrogen evolving iron remained a mystery till I noticed that the gas blackened particles of rust floating in the water over which it was caught. The gas was found to be capable of discoloring paper moistened with a lead solution. So a small fractional per-centage of sulphur is the chief, if not the only, constituent of iron which suffices to make the acid depart from its more usual modes of operation; and probably it is the proportion of sulphur which determines for acid of a given strength the dividing limit of temperature between hydrogen and nitric oxyd production. I have seen a temporary evolution of hydrogen even with an acid of sp. gr. 1·08, and it is possible that a still stronger acid might operate in the same way on some irons, at a very low temperature.

The following trials show differences caused by what is conveniently designated as "mass;" but how far "mass" depends on mere weight and how far on the extent of surface exposed to action has not been ascertained.

6, *a.*—At 35° C., 100 parts of nitric acid of sp. gr. 1·04, with 5 parts of wrought iron turnings, gave off hydrogen only.

b.—At 35° C., 100 parts of nitric acid of sp. gr. 1·04, with two parts of iron turnings, gave hydrogen at first and then evolved nitric oxyd and made a red solution.

7, *a.*—At 23° C., 100 parts of nitric acid of sp. gr. 1·04, with 28 parts of cast iron, gave hydrogen only and made a protonitrate.

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[*] Experiments and Observations on Different Kinds of Air; London, 1779, iii, 134.

b.—At 23° C., 100 parts of nitric acid of sp. gr. 1·04, with 4 parts of the same cast iron, evolved nitric oxyd and yielded a pernitrate.

8, *a.*—100 parts of nitric acid of sp. gr. 1·05, with 50 parts of cast iron, emitted hydrogen only.

b.—100 parts of nitric acid of sp. gr. 1·05, with ten parts of the same iron, extricated hydrogen at first and then evolved nitric oxyd and produced a pernitrate.

Minute proportions of sulphur or phosphorus are known to have a wonderful effect on the physical properties of iron, and now the hydrogen production shows that one substance at least possesses a not less remarkable power of altering the normal chemical reactions; for when we take the purest iron to be had, such for instance as that used for fine wire, the alternative of nitric oxyd evolution is silent solution. When no gas is set free the product may be either protonitrate, or pernitrate, or an uncertain mixture of the two; and after long continued action, protoxyd of iron or peroxyd may be precipitated. Oftentimes, when air is freely admitted there is not only no loss but an actual gain in weight. In such cases probably nitric oxyd in fact forms slowly, but rising no farther than the surface of the liquid it is regenerated and fixed by atmospheric oxygen. There is always a production of nitrate of ammonia, but I have invariably found the quantity much too small to account for all the oxydation that takes place.

For trials of the purer irons, coarse wire was taken, of such size that one meter weighed 15·6 g. A fine wire was used weighing 0·54 g. per meter, and therefore for the same weight presenting 5⅓ times as much surface as the course.

9, *a.*—At 0° C., 100 g. of nitric acid, of sp. gr. 1·05, with 5·2 g. of coarse wire, gave off very little gas and made a protonitrate containing 0·26 p. c. of ammonia.

b.—At 22° C., 105 g. of nitric acid of sp. gr. 105, with 5·2 g. of coarse wire, gave off nitric oxyd, and made a solution of basic pernitrate which contained 0·12 p. c. of ammonia.

10.—At 0° C., 54 g. of perfectly pure nitric acid of sp. gr. 1·05 with 6 g. of fine wire, lost 0·080 g. and made a protonitrate and some green oxyd. The product contained 0·2 p. c. of ammonia.

11.—At 0° C., 68 g. of nitric acid of sp. gr. 1·06 and 3·5 g. of fine wire lost only 0·090 g. Somewhat more than half of the iron dissolved was in the state of protonitrate. There were in the product 0·09 p. c. of ammonia.

12.—At 0° C., 97 g. of nitric acid of sp. gr. 1·10 and 10 g. of fine wire lost 0·72 g. The product contained 0·26 p. c. of ammonia and 6·7 p. c. of iron, of which about two-thirds were in the form of protonitrate.

Thus, by operating on the purer sorts of iron at low temperatures, we may get an unmixed protonitrate, with acid of as high a specific gravity as 1·05. And acid of much greater strength gives some protonitrate, if the action is retarded by cold.

13.—71 g. of nitric acid of sp. gr. 1·25 being cooled in snow, 10 g. of the fine wire were gradually added. 8 g. of the iron were dissolved and the loss was only 1·23 g. Were the metal in this case oxydized by the mere decomposition of the acid, 4 g. of nitric oxyd should have been given off. The solution did indeed contain some ammonia, but its amount was too trifling to account for any considerable part of the oxydation.

The nitric oxyd, therefore, instead of passing off as fast as it was liberated, must have been in part oxydized by the superincumbent air and absorbed back into the liquor. The following instance gives a more forcible illustration of this matter:

14.—7 g. of fine wire were gradually dropped into 97 g. of nitric acid of sp. gr. 1·20. The iron was all taken up leaving only some black skeletons of the fibers. There was no loss, but a gain of 0·26 g. The product contained 0·13 p. c. of ammonia and a trace of protonitrate.

As the iron in this experiment was all used up, it is plain that the production of ammonia and a protosalt was not owing to the after reaction of the partly made pernitrate on an excess of the metal. And it seems likely that the formation of protonitrate, pernitrate, nitrate of ammonia, and nitric oxyd, and the absorption of oxygen from the air were going on simultaneously, and during the whole time.

It is not certainly known whether, in the direct action of nitric acid on iron, the generation of ammonia and the formation of protonitrate have any necessary connection. I have never detected protonitrate in the product unless ammonia was also present; but ammonia is sometimes found when the iron is all in the state of pernitrate. Yet in the latter case a protosalt may have been formed in some stage of the operation and subsequently oxydized as the action went on. No doubt there is often a partial rearrangement of the elements; for a perfectly clear solution decanted from the undissolved iron, will sometimes in the course of a few days, deposit a considerable quantity of rust and give off some nitric oxyd.

The inquiry naturally suggests itself whether in case of silent solution there may not be a nitrite formed,—2Fe and $3NO_5$ giving $Fe_2O_3\,3NO_4$. Were it so the product would slowly decolorize permanganic acid; but the amount of permanganate bleached by any of the products has never much exceeded what was due to the quantity of protonitrate present. I have not, indeed, found any good reason for believing that there is any such thing as a sesquinitrite of iron.

When we bring together iron and nitric acid of as high a specific gravity as 1·3 the phenomena of passivity sometimes manifests itself; and the induction of such a state depends not merely on the strength of the acid, but has some relation to its quality and temperature, and to the kind and condition of the iron. In speculations respecting the cause of passivity, it has not unfrequently been taken for granted that this singular change is wrought in iron only by the very strongest acid. Thus in a brochure[*] setting forth a new theory of the constitution of acids, Peretti gives the following unsound instance in support of his views, and goes on to argue that inactivity is owing to the lack of a certain hypothetical deutoxyd of hydrogen:—"Il ferro che è attaccato sì fortemente da questo acido, preparato con il commune metodo AzO_2+2HO_2, cessa di esserlo posto a contatto con l'acido azotico fumante AzO_4+HO_2 cioè a dire con questo acido privato de un equivalente di acqua," etc. But the fact that an acid of as low a specific gravity as 1·20 may refuse to act on certain kinds of iron is far from lending any support to this and some other theories that have been proposed. The strongest acid has apparently no more effect on iron than so much water would have; but with acid moderately strong there is often a momentary action, and then the metal brightens and undergoes no further change. Initial temperature has much to do with determining the continuance or termination of the active state; and yet for any particular acid or iron it is not easy to state the exact degree beyond which quiescence is no longer possible; for in a still liquid, an incipient action may, by reason of local heating, get such a start that it will go on to the end, while a thorough and constant stirring would so distribute the small amount of heat evolved on first contact as to cause a speedy cessation of chemical change. But when passivity has once supervened, it requires very strong heating of the acid to make it attack the iron again. The following trials show that in treating of passivity we should take into account not only the strength of the acid but also its quality and temperature, the degree of agitation, and the kind and condition of the iron:

15.—Some nearly pure commercial nitric acid of sp. gr. 1·36 being heated to 34° C. and briskly stirred, a bit of bright iron wire was dropped in. The action continued till the metal was all gone.

In another experiment with the acid at 33° C., the action quickly ceased.

16.—Nitric acid of sp. gr. 1·38 warmed to 31° C., soon rendered iron wire passive.

Some of the same acid at 32° C., kept on acting till the iron was used up.

17, *a.*—A nearly pure and colorless acid of sp. gr. 1·39 warmed to 30° C., rendered iron wire passive; but when such acid was heated to 31° C., before adding the iron, the action kept on.

b.—Some crude red acid of sp. gr. 1·39 warmed to 41·5° C. exercised but a momentary action on wire; and when the acid and iron were heated together after the contact, the wire did not recover its active condition till the temperature was carried up to 88° C.

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[*] **Dell'Azione chimica dell'Acqua sopra i Sali e sopra gli Acidi. Roma, 1861.**

This acid heated to 42° C. rapidly dissolved iron wire.

18, *a.*—A pale commercial acid of sp. gr. 1·40, warmed to 33° C., refused to dissolve iron wire.

With such acid at 34° C., the action kept on.

b.—Some pure acid redistilled with bichromate of potash, having the sp. gr. 1·40, and being warmed to 36° C., quickly induced passivity in iron wire.

With some of the same acid at 37° C., the action did not cease till the iron was all gone.

c.—Pure red acid of sp. gr. 1·40 warmed to 58° C., rendered wire passive.

Such acid at 59° C. continued to act.

d.—Some commercial red acid of sp. gr. 1·40 warmed to 65° C., had but a momentary effect on iron wire.

The same acid at 66° C., kept on acting.

19.—A pale acid of sp. gr. 1·41 heated to 41° C., rapidly dissolved iron dropped in during slight agitation; but with the initial temperature 40° C., the wire became passive.

When this same acid was briskly shaken at the time of contact, 49° C. was the limit of passivity.

20, *a.*—Pale acid of sp. gr. 1·42, at 55° C., rendered iron wire passive; at 56° C. the action kept on.

b.—Common red nitric acid of sp. gr. 1·42, even when warmed to 82° C., induced passivity in iron; but with the same acid at 83°, the action continued.

21.—Steel wire scoured bright became passive when it was agitated with pure colorless nitric acid of sp. gr. 1·30, heated to 50° C.; but when the acid had first been warmed to 51° there was a continued action.

Other acid of sp. gr. 1·30 with other sorts of steel, gave the same limit of passivity.

22, *a.*—Pure nitric acid of sp. gr. 1·34 heated to 71° C., soon ceased to act on steel wire; when such acid was first heated to 72° it did not induce passivity.

b.—The same acid even at 100° C., had no action on the same steel wire freshly annealed and scoured bright.

For trials of rough and less compact steel, two very long turnings were broken up, and furnished bits enough for a large number of experiments with metal of one and the same quality.

23.—On this steel nitric acid of sp. gr. 1·26 acted rapidly and continuously, even when it had been previously cooled to 0° C.

Nitric acid of sp. gr. 1·27 and at 40° C. soon ceased to act on a bit of steel added during brisk agitation; with the acid first heated to 40·5° the action continued.

And with acids of sp. gr. 1·28, 1·29, 1·32, 1·33 and 1·34, the limit of passivity was between 35° and 40° C.

With acid of sp. gr. 1·345 and at 52·8° C., the steel became passive; with the same acid first heated to 53·3°, the action went on.

Acid of sp. gr. 1·348 even at 76·7° C. rendered the steel passive; when this acid was first heated to 79·5° the action would not stop.

24.—The steel turnings were also tried with common red nitric acid.

Such acid of sp. gr. 1·34, at 65° C., rendered the steel passive; with the initial temperature 65·6° it continued to act.

Acid of sp. gr. 1·335, at 58·3° C., soon ceased to act; with such acid at 58·9° the action went on to the end.

With acid of sp. gr. 1·33, at 49·4° C., the action was momentary; with this acid first heated to 50° there was no stoppage.

With acid of sp. gr. 1·327 the limit of passivity lay between 47·2° and 47·8° C.

Acid of sp. gr. 1·32 at 32·2° induced passivity; with the same acid at 32·8° the action continued.

With cast iron the limit of passivity cannot be so precisely defined. Acid of moderate strength often continues to dissolve the iron very slowly and silently after the first momentary, violent action is over. It having been found by many trials that passivity may occur in cast iron even with acid of as low sp. gr. as 1·20, the following definite experiments were made:

25.—Took in each case a tared lump of iron weighing between 2·8 and 3·3 grams, and some acid at 25·5° C. At the end of four hours the bits of iron were cleared of the loosened carbon, washed, dried and weighed.

With acid	of sp. gr.	1·20	the iron	had lost	0·218 g.
"	"	1·19	"	"	0·290
"	"	1·18	"	"	0·386
"	"	1·17	"	"	1·497
"	"	1·16	"	"	1·529
"	"	1·15	"	"	1·512
"	"	1·14	"	"	1·454

With the 1·20 acid the apparent action lasted but five minutes, and in the next two cases the evolution of gas continued somewhat longer. In the remaining instances the action lasted throughout.

26.—Of another specimen of iron, pieces weighing between 6 and 7 grams were tried, each with 20 c. c. of acid at 28° C. At the end of 13 hours the products were gelatinous, basic pernitrates, turbid looking, but quite soluble in water.

With acid	of sp. gr.	1·18	the iron	had lost	2·264 g.
"	"	1·175	"	"	2·295
"	"	1·17	"	"	2·266
"	"	1·16	"	"	2·246
"	"	1·15	"	"	2·141

27.—Another sample of cast iron in pieces weighing between 2·7 and 3 grams was tried in each instance with 20 c. c. of acid at 23° C. At the end of nine hours the iron in acid of sp. gr. 1·20 had lost 0·285 g.

"	"	1·195	"	0·296
"	"	1·19	"	0·279
"	"	1·185	"	0·315
"	"	1·18	"	0·375
"	"	1·175	"	0·407
"	"	1·17	"	0·414
"	"	1·165	"	0·708
"	"	1·16	"	1·692
"	"	1·155	"	1·856
"	"	1·15	"	1·830

In the first instance the action was apparently over in six minutes. In the next five cases all was quiet at the end of about twenty minutes. In the last three there was no stoppage.

The phenomenon of passivity,—which had been observed by Wenzel[7] as long ago as 1782, and has been the subject of more speculation than experiment,—is explained by some as owing to the formation of a coating of insoluble oxyd. But we have no reason to suppose that an uncombined oxyd can possibly be formed in the presence of a great excess of strong acid. In fact after contact with the acid the surface of the iron often remains as bright as ever, and a crust so thin as to be transparent must be too thin to be impermeable.

Others make the more plausible assertion that the metal immediately becomes covered with a layer of nitrate insoluble in nitric acid. Thus Heldt says:[8]—"Die Haut von saltpetersauren Eisenoxyd durch welche das blanke Metall mit seinem Glasz hindurch schimmert, ist unter einer guten Lupe deutlich zu erkennen." I have never been able to detect any such film with a magnifier, and my experiments with heated nitric acid go to show that in many cases an adhering coat of nitrate cannot exist; for though the crystallized salt is not soluble in cold strong acid, it dissolves readily enough even in monohydrated acid with the aid of a moderate heat. When iron has become passive while cold, the acid ought not to require heating beyond 50° to make the action recommence. But we have seen that even boiling sometimes fails to remove passivity.

When the iron is temporarily attacked, an incrustation of oxyd, or of nitrate, could only by degrees acquire sufficient thickness to resist the acid; and hence the theory of a protective film would require the gradual cessation of activity; but in fact the cessation is usually sudden and abrupt.

Reason seems to be on the side of those who refer passivity to a molecular change in the iron, which may be induced by a momentary electric action set up on the first contact of the acid with the metal. And such an alteration in the electric or chemical relations of iron is no more wonderful than the modification of the mechanical properties of steel by tempering, or the induction of permanent magnetism in steel.

The following summary shows the results to be obtained by bringing together iron and nitric acid under various conditions.

1.—There may be either no perceptible action or only a momentary one, the iron quickly becoming passive.

Passivity supervenes most readily with a red acid and with carburetted iron. And steel has its liability to become inert enhanced by annealing.

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[7] Lehre von Verwandschaft der Körper, p. 108; note.

[8] Erdmann's Jour. f. Pr. Chem. xc, 267.

2.—There may be silent action with the production of protonitrate, or of pernitrate, or of both together.

3.—There may be a more or less rapid evolution of gas,—mostly nitric oxyd,—and the formation of an acid, normal, basic, or rusty pernitrate, or of a mixture of protonitrate and pernitrate.

4.—Hydrogen may be given off continuously, while a protonitrate is formed.

5.—Hydrogen may be evolved rapidly at first and then more and more slowly till there comes a pause; after which the reaction changes and nitric oxyd is liberated abundantly, a pernitrate being the final product.

6.—A very small portion of the acid may be decomposed so as to generate nitrate of ammonia; and this can take place while nitric oxyd, or hydrogen, or no gas at all is given off.

7.—During a moderate action there may be an absorption of oxygen from the air, and consequently a greater amount of oxydation will ensue than can be accounted for by the gases extricated and the nitrate of ammonia formed.

Pernitrate solutions made with weak acid and excess of iron are generally so basic as to become turbid when they are treated with a solution of any sulphate. The precipitate, which is probably the tribasic persulphate of iron, is producible whenever the nitrate solution contains less than two equivalents of acid to one of ferric oxyd.

The proneness of nitric acid to dissolve an excess of iron, renders it hardly possible to make directly a solution that shall contain an exact normal nitrate; for even if we try to use the precise quantities of material which theory would indicate, the reaction is not simple enough to allow us to predict just how much acid will be consumed in effecting the oxydation. Yet the procuring of a normal nitrate, which has been spoken of by some writers as very difficult, is far from being a hard matter. We have only to resort to the process by which most other crystallizable salts are insured free from excess of acid or base. The nitrate readily crystallizes out of an acid or normal solution, and of all the ferric salts it is the easiest to obtain in crystals and in a state of purity. In concentrating a solution, care should be taken to apply but a moderate heat: and, before setting it aside to crystallize, enough strong nitric acid should be added to make the liquid seen in a thin stratum appear of a pale yellow color instead of a deep red.

Solid Nitrate of Iron.—There are two definitely crystallized sesquinitrates of iron, differing much in form and composition. The cubic nitrate noticed by Schönbein[9] in 1836 and analyzed by Hausmann[10] in 1854, is a tetrahydrate, $Fe_2O_3\,3NO_5\,12HO$,

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[9] Pogg. Ann., xxxix, 141.

[10] Annalen der Ch. and Ph., lxxxix, 109.

—and, as will be shown presently, is produced only under circumstances of peculiar constraint. The more commonly occurring salt, which forms when there is no deficiency of water, is the oblique rhombic sexhydrate,—$Fe_2O_3\ 3NO_5\ 18HO$,—described in this Journal in 1850.[11] At that time I was not aware that any previous analysis had been recorded; but in a little work published in 1834, entitled "Manuel du Fabricant d'Indiennes, par L. J. S. Thillaye," is to be found the following passage:—"Si l'on veut obtenir des cristaux de pernitrate, on fait dissoudre lentement le fer dans l'acide nitrique à 34°, en plaçant le vase dans un lieu frais. Lorsque la dissolution est à peu près complète, il se forme des cristaux qui affectent la forme de prismes droites à quatre pans et à bases carrées, quelquefois ils ont six pans. * * * * Le pernitrate cristallise est formé de Acide Nitrique 41,968, Peroxide de fer 20,725, Eau 37,307." This composition corresponds nearly to $Fe_2\,O_3\,.\,3NO_5\,.\,16HO$, and the specimen analyzed must have been a mixture of the two sorts. Having observed scores of crops of crystals formed under very diverse circumstances, and having invariably found the sexhydrated salt to belong to the monoclinic system, I think it would not be too much to say that those who speak of square prisms, either have had the cubic nitrate or a mixture to deal with, or have used Vauquelin's eyes instead of their own. "An observation which Citizen Vauquelin has communicated" is thus given in Fourcroy's Chemistry:—"Concentrated nitric acid poured on an oxide of iron arising from the decomposition of water, had been left in contact with it several months. * * * Citizen Vauquelin was much astonished on stirring the liquor, which was of a brown color, to see in it several large crystals, the least of which weighed upwards of four grammes. * * * Their form was that of a square prism terminated by a bevel." As Vauquelin used concentrated acid, he might possibly have had the cubic crystals; but the cubic nitrate is not often modified, and the last clause, respecting termination "by a bevel," suggests a query whether the dimetric form was not predicated from a hasty glance rather than from measurement or from careful inspection. And there is the more room for a doubt because in "Citizen" Vauquelin's time the science of crystallography lacked much of the simplicity and precision which it has subsequently attained. In all likelihood it was the oblique nitrate which was first observed by Vauquelin; and I may, perhaps, rightfully claim to have been the first to determine its true character and composition, and its close correspondence to the nitrates of alumina and chromium.

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[11] **The editor of the Chemical Gazette,—xii, 211,—speaking of Hausmann's nitrate says:—"Ordway gave the formula $Fe_2\,O_3\ 3NO_5\ 18HO$ for it." He should have observed that I gave no formula for the still undiscovered cubic salt, but described crystals the inclination of whose upright prismatic faces differed 11° from a right angle.**

Hausmann obtained the cubic nitrate by evaporating a solution, at a very moderate heat, to the consistency of syrup, and then adding to it half its volume of nitric acid. By analysis, the crystals pressed in absorbent paper, gave him per-centages corresponding to $Fe_2O_3, 3\frac{2}{9}NO_5 + 13HO$, equal to $Fe_2O_3, 3NO_5, 12HO + \frac{2}{9}(NO_5 4HO)$.

Wildenstein[13] found several pounds of cubic crystals deposited from a quantity of very strong commercial nitrate that had stood a year and a half. They had almost exactly the composition $Fe_2O_3, 3NO_5, 12HO$. These accidentally formed crystals appear to have come from a somewhat basic liquor deficient in water; and that the segregation had taken place under very unfavorable circumstances, is shown by the fact that only "10 Pfund" crystallized out of "2½ Centner" of the solution.

Hausmann's crystals were produced in a very concentrated liquor containing a large excess of acid,—a condition much better suited to giving a large crop. A lack of water is, no doubt, the essential requisite for the production of cubic nitrate; for I find, by many experiments, that unmixed cubic crystals can be made at pleasure by so adjusting the solution to be crystallized, that the attraction of the tetrahydrated nitrate for water, shall be rather more than counteracted by the attraction of an excess of nitric acid for water;—that is, so that the liquid shall consist of tetrahydrated ferric nitrate dissolved in trihydrated nitric acid. Almost the whole of the nitrate then crystallizes out, because at low temperatures it is but slightly soluble in acid of such strength. A solution containing more water than will make up $Fe_2O_3, 3NO_5, 12HO + n(NO_5, 3HO)$ may afford a few of the rhombic sexhydrate crystals mixed with the cubic; and one containing less water will hardly give any solid nitrate at all. When there is water enough present to make up $Fe_2O_3, 3NO_5 18HO + n(NO_5, 4HO)$, the crystals will be all oblique rhombic.

The conditions of formation of the solid nitrates are somewhat similar to those for the production of the two hydrated chlorids. When a solution is not basic and contains between six and twelve equivalents of water to one of perchlorid of iron, it first deposits the deutohydrate, $Fe_2Cl_3 6HO$, in transparent crystals, and then goes on to form the light yellow, opaque, indistinct tetrahydrate, $Fe_2Cl_3 12HO$. Unless the liquid lacks water, no transparent crystals can make their appearance.

For making cubic nitrate, the oblique crystals afford a definite material that admits of appreciable treatment. We may take a weighed quantity of these crystals in a tared dish, and evaporate by a gentle heat about fourteen per cent of their weight, or six equivalents of water; then add trihydrated nitric acid enough to double the bulk of the liquid, and set the whole aside to crystallize. A still easier method is to melt one equivalent of the common nitrate and add a little over two equivalents of monohydrated nitric acid. Atmospheric moisture must be carefully excluded during the cooling and the subsequent repose.

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[13] Erdmann's J. für Pr. Ch., lxxxiv, 248.

The following experiment was made with a view of finding out whether there is any crystallizable nitrate of iron containing still less water than the cubic salt:

28.—100 g. of $Fe_2O_3, 3NO_5, 18HO$, were gently heated till 27 p. c. were evaporated, one-half of the water and one-eighth of the acid being expelled. 42 g. of monohydrated nitric acid were added and the dish suitably covered was set out in an open shed for several days in the coldest weather of winter. No crystals formed except a slight fringe around the edge, where a little moisture had probably been absorbed from the air.

Afterwards the whole was heated and 33 g. of oblique rhombic crystals were dissolved in it. On standing in a cold place a full crop of the cubic salt was soon deposited.

So then the drying power of nitric acid does not enable us to get a nitrate containing less water than the tetrahydrate, but, when pushed too far altogether prevents crystallization. Yet Scheurer-Kestner[13] speaks of a salt of the very remarkable and irregular composition $Fe_2O_3, 3NO_5, 2HO$. To obtain this, he evaporated a solution of nitrate, at a moderate heat, till it became very dense and a part was decomposed. The liquid portion on cooling produced a mass of confused crystals of the almost anhydrous salt. The mother liquor by long standing gave crystals of the cubic nitrate.

M. Scheurer-Kestner erroneously cites me as having mentioned in this Journal, vol. xxvii, p. 197, such a salt as $Fe_2O_3, 3NO_5, 6HO$. He probably meant to refer to p. 17 of that volume, where I have taken the liberty of showing the generic agreement of the sesquinitrates with the sexhydrated protonitrates, by writing the ferric salt $\bar{\mathrm{F}}\mathrm{e}+3\dddot{\bar{\mathrm{N}}}\bar{\mathrm{H}}_6$; and he must have been led astray by a misapprehension of the Berzelian method of notation. A dihydrated nitrate would be too singular to be accepted without a full characterization; and I should be very far from calling on the world to believe in a $\bar{\mathrm{F}}\mathrm{e}\dddot{\bar{\mathrm{N}}}_3\bar{\mathrm{H}}_6$ on the strength of a mere formula. At the time of writing the article on nitrates, I had not met with Hausmann's account of the cubic salt, and therefore was not aware of the peculiarity of nitrate of iron in ranking among the tetrahydrates as well as in the more extensive class of sexhydrates. Other salts of the tetrahydrate class may exist as anhydrous nitrates, and it is a question whether Scheurer-Kestner's more recently discovered salt, $\bar{\mathrm{F}}\mathrm{e}\dddot{\bar{\mathrm{N}}}_3\bar{\mathrm{H}}_2$, is not in reality the anhydrous pernitrate of iron with a slight admixture of the cubic nitrate. For he did not obtain it in well defined crystals and therefore could not be sure of its singleness; and moreover there is no analogy in favor of the formula which his analysis assigns to it. It is very remarkable that the tetrahydrate is more fusible than the oblique rhombic salt which contains nine per cent more water. The sexhydrate melts at 47° C., and Hausmann found the cubic nitrate to liquefy at 35° C.; but his product contained a little free acid and it is probable that the pure tetrahydrate melts at a point between 35° and 40°.

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[13] Bulletin Soc. Chimique de Paris, March, 1862, p. 36.

If six equivalents of water are added to the melted cubic nitrate, heat is evolved, and, after a while, the whole becomes solid.

The unaccountable anomaly of greater fusibility with a less percentage of water is not peculiar to the nitrates of iron, but is shown also by the perchlorids. For I find the transparent dihydrate, $Fe_2 Cl_3\ 6HO$, to melt at 31° C., while the opaque tetrahydrate, $Fe_2 Cl_3\ 12HO$, fuses at 35 5° C. And if a mixture of the two chlorids is slowly heated, the transparent crystals liquefy first. The addition of six equivalents of water to the melted dihydrate is attended with the production of heat.

The plan of using fuming nitric acid to contend with a hydrated nitrate for the possession of water, of course admits of more extended application. With nitrate of alumina indeed it does not work well, on account of the high melting point of that salt and its very slight solubility in even boiling acid. Nitrate of chromium is more tractable, and a mixture of monohydrated acid with the melted crystals, on standing, lets fall nearly all the nitrate as a confused crystalline mass probably containing less than twelve equivalents of water, and having little resemblance to the cubic nitrate of iron. In a single instance I have seen in the mass one or two crystals apparently cubic, but they were too minute and scanty to allow any special examination. Nitrate of glucina has never been obtained except as a trihydrate, as mentioned in a former paper. Since the date of that publication I have succeeded in getting more distinctly formed crystals which appeared to the eye dimetric, being indeed very short square octahedra. It remains then still uncertain whether the other sesquinitrates can form salts corresponding to the cubic nitrate of iron.

With the help of strong nitric acid, the nitrates of manganese and zinc may be crystallized with less than six equivalents of water, but these products have not yet been fully examined.

Protonitrate of iron is a salt of such a nature that we can never expect to get it in the solid state except as a sexhydrate.

Protonitrate of Iron.—The most convenient way of procuring ferrous nitrate, is to dissolve the protosulphid of iron in nitric acid of less sp. gr. than 1·12, no considerable elevation of temperature being allowed. Though protonitrate crystals are very unstable, a moderately strong solution may be evaporated without much risk by a heat not exceeding 60° C. As the strength increases, however, the temperature must be lowered, especially if there is an excess of acid. By thus proceeding with greater caution as the liquid becomes more concentrated, we may get it strong enough to crystallize when exposed to the cold of winter.

The following experiments show the difference between weak and strong, acid and neutral solutions, in respect to their endurance of heat:

29, *a.*—A solution made directly with sulphid of iron and nitric acid of sp. gr. 1·08, contained 29 p. c. of $FeO, NO_5, 6HO$, and 0·6 p. c. of free acid. Some of it heated slowly remained clear and unchanged while the temperature rose to 83° C. At 90° it underwent a rapid transformation and became rusty.

b.—Some of the solution mixed with about its own weight of water, continued unaltered till it was heated to 100° C. It required some five minutes boiling to effect a complete rearrangement of its elements.

c.—Some of the solution on being merely mixed with one-sixth of its weight of pure nitric acid of sp. gr. 1·42, at once gave off gas and made a clear, dark red liquid.

d.—Some of the solution was mixed with nearly its own weight of nitric acid of sp. gr. 1·10 and heated slowly. At 66° C., it started and quickly changed to pernitrate.

e.—A mixture of 28 g. of the solution with 11 g. of pure nitric acid 1·10, began to alter at 77° C.

30.—A solution of the crystals, saturated at 15° C. and containing 71 p. c. of $FeO, NO_5, 6HO$, was heated in a water-bath. It began to give off gas at 85° C. At 88° the rearrangement went on violently and was soon finished.

The crystals themselves while still wet, may be kept in the cold without alteration; but when they are quite free from mother-liquor, they are apt to start suddenly, even at the common temperature of the air, and undergo a rapid transformation, a dark red, clear, basic pernitrate being produced. The composition of the pasty mass, in one instance, was found to be about $\overline{\mathrm{Fe}}\,\overset{\cdot\cdot}{\overline{\mathrm{N}}}_{1\cdot8}$; in another it was $\overline{\mathrm{Fe}}\,\overset{\cdot\cdot}{\overline{\mathrm{N}}}_{1\cdot7}$. These products are apparently soluble in water; but the liquor is slightly opaline when looked at, though it is clear when seen by transmitted light. The crystals themselves therefore change to a substance almost perfectly soluble in water, while solutions of the crystals, by spontaneous decomposition, become rusty, a portion of the peroxyd assuming the inert, insoluble state.

The foregoing experiments show that neutral solutions, even when very strong, will bear a pretty high temperature. Weak solutions may be brought for a moment to the boiling point without immediate injury. But acid solutions undergo a change much more readily, and the greater the quantity of acid the less stable are they.

Crystals of protonitrate of iron, well drained and dried at a very low temperature, were found to yield by ignition 27·565 p. c. of ferric oxyd. The formula must therefore be $FeO, NO_5, 6HO$, which would correspond to 27·778 p. c. of peroxyd.

A solution saturated at 0° C., yielded a quantity of peroxyd equivalent to 66·3 p. c. of the crystals. So at 0° the salt is soluble in half its weight of water.

A solution saturated at 15° C., contained 71 p. c. of crystals. Its sp. gr. was 1·48.

A solution saturated at 25° C., contained 75 p. c. of crystals. Its sp. gr. was 1·50. At 25° C., then the crystals are soluble in one-third their weight of water.

The slight difference in strength between cold and warm solutions, as well as the instability of the solid salt, shows the inexpediciency of attempting to do anything with it except in the coldest weather.

Art. XXXIV.—*Some Indications of a Northward Transportation of Drift Materials in the Lower Peninsula of Michigan;* by Professor Alexander Winchell.

Throughout the northern part of Lenawee and Hillsdale counties, the southern and eastern parts of Jackson, and the southern and western parts of Washtenaw county, are found numerous tabular, detached masses of limestone, sometimes cropping out on a hill side, like a ledge in place, and sometimes imbedded two or three feet in the sand and gravel at the summit. The position of these masses is generally nearly horizontal, though for the greater part slightly tilted in one direction or another. They sometimes present an extent of six, eight, or twelve feet square; and in occasional instances even more, so as to offer every appearance of an outcropping formation. In some cases many hundred bushels of lime have been burned from them before exhaustion. Underneath them we find the semi-stratified drift materials so characteristic of the general surface of the peninsula. At the bottom of the drift, which in some places is not over ten or twenty feet deep, we find the rocks of the Huron, or more frequently the Marshall group, in place. Many patches, nevertheless, occur as far north as the outcrops of the Carboniferous limestone, and create great confusion in tracing the latter formation unless the fossils are strictly attended to. Smaller fragments of the same limestone are still more abundant throughout the same limits; and, by their disintegration, have imparted a highly calcareous element to the soil, even along the arenaceous belts. The percolation of meteoric waters, in turn, has given rise, in great abundance, to calcareous springs, and deposits of marl, tufa and travertin.

In the southwestern part of the peninsula, in the counties of Berrien, Van Buren and Ottawa, similar phenomena are again observed. The calcareous element of the soil is even more abundant; and large patches of sand have become firmly cemented, so as to present the consistency and appearance of ledges of sandstone. It really requires an extended series of observations to convince one's self that the region does not furnish considerable outcrops of a sandstone formation—especially as numerous blocks of undoubted Marshall sandstone occur upon the surface.

Some examples of these phenomena may be more definitely cited. On the S.W. ¼ S.E. ¼ sec. 13, Woodstock, Lenawee County, (221),[1] in the side of "Prospect Hill," limestone occurs in tabular masses six by twenty feet, and seven feet thick. An old lime-kiln stands near. Similar limestone occurs on the S.E. ¼ S.W. ¼ sec. 12, Woodstock (223) and S.W. ¼ S.E. ¼ sec. 1. From the latter locality several hundred bushels of lime have been burned. On sec. 3 of the same township, the Marshall sandstone is reached at the depth of 4 to 12 feet from the surface. On sec. 4 the sandstone is known to be over 75 feet thick; so that there can be no mistake in assuming it to be in place. On the N.E. ¼ N.W. ¼ sec. 6, Woodstock (230), is a large tabular mass 10 feet long and known to be over 6 feet broad. The dip is toward the east.

On the N.W. ¼ N.W. ¼, sec. 32, Columbia, Jackson county, (233) is a mass of limestone. Sixty rods north of here the sand-rock is known to be within 20 feet of the surface, and has been penetrated 44 feet. On sec. 30 it has been penetrated 60 feet. On the N.W. ¼ N.W. ¼ sec. 26, Liberty, Jackson county, (235), masses of limestone are so abundant that a kiln has been constructed and several hundred bushels of lime manufactured. At the time of my visit, about 35 cords of wood were piled by the kiln, indicating considerable confidence in the resources of the quarry. Fragments of Marshall sandstone, with its fossils, are abundant, mingled with the fragments of limestone. Similar masses of limestone may be seen again on N.W. ¼ N.E. ¼ sec. 26, Hanover, (239), and on the S.W. ¼ N.E. ¼ same section, (240). Several wagon loads of fragments have been removed from the subsoil over the principal mass at the latter place. On the S.E. ¼ S.E. ¼, same section, a common well reached the Marshall sandstone at the depth of 35 feet, and was excavated 40 feet in the rock. The hills on N.E. ¼ N.E. ¼ sec. 33, same township,

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[1] These numbers designate the localities.

(243), are filled with fragments of limestone, while the Marshall sandstone is struck at the depth of 50 feet on sec. 35, and at 30 feet on sec. 27. Farther north, on secs. 10 and 11, Leoni, (92 and 206), similar fragments again occur. Also on sec. 25, Grass Lake, (208). At a place one mile northeast of Franciscoville, (209), 20,000 bushels of lime have been manufactured in ten or twelve years.

In the adjoining parts of Washtenaw county, several kilns proclaim the presence of extensive nests of limestone. Even within the corporate limits of the city of Ann Arbor an extensive deposit has been quarried; and just beyond the limits, on the east, are the ruins of a limekiln which, many years ago, exhausted still another deposit.

Similar masses of limestone occur in Hillsdale county, one half mile southwest of Jonesville, (270); on S.W. ¼ N.W. ¼ sec. 21, Allen, (274); N.E. ¼ N.W. ¼ sec. 21, Adams, (289); S.E. ¼ N.E. ¼ sec. 22, Adams, (291); N.E. ¼ N.E. ¼ sec. 24, Adams, (292); S.W. ¼ N.W. ¼ sec. 19, Woodbridge, (296), and in many other localities.

In the southwestern part of the state, on the S.E. ¼ N.W. ¼ sec. 11, Hartford, Van Buren county, (415), limestone is found within two feet of the surface over the space of three or four square rods. One hundred rods north of here the wells are from 40 to 100 feet deep, without reaching any native rock—though thick beds of cemented sand and gravel are frequently encountered.

On the S.W. ¼ sec. 17, T. 7 N.R. 13 W, Ottawa county, (438) is the last occurrence that will be cited. Several slabs 3 or 4 feet long have been removed, and others remain, over an area of at least a square rod.

It is quite evident that such masses of stratified limestone have not been rolled to the same extent as the quartzose and gneissoid boulders which constitute the most striking feature of the "northern drift" of the same regions. By some agency these tables have been lifted gently from their original sites and carefully deposited where we find them. Paying no regard to their included fossil remains, it might be imagined that they constitute the ruins of the Carboniferous limestone formation, whose place is between these fragments and the centre of the peninsula. This formation gently rises toward the periphery of the peninsula, and at certain distances from its present outcrops would intersect the highest diluvial hills in the places occupied by the fragments which I have described; and which, for this reason, might be imagined as marking the outermost limits of a once continuous formation of Carboniferous limestone. The abundant fossil remains contained in these fragments, however, not to speak of their lithological characters, convince us that the rock belongs to a much earlier epoch.

In short, no doubt could be entertained by one who has examined the subject, that these fragments appertain to the age of the Corniferous limestone. The rock, in structure, is irregular, often brecciated, with streaks and nests of bituminous and argillaceous matter. At other times it is compact and massive. Not unfrequently it presents the peculiar structure known as "lignilites." All these characters belong to the Corniferous limestone as exposed along the western shore of Lake Erie, and at numerous points throughout the county of Monroe.

Turning to the more reliable evidence of the fossil remains, it may be stated that the following are examples of the more frequent identifications:

At 92, *Heliophyllum Canadense* Billings, *Clisiophyllum Oneidaënse* Bill., *Acervularia Davidsoni* E. & H., *Conocardium trigonale* Hall, *Proetus crassimarginatus* Hall, *Amplexus* and *Favosites.*

At 182, *Lucina proavia* Goldf., *Conocardium trigonale*, *Dentalium* and *Fenestella.*

At 208, *Lucina proavia* and three species of Bryozoa.

At 230, *Strophomena rhomboidalis* Wahl, *Chonetes glabra* Hall, *Spirifera gregaria* Clapp, *Atrypa reticularis* Dal.

At 233, *Chonetes hemispherica* Hall, *Atrypa reticularis*, *Cyrtodonta* (*Vanuxemia*) *Tompkinsi* Bill, *Pleurotomaria rotunda*, Hall.

At 270, *Chonetes lineata* Hall, *Atrypa reticularis.*

At 289, *Chonetes lineata*, *C. glabra* Hall, *Atrypa reticularis*, *Leiorhynchus multicosta* Hall, *Rhynchonella Thalia* Bill., *Lucina proavia*, *Proetus crassimarginatas* Hall, *Platyceras and Proetus* sp.?

At 291, *Cyathophyllum Zenkeri* Bill, *Chonetes lineata*, *C. glabra*, *Orthis Vanuxemi* Hall, *Strophomena hemispherica*, *S. rhomboidalis*, *Spirifera varicosa* Hall, *S. gregaria*, *Atrypa reticularis*, *Charionella scitula* Bill., *Rhynchonella Thalia*, *Lucina proavia*, *Conocardium trigonale*, *Proetus crassimarginatus*, *Fenestella*, *Producta* (two species), *Streptorhynchus*, *Athyris*, *Platyceras.*

At 292, *Orthis Vanuxemi*, *O. propinqua* Hall.

At 296, *Chonetes glabra*, *C. arcuata* Hall, *Stricklandia elongata* Bill.

At 298, *Strophomena hemispherica.*

The following are the more common Corniferous fossils occurring in the Drift at Ann Arbor:

Favosites Gothlandica Goldf., *F. cervicornis* De Blainv., *F. turbinata* Bill., *F. polymorpha* Goldf., *F.* (*Emmonsia*) *hemispherica* Y. & S. sp., *Fistulipora Canadensis* Bill., *Michelinia convexa* D'Orb., *M. favosoidea* Bill., *Alveolites labiosa* Bill., *A. Rœmeri** Bill., *Syringopora perelegans* Bill., *S. Hisingeri* Bill., *S. Maclurei* Bill., *S. nobilis* Bill, *Stromatopora concentrica* Lonsd., *Cyathophyllum Zenkeri* Bill., *Zaphrentis prolifica* Bill., *Clisiophyllum Oneidaënse* Bill., *Heliophyllum Eriense* Bill., *H. exiguum* Bill., *Cystiphyllum Americanum* E. & H., *Blothrophyllum decoricatum* Bill., *Diphyphyllum Archiaci*[1] Bill., *Phillipsastræa Verneuili* E. & H., *P. gigas* Owen, sp., *Acervularia Davidsoni*[1] E. & H., *Tentaculites scalaris* Schlot., *Chonetes glabra* Hall, *C. hemispherica* Hall, *Strophomena hemispherica* H., *S. perplana* Con., *S. inæquiradiata* H., *S. demissa* Con., *Orthis propinqua* H., *O. Eryna* H., *Ambocœlia umbonata* Con. sp., *Spirifera gregaria* Clapp, *S. varicosa* H., *S. acuminata* Con. sp., *Leiorhynchus multicosta*[1] Hall, *Nucleospira concinna*[1] Hall, *Charionella scitula* Hall, sp., *Atrypa reticularis* Dal., *A. impressa* H., *A. aspera?* Hall, *Meristella unisulcata* Con., sp., *M. nasuta* Con., sp., *Leptocœlia concava* H., *Pentamerus aratus* Con., sp., *Stricklandia elongata* Vanux., sp., *Centronella glansfagea* Hall, sp., *Rhynchonella Thalia* Bill., *Lucina proavia* Goldf., *Conocardium trigonale* Hall, sp., *Platyceras Thetis* H., *P. crassum* H., *P. dumosum* Con., *Platyostoma strophius* H., *Murchisona Leda* H., *Proetus crassimarginatus* H., and more than two dozen species which seem to be undescribed.

If no reasonable doubt exists that these detached masses belong to the Corniferous limestone, the next question which presents itself relates to the region whence they have been derived. In view of the facts cited, it is evidently absurd to assume that no transportation has taken place; for these masses of Corniferous limestone are found resting over the Hamilton group, the Marshall group and the Carboniferous limestone—and, I am pretty well convinced, even in some cases, as far north as the Coal measures. There are insuperable objections to assuming that they have been transported with the great mass of drift materials from the northern outcrops of the rocks of this age at Mackinac and the surrounding region. *First*, the transporting agency has not moved masses of other kinds of rocks which attain to anything like the same dimensions. *Secondly*, That agency, if we may judge from the condition of the siliceous, trappean and gneissoid boulders of admitted northern origin, would have ground to powder so fragile and friable a rock as these limestones; or at least would have broken them into small fragments, and deposited them in a worn and rounded condition. *Thirdly*, If the Corniferous limestone could have been transported in such masses from its northern outcrops to southern Michigan, much more would the harder and more massive Niagara limestone of the same regions have been similarly transported. The same may be said of the Trenton limestone. We find, however, that fragments of these limestones are of rare occurrence; and the fossils of Silurian age scarcely sustain to those of the Corniferous limestone the ratio of one to one hundred, in the drift deposits of the southern portion of the state. Even the Carboniferous limestone, whose outcrop extends through Kent, Eaton, Jackson and Oakland counties, is scarcely represented among the drift materials of the region in question. It is apparent that we must look in another direction for the origin of these lost rocks.

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[1] These species of the Hamilton group are here included, because occurring in the same fragments with admitted Corniferous (and Schoharie grit) species. The Lower Helderberg *Leptocœlia concava* is included for the same reason. In none of the cases just referred to, however, do we experience any difficulty in discovering slight constant peculiarities in the Corniferous species. The richness of the exotic Drift fauna of this locality, in the number and state of preservation of its remains, far exceeds any that has been signalized by the geologists of the Old World.

The nearest outcrop of the Corniferous limestone is in northern Indiana and Ohio, and the southeastern corner of Michigan[*]. Every fossil cited above is found in place in some part of that region. The formation dips under the peninsula of Michigan; and, throughout the area occupied by the lost masses in question, it lies from one hundred to twelve hundred feet beneath the surface—the depth of course increasing toward the center of the Carboniferous area. The circumstances suggest the exertion of some powerful agency acting northward with tremendous energy, but with a gentle and equable movement. It would seem as if the summit of the low anticlinal in the Corniferous limestone to the southeast of Michigan had been immersed in a shallow sea or lake, the freezing of which had incorporated the upper layers of the rock in an immense thickness of ice, which, by a rise in the water, had floated off as enormous ice-floes, bearing their cargoes of limestone northward till deeper and milder water loosened their icy hold, or they became stranded on the bosses which mark the belt of the Marshall sandstone. From the position of these masses in the "Modified Drift," as well as from the direction and gentleness of the movement, it would seem impossible that the events should have been contemporaneous with the actions which characterized the great glacial epoch.

Additional facts exist which seem to lead the mind further toward a belief in a northward acting post-glacial agency. The fossils of the Hamilton group, whose outcrop is along a belt lying somewhat farther north than the Corniferous limestone, are scattered through the soil of the region lying still farther north, in a degree of abundance which bears about the same ratio to that of the Corniferous fossils as the attenuated Hamilton rocks bear to the Corniferous. Indeed it may be said that *Spirifera mucronata* is the most abundant single species in our drift; as it was certainly the most abundant species that lived in these parts of the Lower Devonian sea. Other common species of the Hamilton group are *Ambocœlia umbonata* Hall, *Cyrtia Hamilton-*

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[*] It probably underlies the drift materials of the southwestern angle of the state and the adjacent parts of Indiana, but no actual outcrops are known to exist.

ensis Hall, *Spirifera Marcyi* Hall, *Spirigera concentrica* Brown, sp., *Platyceras attenuatum* Hall, *Dalmania Boothi* Green, sp., and many others. These species exist in an admirable state of preservation, quite incompatible with the theory of their transportation from the far north; and they occur across a belt of the state reaching at least thirty miles north of the outcrop of the Hamilton rocks.

Again, above the identifiable Hamilton rocks, we find, in this state, a great thickness of argillaceous and bituminous shales, destitute of fossils, but freighted with Kidney iron ore. Nodules of this ore are strewn not only over the region of the outcrop of these "Huron" shales, but throughout Washtenaw and other counties lying over the upper Devonian and lower Carboniferous strata—to say nothing of the occurrence of such nodules within the limits of the Coal measures, where, by some geologists, they might preferably be referred to the indigenous strata.

Still again, the well marked fossiliferous beds of the Marshall sandstone, lying next above the Huron shales, and outcropping along a belt still farther north, is represented by a series of enormous fragments resting over the non-fossiliferous upper portions and the Carboniferous limestone. The lower, or fossiliferous portions of this formation do not outcrop farther north than Moscow, in Hillsdale county, while fragments of it have been transported in great abundance into the southern townships of Jackson county. The most notable example occurs in a deep railroad cut three miles north of Napoleon, where the abundant fossiliferous fragments led me for some time to suppose the actual outcrop must be in the immediate vicinity; although I had found the non-fossiliferous Napoleon sandstone intervening between the locality and the most northern known outcrop of the fossiliferous beds at Moscow. I collected here a large proportion of the common fossils of the Marshall group such as *Rhynchonella Sageriana*, *Chonetes pulchella*, *Myalina Michiganensis*, *Cardiomorpha modiolaris*, *Tellinomya Hubbardi*, *T. Stella*, *Pterinea crenistriata*, *Cardium Napoleonense*, *Solen scalpriformis*, *Bellerophon galericulatus*, *Orthoceras Indianense*, *Goniatites Marshallensis*, and numerous other species. The fossiliferous layers of the Marshall sandstone are decidedly friable—insomuch that it is in little request for building purposes—and it would seem absurd to suppose that these large fragments had been moved two hundred miles from the northern outcrop of the formation, when a transfer of ten or fifteen miles from the southern outcrop would bring them to the position which they occupy. We should expect, also, if derived from the north, that some contrast in the organic facies, due to local, if not to climatic causes, would present itself; but on the contrary, we find the fauna of the fragments strictly identical with that of the nearest indigenous rocks of the same age.

The facts above cited recall some observations made several years since in Alabama, and which led at that time to impressions similar to those just set forth. My observations were made especially upon the neighborhood of the junction of the "Rotten Limestone" of the Upper Cretaceous, with the argillaceous and arenaceous strata of the Lower Cretaceous. The "Red Loam" of the central belt of the state, which I have evidences to prove to be but the Rotten Limestone altered *in situ*, or with slight transportation, has in many cases along the junction of the upper and lower strata, been moved northward over the clayey and sandy region appertaining to the lower Cretaceous; where, by its admixture with diluvial sand and pebbles, it is proved to be an exotic formation.

I forbear to express any belief in reference to the former existence of a transporting agency acting from the south to the north, over all or any portion of the interior of the continent; but such facts as I have cited cannot fail to call to mind the suggestions made some years since by President W. Hopkins, touching the course the Gulf Stream would necessarily pursue, in case of the subsidence of the North American Continent[4]. The facts are of sufficient importance to merit investigation; and it is to be hoped that other observers will inform us whether they are exceptional phenomena, or correspond, in connection with others, to some ancient, glacial or hydrographical area.

University of Michigan, August 4, 1865.

ART. XXXV.—*On the Crystallization of Sulphur and upon the Reaction between Sulphid of Hydrogen, Ammonia and Alcohol*; by CHARLES M. WETHERILL, Ph.D. M.D.

SULPHUR, in three of its four allotropic conditions, has been well studied, notwithstanding the difficulties which the rapid passage of (γ S) through (β S) to (α S) presents to experiments upon the first two modifications.

The most reliable specific gravities which have been assigned to the different forms of sulphur are the following:

	Marchand & Scheerer.	Deville.
Rhombic octohedral (α S)	2·045	2·07
Oblique prismatic (β S)	1 982	1·96
The red, amorphous (γ S)	1·957	1·91

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[4] Quar. Jour. Geol. Soc., viii, 56. Reprinted, Am. Jour. Sci. and Arts, [2], xv, 72.

Regnault found the specific heat of (α S) = 0·20259, and Marchand and Scheerer that of (β S) = 0·20684. During the passage of (γ S) to (α S), a considerable amount of heat is evolved.

Hence in the red amorphous variety, the molecules are more widely separated, and are in a condition of unstable equilibrium. In satisfying their tendency to approach each other, they assume (by the fusion method) the *beta* form of prisms of the monoclinic system; but they soon pass into the condition of rest as rhombic octohedra (α S), of the trimetric system. This transformation takes place, as is well known in the solid prism, which is, without change of form, converted into numerous smaller crystals of (α S).

According to Frankenheim, "*gamma*" sulphur, like other so-called amorphous bodies, possesses the property of crystallization; but the manifestation of the phenomenon is prevented by the admixture of alpha and beta sulphur. Heat is the agent by which the (α) form is converted into the (β) and (γ) modifications, and has always been supposed to play an important part in the crystallization of this element. The ordinary or alpha sulphur, when crystallized from its solvents, sulphid of carbon, or oil of turpentine, reappears as octohedral alpha sulphur.

Frankenheim, however, observed that when the body is precipitated from its solutions at a temperature approaching its point of fusion, it assumes the prismatic form of beta sulphur.

Mitscherlich has shown that crystals of (β S) are transformed *immediately* into (α S), when dipped into a solution of sulphur in sulphid of carbon.

According to Pasteur, both forms of the element may crystallize from the above solvent.

M. E. Royer determined the crystallization from a turpentine solution, either in α or β crystals, according to the temperature. Thus, when fifteen grams of sulphur were heated with two hundred and fifty grams of oil of turpentine, the solution boiled at 158° C. Upon pouring one-half of the liquid into a glass vessel, so that a rapid cooling was effected, prisms were deposited; while the remainder cooled slowly, yielded octohedrons, and not a single prism.

Such experiments would appear to present an analogous condition to the prismatic crystallization by fusion, and subsequent change to the alpha form by rest. By the elevated temperature the molecules of sulphur are separated, and assume the relative positions required for (β S); while, during the slow refrigeration, they have time to arrange themselves into the rhombic octohedral form, in which they are in stable equilibrium.

Each of the allotropic conditions has probably a corresponding vapor density and its own chemical affinities.

The only known vapor density is that of (γ S); the others not having been determined from their transformation to this modification by the heat required for the experiment.

Upon the assumption that sulphid of hydrogen has an atomic constitution analogous to that of water, the specific gravity of the vapor of the sulphur in the compound is (by calculation) 2·21126; but Dumas and Mitscherlich determined the density of the vapor of sulphur (γ S) itself to be three times this number, or 6·63378. It has hence been inferred that the hydrogen sulphid is not constituted like water; but is composed of one volume of hydrogen to one-sixth of a volume of sulphur.

If, however, we were acquainted with the true vapor density of (α S), it may be presumed that a calculation involving this element would establish an analogous constitution for sulphid of hydrogen and water, two bodies resembling each other so much in their chemical behavior.

Otto supposes that sulphur occurs in H S and in most of its combination as (α S), and that perhaps in the persulphids of potassium &c., and in the sulphur acids which contain two, three, and four atoms of that element, it exists in the condition of β or γ sulphur.

When sulphur is dissolved in the fat oils at a moderate temperature, a large portion of the element separates on cooling as a yellow powder (α S); but if the temperature be elevated, as in the preparation of *balsamum sulphuris*, the sulphur remains in solution, probably as (γ S), and a tenacious gelatinous mass is the result of the reaction (Otto).

This chemist infers that since the vapor density of (γ S) is *thrice* that of (α S), the specific gravity of the vapor of (β S) is probably *double* that of (α S). If this assumption be correct we have in sulphur an allotropism like the the polymerism of compound bodies.

From these considerations, the question of the crystal form of sulphur in the act of separation from any of its compounds becomes one of great interest.

One of these instances, the subject of this article, has been presented accidentally to my notice.

An experiment was instituted to ascertain whether the presence of copper in alkaline solution (ammonia oxyd of copper) would determine a decomposition of glucose different from that effected by the alkali alone. The ammonia copper was added to a boiling solution of glucose until the blue color of the former was no longer discharged. Upon cooling, alcohol was added and then sulphid of hydrogen was passed through the liquid to separate the copper. On the next morning the filtrate contained a quantity of beautiful prismatic crystals of sulphur, of which some exceeded an inch in length. When spread upon filter paper to dry they became opaque, and broke up readily into granules.

To ascertain whether the products of decomposition of the glucose effected the crystallization, a liter of 94 p. c. alcohol was, on July 4, 1864, saturated first with ammonia, then with sulphid of hydrogen, and was placed in a loosely stoppered bottle upon a shelf in the laboratory of the Smithsonian Institution, where it remained undisturbed until May 26, 1865.

At this time the sides of the bottle were coated with white, slightly nacreous crystals, which were more numerous upon the part of the bottle most exposed to the light. The formation of these crystals commenced upon the day after the saturation with the hydrogen sulphid. The solution was of brownish deep red color. The crystals measured ⅛th of an inch in diameter, and, under the microscope, had the appearance of scales or plates, with rounded, ill-defined contour. A few appeared to be hexagonal and a very few were of spear shape. They were all completely soluble in water, more slowly so in alcohol, yielding a solution of faint acid reaction. They lost ammonia upon being heated with caustic potassa. Their solution treated with sulphuric acid evolved sulphurous acid and deposited sulphur. Heated upon platina foil they evaporated without previous fusion. Their taste was sharp and biting. Heated in a tube they yielded a deliquescent sublimate which contained sulphur. Their solution did not blacken acetate of lead, but gave a white precipitate with this reagent. With nitrate of baryta a white precipitate, insoluble in hydrochloric acid, fell after a short time. With nitrate of silver a precipitate was obtained which passed gradually from white through yellow and brown to black. It follows from these reactions that the crystals are hyposulphite of ammonia. Rammelsberg gives for the constitution of this salt the formula $3(NH_4O, S_2O_2,)+HO$.

If the experiments had rested at this point, the erroneous inference would have been drawn from them that the presence of organic matter (from the glucose,) occasioned the prismatic crystallization of sulphur; but the solution in the last example was filtered to remove the crystals; and the clear liquid was returned to the bottle. The crystals were washed upon the filter with a little alcohol, which was suffered to drop into the bottle and formed a stratum of a few lines in depth upon the surface of the dark colored solution.

On the next day a beautiful crystallization of sulphur in *prisms* was observed, and its advance was watched for several days. The crystals formed upon the sides of the vessel at the surface of the liquid, extended inwards, and developed themselves downward. In proportion as they grew, the liquid exchanged its dark red for a light amber color until the whole of the solution had acquired that tinge, the advance being from the surface, downward. The prisms were very slender, and terminated in planes oblique to the axis of the crystal. Not a single octahedron was detected. By their insolubility in water, and by heating upon platina foil they were proved beyond a doubt to be sulphur. Some of the crystals measured over two inches in length; they were at first transparent, and of a canary yellow color.

On the second day after their appearance they had lost their transparency, and the microscope showed them to be full of fissures. Very minute crystals were formed upon the prisms and upon the sides of the bottle. Those occupying the latter position were magnified with a high power, and appeared to be bushes or star-shaped groups of *prismatic* crystals, radiating from a central *globular* and minute molecule. On the third day the latter crystals had become sufficiently developed to show that they were not prisms but rhombic octahedra, and that the crystals strung along the *primary prisms* like beads, were also rhombic octahedra. These crystals were all *truncated* as to their apices by planes perpendicular to the principal axes. The similar faces (O) of the lines of octahedra situated along the primary prisms were contained in parallel planes. These octahedra did not appear to have resulted from a breaking up of the primary prisms; but to be *deposited* upon the latter. I inferred that the prisms were at first (β S), in which a change to multitudes of octahedral crystals of (α S), contained within the prism, was effected; and that the contact of this group of (α S) crystals with the sulphur separating gradually from its solution determined the crystallization, *in the neighborhood of the altered prisms*, according to the alpha modification. It was remarkable that while this octahedral crystallization was going on, *transparent prismatic* crystals were forming in other parts of the solution, and I supposed at first that the *prisms* might belong to the trimetric system; but long observation of them assured me that they were monoclinic.

On June 4th the separation of the sulphur was complete from the liquid, which was of amber color. At this time other crystals in the form of transparent scales were forming. These were ascertained, by the taste and action of reagents, to be hyposulphite of ammonia. There was nothing new in the crystallization of the sulphur except the very instructive fact that where a prism had fallen from the side of the vessel to the bottom, the sulphur had crystallized in beautiful, well defined rhombic octahedra, with perfect apices, and which were as clear and colorless as rock crystal. They were arranged in rows, thus indicating the former positions of the prisms upon the bottle.

All of these crystals were first treated with water to dissolve the hyposulphite of ammonia. When this was performed under the microscope, the transparent scales were seen to disappear, and there remained octahedrons and prisms, some of which had imperfect octahedral crystals deposited upon them. The prisms appeared granular and were of canary color; the octahedrons were almost white. When heated, and observed thus with the microscope, the crystals became of deep yellow color, and by elevating the temperature fused, yielding all of the characters of sulphur. It would seem to result from these experiments that sulphur exists in combination with hydrogen, (or perhaps with ammonium), in the B allotropic condition. We may readily conceive a change in the sulphur after it has been separated from the β to the α modification; but we cannot so readily imagine that it should leave its *alpha* form to assume the more unstable *beta* condition.

We have also here an example of the crystallization of (β S) *without heat.* Since the original prisms of *beta* sulphur are canary colored, and not of brownish tinge, it would seem that the prisms obtained by fusion owe their brown tint to *p* sulphur. The hypothesis may be ventured that the color of (α S) is white or very pale yellow, that of (β S) yellow, that of (γ S) reddish brown and that of (δ S) nearly black.

In the reaction between sulphid of hydrogen in the alcoholic solution of ammonium, sulphid of ammonium, distinguished by its odor, is at first formed; the smell is gradually exchanged for a strong and persistent one of sulphid of ethyl until at least no ammonium sulphid can be, by the proper reagents, detected in the liquid. The sulphur is then contained in the clear solution partly as hyposulphite and in part as free sulphur dissolved, or in some manner kept from separating by the hyposulphite.[1] It is to this dissolved sulphur that the liquid owes its dark tinge, which depth of color came on gradually as the sulphid of ammonium was oxydized. When the solution lost a portion of its hyposulphite by crystallization, the free sulphur separated and the liquid lost its dark color. Then another portion of hyposulphite of ammonia appeared in crystals.

As the experiment has been set aside for further examination by slow crystallization, I am unable at present to say whether any sulphite of ammonia has been formed. The odor of free ammonia is very powerful, and the liquid contains sulphite of ethyl. A drop evaporated in the air upon a glass slide exhibits microscopic oil globules, having a strong smell of the sulphid of ethyl. The same globules may be absorbed by agitating the liquid with ether, precipitating by water, and suffering the ethereal solution to evaporate spontaneously in a watch glass.

The liquid also contains hyposulphite of ammonia in solution, and there are no crystals in it at present.

The following formula may illustrate its reaction:

$$3NH_4S+5O=NH_4O,S_2O_2+2NH_4O+S;$$

and for the sulphid of ethyl, perhaps,

$$NH_4S+C_4H_5O,HO=NH_4O,HO+C_4H_5S.$$

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[1] Are γ S or β S soluble in the hyposulphite?

I cannot say whether mercaptan was formed; the odor appeared to be different from that of this alcohol, and there was no action by the solution on oxyd of mercury. I have regarded the formation of the ethyl sulphid as without influence upon the peculiar crystallization of sulphur here described. It may however be otherwise, for MM. Dietzenbacher and Moutier (C. R., lx, 353) have recently discovered that naphthaline, camphor, creosote, oil of turpentine, carbon, &c., modify the condition of sulphur rendering it soft and plastic, as well as partially insoluble in sulphid of carbon.

ART. XXXVI.—*On the History of Eozoön Canadense.*

WITH A PLATE.

IN volume xxxvii of this Journal, at p. 272, will be found a note to one of the Editors from Sir W. E. Logan, dated Feb. 17, 1864, announcing the discovery in the Laurentian limestones of Canada of organic remains, which, after a careful microscopic examination had been pronounced by Dr. Dawson, to belong to a gigantic Rhizopod. On page 431 of the same volume is a further note by Dr. T. Sterry Hunt, dated April 2, 1864, in which he states that in these organic remains the calcareous skeleton remains unchanged, while the sarcode is replaced by certain silicates, pyroxene, serpentine, and a mineral allied to chlorite in composition; the minute tubuli of the fossil being injected by these silicates in a manner analogous to that presented by the tertiary and modern Rhizopods, which are filled with glauconite.

These were the first announcements of this remarkable discovery, which has since been zealously followed up. Specimens of this fossil, which Dr. Dawson has named *Eozoön Canadense*, have been submitted to Dr. W. B. Carpenter of London, who is well known to be the great authority on Rhizopods, and the result of his examinations has been to confirm completely the determinations of Dr. Dawson, and to add to the description by the latter some further structural details, which the more perfect specimens examined by Dr. Carpenter enabled him to detect.

The results of the various investigations of the Eozoön are found in four papers published in the Journal of the Geological Society of London, for February, 1865. The first of these by Sir W. E. Logan describes the general geological relations of the Laurentian rocks, and is accompanied by two sections which we reproduce below.

He also relates the history of the first discovery of the fossil, specimens of which were exhibited by him to the American Association, at Springfield in August, 1859—and were then regarded by him and by Prof. James Hall as organic, although the microscope had as yet failed to detect the beautiful structure since found in such perfection in the specimens from other localities.

Sir William Logan moreover recalls the fact that Dr. Hunt had already from the presence of iron-ore beds, graphite, and metallic sulphurets, argued for the existence of life during the formation of the Laurentian rocks. See on this point a paper read in Jan. 1859 before the Geol. Society of London, and published in the Journal of that society, of which a review will be found in this Journal, [2], xxx, 134. This view is further developed in Dr. Hunt's paper on American Geology in this Journal, [2], xxxi, 396, where he concludes on chemical grounds to "the existence of an abundant vegetation during the Laurentian period."

The second paper is by Dr. Dawson, and gives his zoological description of Eozoön and its affinities, with a lithographed plate. The third is an extended note by Dr. Carpenter, in which he fully confirms the sagacious determination of Dr. Dawson as to the rhizopod characters and foraminiferal affinities of Eozoön, and is illustrated with a wood-cut and ten lithographed plates. In the fourth place we have a paper on the mineralogy of Eozoön by Dr. T. Sterry Hunt.

These four papers are reprinted in the Canadian Naturalist for April, 1865, with a single lithographed plate containing selections from the three just mentioned, and with the addition of a nature-printed section of Eozoön, both of which we place before our readers, together with extracts of the papers.

The Eozoön has also been carefully studied by Prof. T. Rupert Jones, who in the Popular Science Review for April, 1865, has given an excellent paper on the geological and zoological relations of the new fossil, together with a colored plate. In addition to this the Intellectual Observer for May, 1865, contains an essay of twenty-four pages on the same subject by Dr. Carpenter, with two excellent plates. As a further contribution to the literature of Eozoön we may mention that the pages of the London Reader for June, contain a correspondence between Dr. Carpenter and Messrs. King & Rowney, of Galway, who venture to question the opinion of Messrs. Dawson, Carpenter and Rupert Jones as to the organic nature of Eozoön. This correspondence is chiefly interesting as giving from Dr. Carpenter the authorized announcement by Milne-Edwards, that he, after a careful study, fully concurs in the views of the latter named observers as to the structure and affinities of Eozoön.

We now proceed to notice Sir William Logan's description of the Laurentian rocks, the general facts in whose history are briefly given in a paper by Dr. T. Sterry Hunt in this Journal, [2], xxxvi, 222.

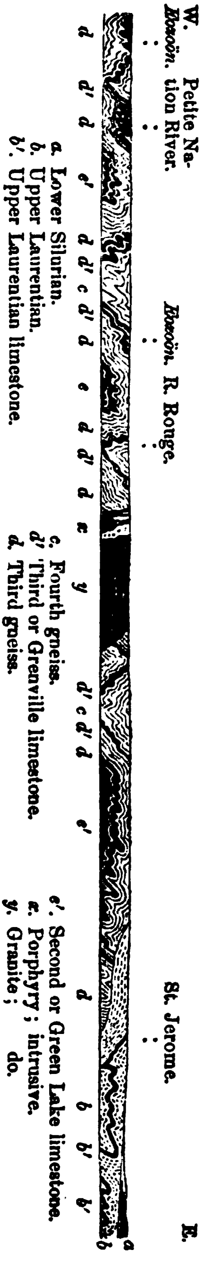
"The oldest known rocks of North America are those which compose the Laurentide Mountains in Canada and the Adirondacks in the state of New York. By the investigations of the Geological Survey of Canada, they have been shown to be a great series of strata, which, though profoundly altered, consist chiefly of quartzose, aluminous, and calcareous rocks, like the sedimentary deposits of less ancient times. This great mass of crystalline rocks is divided into two groups, and it appears that the Upper Laurentian or Labrador series rests unconformably upon the Lower Laurentian series. The united thickness of the two groups in Canada cannot be less than 30,000 feet, and probably much exceeds it. The Laurentian of the west of Scotland, also according to Sir Roderick Murchison, attains a great thickness." In that region the Labrador series had not until recently been certainly recognized, although from the descriptions of Mcculloch, and from an examination of the specimens collected by him, and now in the Museum of the Geological Society in London, Dr. Hunt in 1863, had expressed the opinion that the rocks of Skye belong to this series. (See as above, page 226.) Prof. Haughton of Dublin has since visited the islands of Iona and Skye, and confirmed the observations of Dr. Hunt. (Geol. Magazine, Feb. 1865, page 73.)

The labradorite and hypersthene rocks from that island are identical with those of the Labrador series in Canada and New York, and unlike those of any formation at any other known horizon. This resemblance did not escape the notice of Emmons, who, in his description of the Adirondack Mountains, referred these rocks to the hypersthene-rock of McCulloch, although these observers, on the opposite sides of the Atlantic, looked upon them as unstratified.

Sir William then proceeds to notice the ancient gneiss which in Bavaria and Bohemia underlies the primordial zone, with a great intervening mass of clay slates. This old gneiss, which is estimated by Gümbel and Crejci to have a thickness of 90,000 feet, may, as Sir William suggests, include both the Upper and Lower Laurentian, and perhaps the Huronian, which is believed to be more recent than the Labrador series, although the two groups have never yet been found in contact. He then proceeds to remark:

"The united thickness of these three great series may possibly far surpass that of all the succeeding rocks from the base of the Palæozoic series to the present time. We are thus carried back to a period so far remote, that the appearance of the so-called Primordial fauna may by some be considered a comparatively modern event. We however find that even during the Laurentian period, the same chemical and mechanical processes which have ever since been at work disintegrating and recon-

Fig. 1.—*Section from Petite Nation Seigniory to St. Jerome* (66 miles).



a. Lower Silurian.
b. Upper Laurentian.
b'. Upper Laurentian limestone.
c. Fourth gneiss.
d'. Third or Grenville limestone.
d. Third gneiss.
e'. Second or Green Lake limestone.
x. Porphyry; intrusive.
y. Granite; do.

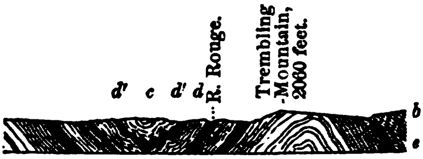
structing the earth's crust were in operation as now. In the conglomerates of the Huronian series there are enclosed boulders derived from the Laurentian, which seem to show that the parent rock was altered to its present crystalline condition before the deposit of the newer formation; while interstratified with the Laurentian limestones there are beds of conglomerate, the pebbles of which are themselves rolled fragments of an older laminated sand-rock, and the formation of these beds leads us still further into the past."

"In both the Upper and Lower Laurentian series there are several zones of limestone, each of sufficient volume to constitute an independent formation. Of these calcareous masses it has been ascertained that three, at least, belong to the Lower Laurentian. But as we do not as yet know with certainty either the base or the summit of this series, these three may be conformably followed by many more. Although the Lower and Upper Laurentian rocks spread over more than 200,000 square miles in Canada, only about 1500 square miles have yet been fully and connectedly examined in any one district; and it is still impossible to say whether the numerous exposures of Laurentian limestone met with in other parts of the province are equivalent to any of the three zones, or whether they overlie or underlie them all."

In this connection are given the two following sections, which serve to show the structure of the Lower Laurentian rocks, and their relation to the overlying Labrador and Lower Silurian series.

The Eozoön Canadense occurs at Grenville, and in the Petite Nation seigniory at the two points indicated in the first section, and in both places in the third or uppermost band of the Lower Laurentian limestone. The same fossil has also been obtained to the south of the Ottawa, in Burgess, and farther west at the Grand Calumet on the Ottawa, in both instances in a limestone band whose precise place in the series has not been determined, nor is it known whether the fossil extends to the two lower conformable limestone bands, or to the calcareous zones in the unconformable Upper Laurentian series.

Fig. 2.—*Section across Trembling Mountain* (21 miles).

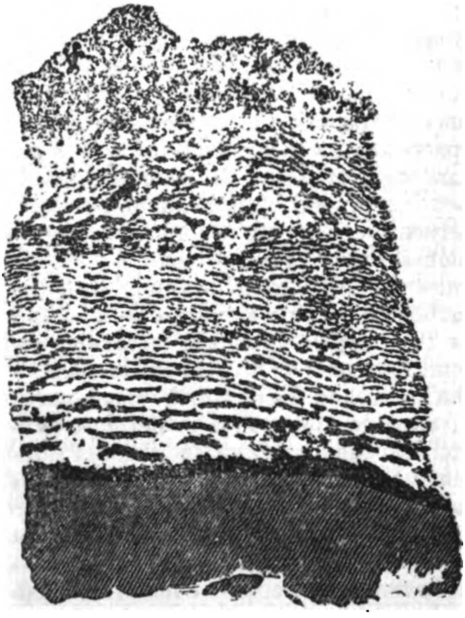


b. Upper Laurentian.
c. Fourth gneiss.
d'. Third limestone.
d. Third gneiss.
e'. Second limestone.
e. Second gneiss.
f'. First limestone.
f. First gneiss.

"The Grenville zone of limestone is in some places about 1500 feet thick, and it appears to be divided for considerable distances into two or three parts by very thick bands of gneiss. One of these bands occupies a position toward the lower part of the limestone, and may have a volume of between 100 and 200 feet. It is at the base of this limestone formation that the fossil occurs. This part of the zone is largely composed of great and small irregular masses of white crystalline pyroxene, some of them twenty yards in length by four or five wide. They appear to be confusedly placed one above another, with many ragged interstices, and smoothly-worn, rounded, large and small pits and sub-cylindrical cavities, some of them pretty deep. The pyroxene, though it appears compact, presents a multitude of small spaces filled with carbonate of lime, and many of these show minute structure similar to that of the fossil. These masses of pyroxene may characterize a thickness of about 200 feet, and the interspaces among them are filled with a mixture of serpentine and carbonate of lime. In general a sheet of pure dark green serpentine invests each mass of pyroxene; the thickness of the serpentine, varying from the sixteenth of an inch to several inches, rarely exceeding half a foot. This is followed in different spots by parallel waving, irregularly alternating plates of carbonate of lime and serpentine, which become gradually finer as they recede from the pyroxene, and occasionally occupy a total thickness of five or six inches. These portions constitute the unbroken fossil, which may sometimes spread over an area of about a square foot, or perhaps more. Other parts, immediately on the outside of the sheet of serpentine, are occupied with about the same thickness of what appear to be the ruins of the fossil, broken up into a more or less granular mixture of calcspar and serpentine, the former still showing minute structure; and on the outside of the whole a similar mixture appears to have been swept by currents and eddies into rudely parallel and curving layers; the mixture becoming gradually more calcareous as it recedes from the pyroxene. Sometimes beds of limestone of several feet in thickness, with the green serpentine more or less aggregated into layers, and studded with isolated lumps of pyroxene, are irregularly interstratified in the mass of rock; and less frequently there are met with lenticular patches of sandstone or granular quartzite, of a foot in thickness and several yards in diameter, holding in abundance small disseminated leaves of graphite."

"The general character of the mass produces the impression that it is a great Foraminiferal reef, in which the pyroxenic masses represent a more ancient portion, which having died, and having become much broken up, and worn into cavities and deep recesses, afforded a seat for a new growth of *Foraminifera*, represented by the calcareo-serpentinous part. This in its turn became broken up, leaving in some places uninjured portions of the general form. The main difference between this Foraminiferal reef and more recent coral-reefs seems to be that, while with the latter are usually associated marine shells and other organic remains, in the more ancient ones the only remains yet found are those of the animal which built the reef."

The structure and appearance of the unbroken fossil will be understood from the following nature-printed section, the preparation of which is thus described by Dr. Hunt.



3. Nature-printed section of a specimen of ***Eozoön Canadense*** from Petite Nation Seigniory.

" The replacing mineral in this specimen being serpentine, the calcareous septa were dissolved from the polished surface by the action of an acid, and the fine material replacing the tubuli having been removed by the aid of a brush, a wax mould of the etched surface furnished the electrotype cast from which the above figure is printed. The lights thus represent the calcareous skeleton, and the shaded portion a thick mass of serpentine, which is distinguishable from a contiguous thin stratum of the same mineral, that seems to form the base of the Eozoön. The gradual passage from the wide chambers and thick septa to the narrower and thinner ones, and finally to the irregularly aggregated mode of growth, designated by Dr. Carpenter as *acervuline*, is well seen. The white patches in the upper portion of the figure do not arise from any imperfection in the electrotype, but represent the irregular growth of this part of the calcareous skeleton."

Slices of the fossils having been prepared for microscopic examination, and submitted to Dr. Dawson, were at once recognized by him as presenting the characters of Foraminiferal shells. After a careful examination of a large number of specimens he named and described the fossil as follows.

"EOZOÖN CANADENSE; *gen. et. spec. nov.*

General form.—Massive, in large sessile patches or irregular cylinders, growing at the surface by the addition of successive laminæ.

Internal structure.—Chambers large, flattened, irregular, with numerous rounded extensions, and separated by walls of variable thickness, which are penetrated by septal orifices irregularly disposed. Thicker parts of the walls with bundles of fine branching tubuli."

The grounds on which he inferred its foraminiferal character are thus stated by Dr. Dawson:

"1. The serpentine and pyroxene which fill the cavities of the calcareous matter have no appearance of concretionary structure. On the contrary, their aspect is that of matter introduced by infiltration, or as sediment, and filling spaces previously existing. In other words, the calcareous matter has not been moulded on the forms of the serpentine and augite, but these have filled spaces or chambers in a hard calcareous mass. This conclusion is further confirmed by the fact, to be refered to in the sequel, that the serpentine includes multitudes of minute foreign bodies, while the calcareous matter is uniform and homogeneous. It is also to be observed that small veins of carbonate of lime occasionally traverse the specimens, and in their entire absence of structures other than crystalline, present a striking contrast to the supposed fossils."

"2. Though the calcareous laminæ have in places a crystalline cleavage, their forms and structures have no relation to this. Their cells and canals are rounded, and have smooth walls, which are occasionally lined with films apparently of carbonaceous matter. Above all, the minute tubuli are different from anything likely to occur in merely crystalline calcspar. While in such rocks little importance might be attached to external forms simulating the appearances of corals, sponges, or other organisms, these delicate internal structures have a much higher claim to attention. Nor is there any improbability in the preservation of such minute parts in rocks so highly crystalline, since it is a circumstance of frequent occurrence in the microscopic examination of fossils that the finest structures are visible in specimens in which the general form and the arrangement of parts have been entirely obliterated. It is also to be observed that the structure of the calcareous laminæ is the same, whether the intervening spaces are filled with serpentine or with pyroxene."

"3. The structures above described are not merely definite and uniform, but they are of a kind proper to animal organisms, and more especially to one particular type of animal life, as likely as any other to occur under such circumstances; I refer to that of the Rhizopods of the order *Foraminifera.* The most important point of difference is in the great size and compact habit of growth of the specimens in question; but there seems no good reason to maintain that *Foraminifera* must necessarily be of small size, more especially since forms of considerable magnitude referred to this type are known in the Lower Silurian. Prof. Hall has described specimens of *Receptaculites* twelve inches in diameter; and the fossils from the Potsdam formation of Labrador, referred by Mr. Billings to the genus *Archœocyathus*, are examples of *Protozoa* with calcareous skeletons scarcely inferior in their massive style of growth to the forms now under consideration."

"These reasons are, I think, sufficient to justify me in regarding these remarkable structures as truly organic, and in searching for their nearest allies among the *Foraminifera.*"

"Supposing then that the spaces between the calcareous laminæ, as well as the canals and tubuli traversing their substance, were once filled with the sarcode-body of a Rhizopod, comparisons with modern forms at once suggest themselves."

"From the polished specimens in the Museum of the Canadian Geological Survey, it appears certain that these bodies were sessile by a broad base, and grew by the addition of successive layers of chambers, separated by calcareous laminæ, but communicating with each other by canals or septal orifices sparsely and irregularly distributed. Small specimens have thus much the aspect of the modern genera *Carpenteria* and *Polytrema.* Like the first of these genera, there would also seem to have been a tendency to leave in the midst of the structure a large central canal, or deep funnel-shaped or cylindrical opening, for communication with the sea-water. Where the laminæ coalesce, and the structure becomes more vesicular, it assumes the 'acervuline' character seen in such modern forms as *Nubecularia.*"

"Still the magnitude of these fossils is enormous when compared with the species of the genera above named; and from the specimens in the larger slabs from Grenville, in the Museum of the Canadian Survey, it would seem that these organisms grew in groups, which ultimately coalesced, and formed large masses penetrated by deep irregular canals; and that they continued to grow at the surface, while the lower parts became dead, and were filled up with infiltrated matter or sediment. In short, we have to imagine an organism having the habit of growth of *Carpenteria*, but attaining to an enormous size, and by the aggregation of individuals assuming the aspect of a coral reef."

"The complicated systems of tubuli in the the Laurentian fossil indicate however a more complex structure than that of any of the forms mentioned above. I have carefully compared these with the similar structures in the 'supplementary skeleton' (or the shell-substance that carries the vascular system) of *Calcarina* and other forms, and can detect no difference except in the somewhat coarser texture of the tubuli in the Laurentian specimens. It accords well with the great dimensions of these, that they should thus thicken their walls with an extensive deposit of tubulated calcareous matter; and from the frequency of the bundles of tubuli, as well as from the thickness of the partitions, I have no doubt that all the successive walls, as they were formed, were thickened in this manner, just as in so many of the higher genera of more modern *Foraminifera.*"

Dr. Dawson further ascertained that certain parts of the fossil present that irregular mode of growth which Dr. Carpenter has designated as acervuline, and moreover that considerable masses of Laurentian limestones are made up of fragments of the fossils. He further points out the presence in these limestones of other fragments, which are probably organic, and which may indicate the existence of other animal remains. Films of carbonaceous matter in some of these limestones also exhibit under the microscope forms which indicate that they may be the remains of plants. He further adds, that "although the abundance and wide distribution of *Eozoön*, and the important part it seems to have acted in the accumulation of limestone, indicate that it was one of the most prevalent forms of animal existence in the seas of the Laurentian period, the non-existence of other organic beings is not implied. On the contrary, independently of the indications afforded by the limestones themselves, it is evident that in order to the existence and growth of these large Rhizopods, the waters must have swarmed with more minute animal or vegetable organisms on which they could subsist. On the other hand, though this is a less certain inference, the dense calcareous skeleton of *Eozoön* may indicate that it also was liable to the attacks of animal enemies. It is also possible that the growth of *Eozoön*, or the deposition of the serpentine and pyroxene in which its remains have been preserved, or both, may have been connected with certain oceanic depths and conditions, and that we have as yet revealed to us the life of only certain stations in the Laurentian seas."

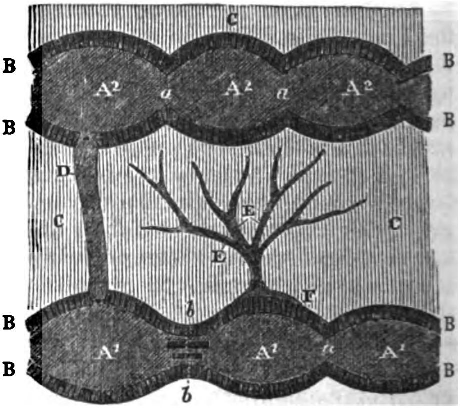
Subsequent to the above examination and description by Dr. Dawson, specimens of the new fossil from Grenville, and also more perfect ones from a similar and newly discovered locality in the Petite Nation Seigniory were taken to London by Sir William Logan and submitted to Dr. Carpenter, who fully confirmed the above results, and made out some additional structure. More especially he observed in specimens from the new locality, the preservation of the fine tubulation of the original cell-wall, which had not before been clearly made out. The additional points are thus stated by Dr. Carpenter:

"Although Dr. Dawson has noticed that some parts of the sections which he examined present the fine tubulation characteristic of the shells of the Nummuline Foraminifera, he does not seem to have recognized the fact, which the sections placed in my hands have enabled me most satisfactorily to determine,—that the proper walls of the chambers everywhere present the fine tubulation of the Nummuline shell (plate, figs. 3, 6); a point of the highest importance in the determination of the affinities of *Eozoön*. This tubulation, although not seen with the clearness with which it is to be discerned in recent examples of the Nummuline type, is here far better displayed than it is in the majority of fossil Nummulites, in which the tubuli have been filled up by the infiltration of calcareous matter, rendering the shell-substance nearly homogeneous. In *Eozoön* these tubuli have been filled up by the infiltration of a mineral different from that of which the shell is composed, and therefore not coalescing with it; and the tubular structure is consequently much more satisfactorily distinguishable. In decalcified specimens, the free margins of the casts of the chambers are often seen to be bordered with a delicate white glistening fringe; and when this fringe is examined with a sufficient magnifying power, it is seen to be made up of a multitude of extremely delicate *aciculi*, standing side by side like the fibres of asbestos. These, it is obvious, are the internal casts of the fine tubuli which perforated the proper wall of the chambers, passing directly from its inner to its outer surface; and their presence in this situation affords the most satisfactory confirmation of the evidence of that tubulation afforded by thin sections of the shell-wall."

"The successive layers, each having its own proper wall, are often superposed one upon another without the intervention of any supplemental or intermediate skeleton such as presents itself in all the more massive forms of the Nummuline series; but a deposit of this form of shell-substance, readily distinguishable by its homogeneousness from the finely tubular shell immediately investing the segments of the sarcode-body, is the source of the great thickening which the calcareous zones often present in vertical sections of *Eozoön.* The presence of this intermediate skeleton has been correctly indicated by Dr. Dawson; but he does not seem to have clearly differentiated it from the proper wall of the chambers. All the tubuli which he has described belong to that canal-system which, as I have shown, is limited in its distribution to the intermediate skeleton, and is expressly destined to supply a channel for its nutrition and augmentation. Of this canal-system, which presents most remarkable varieties in dimensions and distribution, we learn more from the casts presented by decalcified specimens, than from sections, which only exhibit such parts of it as their plane may happen to traverse."

"It does not appear to me that the 'canal-system' takes its origin directly from the cavity of the chambers. On the contrary, I believe that, as in *Calcarina* (which Dr. Dawson has correctly referred to as presenting the nearest parallel to it among recent *Foraminifera*), they originate in lacunar spaces on the outside of the proper walls of the chambers, into which the tubuli of those walls open externally; and that the extensions of the sarcode-body which occupied them were formed by the coalescence of the pseudopodia issuing from those tubuli."

We have here a diagram by Dr. Carpenter which he has given in his paper in the Quarterly Geological Journal; and from his subsequent memoir in the Intellectual Observer extract the following remarks: It will be understood that not only the chamber, but the canals and minute tubuli in the fossil are injected with serpentine, which, when the calcareous skeleton is removed by an acid, remains, and Dr. Carpenter especially notices "the marvellous completeness with which the minutest extensions of the sarcode-body of the animal are represented in decalcified specimens by these serpentine models; even



4. *Diagram illustrating the structure of Eozoön.*

A^1, A^1, A^1. Three chambers of one layer, communicating with each other directly at *a*, and by three passages through a shelly partition at *b*.

A^2, A^2, A^2. Three chambers of a more superficial layer.

B, B, B. Proper wall of the chambers, composed of finely tubular shell-substance.

C, C, C. Intermediate or supplemental skeleton, traversed by D, a stolon of communication between two chambers of different layers, and by E, E, a canal system originating in the lacunar space F.

the most delicate pseudopodial threads, consisting of the softest and most transitory form of living substance which were put forth through pores in the shell-wall less than one ten-thousandth of an inch in diameter, being thus perpetuated to all time." "The proper walls of the chambers are everywhere formed of a pellucid vitreous shell-substance minutely perforated with tubuli, so as exactly to correspond with those of *Nummulites*, *Operculina*, etc." The serpentine casts of these tubuli are frequently detached by the disengagement of the gas while the skeleton is being removed by an acid, but they often remain behind, standing side by side like the filaments which form the pile of velvet, their lower ends resting on the subjacent segment, while their upper extremities present a uniform surface. These casts of tubuli are distinguished from the adjacent serpentine, which is pale-green by their whiteness, yet the two are found by Dr. Hunt to be identical in composition, and thus, as he, and Dr. Carpenter after him, remarks, their whiteness is due to their fine division; many groups and bunches of this white substance being found by Dr. Carpenter to be aggregations of the elementary forms of sarcodic prolongation, which he has described in detail.

With regard to the intermediate or supplemental skeleton, which resembles closely that existing in *Calcarina* as described by Dr. Carpenter in his admirable Introduction to the Study of the Foraminifera, it is an exogenous deposit on the outer surface of the proper walls of the chamber, where it seems to be produced by the sarcodic layer which is formed by the coalescence of the pseudopodia after they have issued from the tubuli, and is traversed by a more or less minutely distributed canal-system, occupied during life by prolongations of that sarcodic layer. In those portions of the fossil where the chambers, instead of being regularly arranged in floors, are piled up in the *acervuline* manner, there is little or no trace of this intermediate skeleton, but in these irregularly aggregated chambers the structure of their proper walls is still well seen, both in transparent sections and in decalcified specimens.

In the solid masses of limestone which are made up of fragments of Eozoön, Dr. Carpenter could find no trace of the intermediate skeleton, though these specimens afford the most perfect examples of the Nummuline tubulation. He hence concludes that "the breaking up of the surface of the original Eozoön must have taken place before the proper walls of its highest tiers of chambers had been strengthened by exogenous deposit."

Space will not permit us to follow Dr. Carpenter in his comparison of Eozoön with other Foraminifera. He remarks, however, the resemblance in its mode of growth with the discoidal modern *Cycloclypeus* from the coast of Borneo, which attains a diameter of more than two inches and is made up of many thousand segments, while in Globigerina the continuous increase of the individual body by segments soon ceases and new individuals are formed by the separation of the segments.

Reverting to Dr. Dawson's remark as to the important part played by the Eozoön in the Laurentian seas, Dr. Carpenter observes the significance of the fact that this lowest type of animal life known to the physiologist (the Rhizopod) should have attained such a great development and apparently culminated in the very earliest known period in the history of the life of our globe.

The serpentine marbles of Tyree and of Skye, whose probable Laurentian age had been pointed out by Dr. Hunt in the paper already cited, offer, according to Dr. Carpenter, "a structure clearly identical with that of the Canadian Eozoön." A like structure has been discovered by Mr. Sandford in the serpentine marble of Connemara, known as *Irish green* marble. "I have," says Dr. Carpenter, "examined decalcified specimens of several portions of this rock, and have not the smallest hesitation in identifying them with the acervuline portion of the *Eozoön Canadense*, although I have met with nothing corresponding to the lamellated structure of other portions of the Eozoön. Moreover in place of the continuous asbestiform layer covering the segments, long straight bundles of filaments radiate from them." The age of the Connemara rock is by no means certain. Sir Roderick Murchison was at first disposed to regard it as Laurentian, but has since expressed the opinion that it is of Silurian age.

In this connection we call the attention of American geologists and microscopists to the crystalline limestones of the Highlands of New York, which are doubtless of Laurentian age,[1] and also to the figure and description, given by Dr. Emmons on page 60 of his Geology of the 2nd district of New York, of a banded arrangement in rounded or oval masses of serpentine and carbonate of lime, much resembling in external form the Eozoön of Canada. The specimen figured by him is from Warrensburg, Warren County, New York.

The mode in which the Eozoön has been preserved by the injection of serpentine has already been noticed, but is further described in Dr. Hunt's paper, from which we make the following extracts:

"The details of structure have been preserved by the introduction of certain mineral silicates, which have not only filled up the chambers, cells, and canals left vacant by the disappearance of the animal matter, but have in very many cases been injected into the tubuli, filling even their smallest ramifications. These silicates have thus taken the place of the original sarcode, while the calcareous septa remain. It will then be understood that when the replacement of the Eozoön by silicates is spoken of, this is to be understood of the soft parts only; since the calcareous skeleton is preserved in most cases without any alteration. The vacant spaces left by the decay of the sarcode may be supposed to have been filled by a process of infiltration, in which the silicates were deposited from solution in water, like the silica which fills up the pores of wood in the process of silicification. The replacing silicates, so far as yet observed, are a white pyroxene, a pale-green serpentine, and a dark-green alumino-magnesian mineral, which is allied in composition to chlorite and to pyrosclerite, and which I have referred to loganite. The calcareous septa in the last case are found to be dolomitic, but in the other instances are nearly pure carbonate of lime. The relations of the carbonate and the silicates are well seen in thin sections under the microscope, especially by polarized light. The calcite, dolomite, and pyroxene exhibit their crystalline structure to the unaided eye; and the serpentine and loganite are also seen to be crystalline when examined with the microscope. When portions of the fossil are submitted to the action of an acid, the carbonate of lime is dissolved, and a coherent mass of serpentine is obtained, which is a perfect cast of the soft parts of the Eozoön. The form of the sarcode which filled the chambers and cells is beautifully shown, as well as the connecting canals and the groups of tubuli; these latter are seen in great perfection upon surfaces from which the carbonate of lime has been partially dissolved. Their preservation is generally most complete when the replacing mineral is serpentine, although very perfect specimens are sometimes found in pyroxene. The crystallization of the latter mineral appears, however, in most cases to have disturbed the calcareous septa."

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[1] See this Journal, [2], xxxix, 97.

"Serpentine and pyroxene are generally associated in these specimens, as if their deposition had marked different stages of a continuous process. At the Calumet, one specimen of the fossil exhibits the whole of the sarcode replaced by serpentine; while, in another one from the same locality, a layer of pale green translucent serpentine occurs in immediate contact with the white pyroxene. The calcareous septa in this specimen are very thin, and are transverse to the plane of contact of the two minerals; yet they are seen to traverse both the pyroxene and the serpentine without any interruption or change. Some sections exhibit these two minerals filling adjacent cells, or even portions of the same cell, a clear line of division being visible between them. In the specimens from Grenville, on the other hand, it would seem as if the development of the Eozoön (considerable masses of which were replaced by pyroxene) had been interrupted, and that a second growth of the amimal, which was replaced by serpentine, had taken place upon the older masses, filling up their interstices."

The paper of Dr. Hunt contains numerous analyses of the various minerals noticed above. We extract his account of the loganite, which replaces the Eozoön from Burgess. The calcareous septa in these specimens are represented by a "somewhat ferriferous dolomite, the analysis of which was made upon portions mechanically separated from the enclosed silicate; it yielded carbonate of magnesia 40·7, carbonate of lime, with a little peroxyd of iron, 59·0=99·7. The septa of the specimen from this locality are in some parts more than 3·0 millimeters in thickness, and exhibit the chambers, cells and septal orifices; but no tubuli are seen. The replacing material has the hardness of serpentine, for which it was at first mistaken. Its color is blackish-green, but olive-green in thin sections, when it is seen by transmitted light to be crystalline in texture. Its fracture is granular, and its lustre feebly shining. It is decomposed by heated sulphuric acid, and was thus analyzed, yielding the result I. The centesimal composition of the soluble portion is given under II.

	I.	II.	III.
Silica	33·75	35·14	36·50
Alumina	9·74	10·15	10·80
Magnesia	30·24	31·47	28·20
Protoxyd of iron	8·19	8·60	9·54
Water	14·08	14·64	14·62
Insoluble sand	2·50	....	....
	98·51	100·00	99·66

"The silicate which here takes the place of the pyroxene and serpentine observed in the other specimens of Eozoön is one of frequent occurrence in the Laurentian limestones, and appears to constitute a distinct species, which I long since described under the name of loganite, and which occurs at the Calumet in dark brown prismatic crystals. I have since observed a similar mineral in two other localities besides the one here noticed. The result III, which is placed by the side of the analysis of the Burgess fossil, was obtained with a greenish-grey sparry prismatic variety from North Elmsley, having a hardness of 3·0, and a specific gravity of 2·539. These hydrous alumino-magnesian silicates, which I have included under the name of loganite,[1] are related to chlorite and to pyrosclerite in composition; but these last are distinguished from it by their eminently foliated micaceous structure."

"When examined under the microscope, the loganite which replaces the Eozoön of Burgess, shows traces of cleavage-lines, which indicate a crystalline structure. The grains of insoluble matter found in the analysis, chiefly of quartz sand, are distinctly seen as foreign bodies imbedded in the mass, which is moreover marked by lines apparently due to cracks formed by a shrinking of the silicate, and subsequently filled by a further infiltration of the same material. This arrangement resembles on a minute scale that of septaria. Similar appearances are also observed in the serpentine which replaces the Eozoön of Grenville, and also in a massive serpentine from Burgess, resembling this, and enclosing fragments of the fossil. In both of these specimens also grains of mechanical impurities are detected by the microscope; they are, however, rarer than in the loganite of Burgess."

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[1] **For a description of this and similar silicates, see Geology of Canada, p. 491.**

"From the above facts it may be concluded that the various silicates which now constitute pyroxene, serpentine, and loganite were directly deposited in waters in the midst of which the Eozoön was still growing, or had only recently perished; and that these silicates penetrated, enclosed, and preserved the calcareous structure precisely as carbonate of lime might have done. The association of the silicates with the Eozoön is only accidental; and large quantities of them, deposited at the same time, include no organic remains. Thus, for example, there are found associated with the Eozoön limestones of Grenville, massive layers and concretions of pure serpentine; and a serpentine from Burgess has already been mentioned as containing only small broken fragments of the fossil. In like manner large masses of white pyroxene, often surrounded by serpentine, both of which are destitute of traces of organic structure, are found in the limestone at the Calumet. In some cases, however, the crystallization of the pyroxene has given rise to considerable cleavage-planes, and has thus obliterated the organic structure from masses which, judging from portions visible here and there, appear to have been at one time penetrated by the calcareous plates of Eozoön. Small irregular veins of crystalline calcite, and of serpentine, are found to traverse such pyroxene masses in the Eozoön-limestone of Grenville."

Veins of fibrous serpentine (chrysotile) in like manner intersect the serpentine of this region, and are sometimes found cutting across the masses of Eozoön. It is stated in a note to this portion of the paper that, "Recent examinations have shown that some of these masses encrusted with Eozoön replaced by serpentine, consist of crystalline pyrallolite (rensselaerite); which seems, like the other silicates, to have replaced the organic matter of the Rhizopod."

"These observations bring the formation of siliceous minerals face to face with life, and show that their generation was not incompatible with the contemporaneous existence and the preservation of organic forms. They confirm, moreover, the view which I some years since put forward, that these silicated minerals have been formed, not by subsequent metamorphism in deeply buried sediments, but by reactions going on at the earth's surface.[2] In support of this view, I have elsewhere referred to the deposition of silicates of lime, magnesia, and iron from natural waters, to the great beds of sepiolite in the unaltered tertiary strata of Europe; to the contemporaneous formation of neolite (an alumina-magnesian silicate related to loganite and chlorite in composition); and to glauconite, which occurs not only in secondary, tertiary, and recent deposits, but also, as I have shown, in Lower Silurian strata.[3] This hydrous silicate of protoxyd of iron and potash, which sometimes includes a considerable proportion of alumina in its composition, has been observed by Ehrenberg, Mantell, and Bailey associated with organic forms in a manner which seems identical with that in which pyroxene, serpentine, and loganite occur with the Eozoön in the Laurentian limestones. According to the first of these observers, the grains of green-sand or glauconite from the tertiary limestone of Alabama are casts of the interior of Polythalamia; the glauconite having filled them by 'a species of natural injection, which is often so perfect that not only the large and coarse cells, but also the very finest canals of the cell-walls, and all their connecting tubes, are thus petrified and separately exhibited.' Bailey confirmed these observations, and extended them. He found in various cretaceous and tertiary limestones of the United States, casts, in glauconite, not only of *Foraminifera*, but of spines of *Echinus*, and of the cavities of corals. Besides, there were numerous red, green, and white casts of minute anastomosing tubuli, which, according to Bailey, resemble the casts of the holes made by burrowing sponges (*Cliona*) and worms. These forms are seen after the dissolving of the carbonate of lime by a dilute acid. He found moreover, similar casts of *Foraminifera*, of minute mollusks, and of branching tubuli, in mud obtained from soundings in the Gulf-stream, and concluded that the deposition of glauconite is still going on in the depths of the sea.[4] Pourtales has followed up these investigations on the recent formation of glauconite in the Gulf-stream waters. He has observed its deposition also in the cavities of *Millepores*, and in the canals in the shells of *Balanus*. According to him, the glauconite grains formed in *Foraminifera* lose after a time their calcareous envelopes, and finally become 'conglomerated into small black pebbles,' sections of which still show under a microscope the characteristic spiral arrangement of the cells.[*]

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[2] This Journal, [2], xxix, 281; xxxii, 286. Geology of Canada, p. 577.
[3] This Journal, [2], xxxiii, 277. Geology of Canada, p. 487.
[4] This Journal, [2], xii, 280.

"It appears probable from these observations that glauconite is formed by chemical reactions in the ooze at the bottom of the sea, where dissolved silica comes in contact with iron-oxyd rendered soluble by organic matter; the resulting silicate deposits itself in cavities of shells and other vacant spaces. A process analogous to this in its results, has filled the chambers and canals of the Laurentian *Foraminifera* with other silicates; from the comparative rarity of mechanical impurities in these silicates, however, it would appear that they were deposited in clear water. Alumina and oxyd of iron enter into the composition of loganite as well as of glauconite; but in the other replacing minerals, pyroxene and serpentine, we have only silicates of lime and magnesia, which were probably formed by the direct action of alkaline silicates, either dissolved in surface-waters, or in those of submarine springs, upon the calcareous and magnesian salts of the sea water."

In the second part of Dr. Hunt's paper on Natural Waters, published in this Journal for July, there will be found in § 41 some observations bearing on the formation of the silicates of lime and magnesia. The chemical and mineralogical relations of the Eozoön, or rather of its replacing silicates, are by no means the least important points in the history of this remarkable fossil.

Explanation of the Plate illustrating the Structure and Affinities of Eozoön Canadense.

Of the figures here given, 1, 3, 6*a*, 6*b*, and 7, are selected from two plates given by Dr. Carpenter to illustrate his paper; while 2, 4, and 5, are from the plates accompanying Dr. Dawson's description, and are from drawings by Mr. Horace H. Smith, the artist of the Canadian Geological Survey.

The figures, with the exception of 7, are from transparent sections of specimens in which the original shell was well preserved, and its minutest cavities infiltrated with serpentine. Figure 7 is from a specimen from which the calcareous skeleton was removed by an acid, and represents the internal casts of the tubes, as seen by reflected light.

Fig. 1. Vertical section of regularly stratified portion of *Eozoön* showing the ordinarily continuous connection of the chambers of each stratum; magnified 10 diameters.

2. Horizontal section of *Eozoön* from Grenville, magnified 25 diameters; *a*, systems of tubuli; *b*, secondary chamber.

3. Portions of two chambers of different layers, showing at *a*, *a*, the proper wells of their chambers; at *b*, *b*, the intermediate

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[*] Report of United States Coast Survey, 1858, p. 248.

skeleton; and at *c*, *c*, a stoloniferous passage; magnified 25 diameters.

Fig. 4. One of the systems of tubuli cut transversely; magnified 100 diameters.

5. Part of a system of tubuli cut transversely; magnified 200 diameters.

6. Portions of the proper wall of the chambers, showing its Nummuline tubulation, as seen at *a* in longitudinal, and at *b* in transverse section; magnified 100 diameters.

7. Cast of the interior of canal-system; an entire group magnified 10 diameters.

Art. XXXVII.—*Notices of Earthquakes.*

I. *Notices of the Earthquake of August 17th, in the Mississippi Valley;* received from Dr. A. Wislizenus, of St. Louis.

1. *Note by Dr. Wislizenus.*—In the city of St. Louis the motion was felt by many persons in two distinct shocks, but lasting, in my estimate, not longer than about twenty seconds. Some observed its direction as going from north to south.

2. *Notice from the Missouri Republican, of St. Louis, Aug.* 18, 1865.—The shock of an earthquake was sensibly felt in this city yesterday morning at 25 minutes to 9 o'clock. The motion was an oscillating one, the earth seeming to swing gently, like the pendulum of a clock. It lasted nearly a minute and a half—the intermediate motions being slighter than was perceived at the beginning and end of the shock. During its duration, chamber furniture, gas fixtures, etc., could be seen to rock, and a cracking sound proceeded from the walls of the houses, as if their steadiness was seriously disturbed.

Cairo, Ill., Aug. 17.—A severe shock of an earthquake was felt here at 8^h 45^m this morning, causing people to rush out of their houses in the greatest consternation.

Jackson, Mississippi, Aug. 17.—The effect of the earthquake was seriously felt about 9 o'clock this morning at Memphis, Lagrange, Holly Springs, Oxford, and Grenada. At Memphis the shock was severe enough to cause people to run out of houses.

3. *Extracts from a letter to Dr. A. Wislizenus* by Mr. John T. Scott, Attorney at Law at New Madrid, Mo., dated Sept. 6, 1865.—On the morning of the 17th of August, about half-past 8 o'clock, I was going to my office, and having to proceed through a park, and being about the middle of it, and in the best possible position for hearing and observation, I heard, as I thought for the moment, heavy rolling thunder. This astonished

me, for it was almost entirely clear, no clouds whatever visible that in the least indicated thunder, rain, or change of weather. I was at this time going in a southeasterly direction, and the thunder, as for the moment I had taken it to be, seemed to be exactly in the opposite direction from that in which I was then going. Astounded as I was, I stopped, and, turning to ascertain from whence this noise proceeded, the earth began to vibrate to and fro from northwest to southeast, and continued for several times, the first vibration being the greatest (apparently not less than three or four inches), and gradually diminishing. On turning fully around, the noise which I had taken for thunder, but which, in fact, was the noise accompanying the earthquake, seemed to be about in a northwesterly direction from here; and had it been thunder, I should have thought it not over twelve or fifteen miles distant. It seemed to roll or pass off in a northeasterly direction, or at right angles to the vibrations of the earth. The noise and vibrations lasted about a minute.

In the town, the druggists' jars, bottles, &c., were all rocked to the very edge of the shelves, and some of them off. Chimneys were considerably damaged; dishes, cups, saucers, glasses, &c. suffered much. The cattle and dogs all ran in confusion and seemed much alarmed, and utterly at a loss to know where or in what direction to go.

I was talking to a ferryman here a few days since about the earthquake, and he told me that he was at the time crossing the Mississippi river in a skiff, and that the water rolled as if a steamboat had just passed. He said he was much frightened until he heard the noise and thought what it was. The fact is, however—and such was the case at the time referred to—that the noise generally precedes the shaking or vibration from one to three seconds. In this case I think it was about two seconds.

I would mention one fact, notorious here, that the earthquakes are much more severe in the swamp or low and inundated lands, than in the high and dry lands. Hence it is that those living west of here, in what is called Little River Swamps, and follow hunting for an occupation, often experience earthquakes when we feel none at all. The earthquake of the 17th, of which I am speaking, was much more severe there than here or in high and dry lands.

Another remarkable fact about the earthquakes here, is, that while sometimes the earth quakes or trembles, as it were, at other times it seems to roll in waves, like the ocean, and at others, as in the one I am more particularly speaking of at present, it vibrates, apparently, in a horizontal direction. They most generally come from the west or northwest, sometimes from the southwest. The character of the noise, or roaring, as it is generally called, is also variable.

May I not now suggest a plan by which valuable information, relative to the time, center, and progressive movement of these convulsions, might be obtained? Suppose there should be a correspondent at the county seat of every county in the vicinity of the region of earthquakes, whose duty it should be to note all the facts of each convulsion, its direction, its duration, &c., and forward the same to some scientific gentleman for comparison and analysis. Could not something be obtained by this means?

I am told that the most graphic description of the earthquakes of 1811 and 1812 is to be found in the works of Lorenzo Dow. This description is from the pen, and in a letter of Mrs. Elizabeth Bryan to that reverend gentleman, Mr. Dow. The old lady was residing here at the time, and is still here, in her 88th year. Perhaps you might glean something from that letter.

I would further remark that I did not conclude the above last evening, and just as I was writing the line above my signature, at a quarter past 8 o'clock this morning, the 7th, we had another shock, shaking houses, &c. considerably. It was not accompanied with much noise—came from a westerly direction, and lasted about half a minute.

II. *Notices*, received from Dr. C. F. Winslow, of Boston.

1. *Letter to Dr. Winslow*, from Mr. G. Pendleton, dated Rochester, Sept. 10, 1865.—In accordance with my promise I herewith send you the facts about the earthquakes I have experienced during my voyages in the Pacific ocean.

(1.) *Earthquake off Kamtschatka in* 1845.—The first case was as follows:—On board the ship Charles Phelps, at sea, September 22nd, 1845, Cape Kamtschatka bearing N.W., distance 15 to 20 miles; the weather clear, wind light and baffling from the westward. At about 8 A. M. we felt a heavy shock of an earthquake, causing the ship to tremble about as she would if going in stays in a strong wind; and the noise on board was much like the rolling of a heavy cask fore and aft the deck. It lasted about 15 to 20 seconds, and then passed off, and left all quiet.

(2.) *Earthquake at Guam, one of the Ladrones, in* 1849.—The next earthquake I find noticed as follows, in my journal:—Ship Mary & Susan, of Stonington, at Ulmata Bay, Island of Guam, January 24th, 1849. At 2·30 P. M. experienced the shock of an earthquake which was far beyond anything I have ever known before. It lasted for about one minute, and was followed by seven other shocks during the night, the last being at daylight of the 25th. The last seven shocks were much lighter than the first. At the time of the first shock, and during the night, the atmosphere was a little hazy, and had a very strong sulphurous smell. The earliest intimation we had of the first great shock was an exceedingly great agitation of the water and the *land*. The convulsion on the land was so great that all of the brick and stone buildings on the Island were more or less injured, and some were reduced to a mass of ruins; and a man could not keep on his feet without holding on to something. I had a boat on shore at the time, and the inflood of the water was so great that it took her into the tops of the trees near the ocean, and swept water casks and such things a fourth of a mile or more into the country. And when the water receded, it left them with hundreds of *fish* high and dry, and the land at the watering place sank about twelve feet. When the water receded it took my ship back with such force that it parted my chain and I lost an anchor; she had run over it when the water flowed in and then went back with great force.

I would say that at the time of the first shock, and through the night, the wind was very light from the northeast. Several ships lying at Apra (the capitol of the Island) lost anchors by being covered up at the bottom of the harbor, and they had to part or cut their chains. I think there were six lost. The motion of the water on the Island was east and west.

2. *Notes from the Journal of Dr. C. F. Winslow.*—(1.) *Earthquake at Lima, March*, 1865.—T. J. Pope, Esq., Secretary of Legation to the Embassy of Peru at Lima, informs me that on the 1st of March 1865, at 6½ A. M., he was then on board the U. S. Ship Lancastre (acting as secretary to the Admiral), and his attention was called by the orderly to the strange agitation of the water in the bay. He states that the surface of the bay for a long distance around the ship was in a state of agitation and ferment, and that the water with wind was thrown up in jets from 12 to 15 inches. The odor of the sulphuretted or carburetted hydrogen was horrid, and the white paint of the ship was blackened. This occurred about the time (just before, in February,) when a great wave suddenly rushed into Callao bay and swept the bathing house away, and several persons drowned. There were several shocks of earthquakes about this time. But Mr. Pope felt no shock on the 1st of March, at the time of the phenomenon he speaks of.

Lima, April 20, 1865.

(2.) *Earthquake off Paita, May*, 1865.—At ¼ past 1 P. M., a strong earthquake was felt here which agitated the people very much. It lasted 20 seconds. This is experienced by me and is the strongest I have felt in Paita. Movement great from N.E.

Paita, May 15, 1865.

(3.) *Earthquake at Amotapa in Aug.* 1858.—In 1858, August 28, at 6·6 A. M., I felt a *very severe* earthquake at Amotapa. Made a crack in the river Chira, so that sulphurous fumes and petroleum came to the surface. This was felt very far into the interior and was supposed to originate in the neighborhood of Longai. Mr. Woodhouse and Mr. Dewal both tell me of this earthquake, being then residents of this place or Paita. The earth trembled slightly very often for the next 24 hours.

Amotapa, May 27, 1865.

(4.) *List of Earthquakes, copied at Lima, from books given me by Mr. Wm. Sterling, of Amotapa, May* 28, 1865[1].—Earthquake in Lima, 1860, Apr. 21, 1·35 A. M.; pretty strong. At 12 o'clock, midnight, of 21st, a slight shock. 22nd, 1·40 P. M., a very heavy shock; walls thrown down and others cracked; some churches injured.—23d, 1 A. M., a light shock; at 6·40 A. M. same day, another very severe shock; at 11·30 A. M. same day, another severe shock accompanied with a loud rumbling noise; 5·45 P. M., another light shock.—23d, at 12, midnight, and on 24th at 12·30 A. M., and at 2 A. M., light shocks.—26th, 6 A. M., a loud rumbling with slight shaking. May 1st, 7 A. M., shock pretty hard. 20th, at 11·30 A. M., shock pretty hard.

(5.) *Letter of June* 8, *from Hon. C. Robinson, U. S. Minister to Peru.*—In 1865, on May 29, at 7·16 A. M., an earthquake was felt at Lima which was very violent and lasted 56 seconds. It was very alarming, but did no serious damage. The air was damp and heavy and misty. Temp. 64° Fah. Cool for Lima.

III. *Miscellaneous Notices.*

(1.) *Earthquake in California.*—At 12^h 45^m October 8th, an earthquake of great violence occurred at San Francisco. Chimneys were tumbled down, the walls of many buildings badly cracked, bells of steeples made to ring, and fissures two or three inches wide were opened in the lower part of the city. There were two violent shocks within half a minute. The accounts from Sacramento, Stockton and San José represent the earthquake as the severest ever felt in those cities. It was not perceived at Marysville or Placerville; but Santa Cruz was severely shaken and some buildings much damaged

The tide at San Francisco rose very high at the time of the shock and fell very low immediately afterward. Ten or eleven distinct shocks were felt after the first shaking, up to 5 A. M. of the 9th.

(2.) *Earthquake at Buffalo.*—The announcement of an earthquake at Buffalo in the last volume of this Journal (xxxix, 372), taken from a Buffalo paper, is an error.

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[1] **In Tacna, Sept, 19, 1860.**

ART. XXXVIII.—*On Metabrushite, Zeugite, Ornithite and other minerals of the Key of Sombrero, W. I.;*[1] by ALEXIS A. JULIEN, Assistant in School of Mines, Columbia College.

SOMBRERO, a remnant of a coral Island, lying in lat. 18° 36′ N. and long. 63° 27′ W., varies in height from fifteen to forty feet, and comprises an area of about ninety-five acres. The quality and continued yield of the deposit of rock guano, which is disseminated in veins through the beds of limestone, have rendered its name familiar. During a residence of between three and four years (1860–1864), the following minerals have been observed by me, an unusual number for a formation of so recent geological origin and limited extent.

Calcite, and its phosphatic pseudomorphs.
Common salt, in cubes and in curved crystals.
Brushite, in massive specimens.
Metabrushite, and its pseudomorph, *zeugite*.
Ornithite, and its pseudomorphs.

1. *Calcite.*—This mineral, in its variety of massive, laminated, and stalactitic forms, is very abundantly dispersed throughout the fissures of the limestone, and small crystals though less common are not rare. All these display the ordinary characteristics of the mineral and possess no features worthy of notice.

However, a remarkable pseudomorphous process, consisting in the substitution of phosphoric for carbonic acid in the calcareous material of the Key, has taken place on a very extensive scale at various periods in its geological history. This has been effected by solutions of certain salts, from the superficial guano deposit, brought down by the rains into the joints of the limestone, and has given rise to various white, yellowish, and brownish materials. These are especially rich in the bone-phosphate of lime ($3CaO, PO_5$), containing about 85 per cent of that salt, also about 5 per cent each of phosphate of magnesia and carbonate of lime, with small percentages or traces of organic matter, sulphate of lime, oxyd of iron, alumina, and their phosphates, and fluorine. The masses of calcite in the vicinity of the guano-veins have participated in the same changes and thus interesting pseudomorphs have been produced, not only of the massive and stalactitic forms but of the crystals. Of the latter three varieties may be mentioned:

(1.) Those which consist simply of an ordinary crystal of calcite, overlaid with a brownish film of the phosphatic material.

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[1] I take this opportunity to acknowledge my indebtedness to Prof. C. F. Chandler of the School of Mines, Columbia College, for the use of his private laboratory, (in which nearly the whole of this investigation has been made), and for his general assistance and advice; to Profs. O. N. Rood and C. A. Joy of the School of Mines, for the use of instruments and books; and to Prof. J. D. Dana of New Haven, for his crystallographic examinations.—A. A. J.

(2.) Hollow crusts, formed by the removal from the former of their cores of carbonate of lime.

(3.) Solid crystals, wholly consisting of the phosphatic material.

2. *Common Salt.*—The numerous pools of sea-water along the edge of the cliff, which are fed by the spray from the surf in high winds and heavy ground swells, sometimes evaporate considerably or wholly during the droughts of the winter season and the long calms of the summer. Thus, as in the large salt-ponds of neighboring islands, their bottoms are left sparkling with the ordinary cubical crystals of common salt.

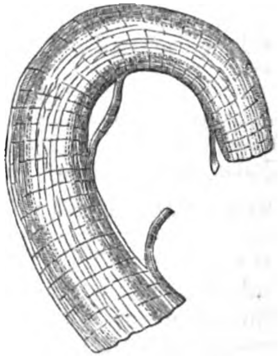
But again, in September, 1863, a quarry in the northern part of the Key was partially filled by the overflow of a heavy ground-swell, and in this way masses of a porous variety of rock-guano were saturated with sea-water. The waters soon subsided through the bottom of the quarry and a peculiar efflorescence of salt afterwards appeared, near the sea level. It occurred over the surfaces of these masses on the sides of the quarry, in cavities shielded from the sun and rain, but otherwise freely exposed to the atmosphere. This crust consisted of slender crystals, usually one-eighth of an inch in length, white, translucent, and so much *curved* as to take, in some cases, one and a half turns, or to return to and become re-embedded in the matrix. In this last characteristic and in their fibrous structure they strongly resemble, especially under the microscope, the similar forms of gypsum, etc. Fracture uneven. A tendency is shown, (by the splitting of their terminations, the separation of narrow off-shoots from their sides, etc.), to a disintegration into the slender fibers of which each crystal seems to be an aggregation. There is also a transverse plane of cleavage, marked by the numerous lines of cross-fracture. In the thicker crystals the breadth is, in most cases, nearly uniform throughout their length; but in others it often decreases from one end to the other. The outer termination is usually rounded and sometimes a slender crystal ends in a sphere of greater diameter. The transverse section of every crystal is apparently almost circular, though irregular. The following are the results of an analysis:

	I.	II.
Moisture (expelled at 100° C),	·34	....
Loss by ignition,	·37	....
Guano, attached to the crystals,	1·53	....
Sulphuric acid,	·35	....
Chlorine,	58.72	58·73
Sodium,	37·39	....
Potassium,	·10	....
Magnesium,	·03	....
Lime,	·40	....
Aluminum,	·07	....
	99·30	

These results are equivalent to water ·55, guano 1·53, sulphate of lime ·75, chlorid of aluminum ·35, chlorid of calcium ·30, chlorid of magnesium ·11, chlorid of potassium ·20, and chlorid of sodium 95·46=99·25.

The crystals were found to be very deliquescent in an atmosphere saturated with moisture, absorbing more than their weight of water after a few days exposure. A sketch of a crystal magnified about fifteen diameters is given in fig. 1.

1.



3. *Brushite.*—A few small specimens of massive brushite, crystalline in structure and having a botryoidal surface, were found in one locality as crusts adhering to guano. Drusy cavities occur in this material, but no crystals like those from Aves Island, described by by Mr. G. E. Moore,[1] have yet been found. The following are the characters of this massive brushite: Hardness =2·76. Specific gravity =2·953–2·999, by two determinations of the coarsely powdered mineral in alcohol and in pure benzole, at 60° F. Luster vitreous, inclining to resinous; shining. Color white, but with a superficial yellowish tinge, (lighter than that of Moore's crystals). Streak and powder white. Translucent; in thin laminæ transparent. Fracture uneven. Brittle.

Gently heated in a closed tube before the blowpipe, it at once gives off water, darkens with a slight empyreumatic odor, and again whitens by ignition. The water, condensed above, is decidedly acid, and gives the reactions for phosphoric acid. In the platinum forceps and on charcoal, it glows with a greenish light at a high temperature and fuses with a little intumesence to a button which is crystalline on cooling. At the same time it imparts a slight greenish tinge to the OFl, especially if previously moistened with sulphuric acid. With borax and with microcosmic salt it fuses easily, with a little effervescence, to a bead which is clear and colorless, both hot and cold; when nearly saturated, the bead is clear and slightly yellowish while hot, transparent and colorless when cold, and becomes milk-white and opaque by flaming; with a still larger excess it is slightly yellowish and transparent while hot, and becomes white and opaque on cooling. With carbonate of soda on charcoal it fuses readily to a clear and colorless glass. It dissolves easily (even after ignition) in nitric and hydrochloric acids, but very slowly in acetic and tartaric. When boiled with water in a test-tube the water gives a strong acid reaction with litmus; most probably from the well-known decomposition of the salt, by hot water, into bone-phosphate and super-phosphate ($CaO, 2HO, PO_5$) of lime.

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[1] This Jour., [2], xxxix, 48.

Two determinations were made of the amount of water in the air-dried material, the assay being successively heated in each case in an air-bath, at various temperatures from 100° to 245° C., and then ignited at a red-heat. The results were as follows:

	Temperature of expulsion.	I.	II.	Mean.
Hygroscopic moisture,	100° C.	1·388	1·071	12·29
Water, - - -	126°–240° C.	19·698	19·929	
Water and org. matter,	ignition.	6·111	6·155	
Combined water,		25·809	26·084	25·947
Total,		27·197	27·155	27·176

A tendency is thus exhibited to the expulsion of the four equivalents of water of crystallization (in the formula below) between 126°–240° C., the basic equivalent being retained until the temperature becomes much higher, or reaches ignition. The following is an analysis made with one gram of material:

		Oxygen Ratios.		
Moisture, - - - - - - -	1·229			
Water and organic matter, -	25·947	23·06	2·52	5
Silica, - - - - - - - -	*trace.*			
Sulphuric acid, - - - - - -	·778			
Phosphoric acid, - - - - -	39·947	22·39	2·45	5
Magnesia, - - - - - - -	*trace.*			
Lime, - - - - - - - -	32·116	9·14	1·	2
Alumina and sesquioxyd of iron, -	·329			
Fluorine, - - - - - - -	*trace.*			
	100·346			

These figures correspond to the following composition:

Moisture, - - - - - - - - - -	1·229	
Organic matter, - - - - - - - -	·346	
Sulphate of lime ($CaO, SO_3+2HO,$) - -	1·675	
Alumina and sesquioxyd of iron, - - - -	·329	
Silica, phosphate of magnesia, and fluorid of calcium,	*traces.*	
Impurities,		3·579
$2CaO$, - - - - - - - - -	31·569	
HO, - - - - - - - - -	5·050	
PO_5 - - - - - - - - -	40·004	
4aq. - - - - - - - - -	20·200	
Brushite,		96·823
		100·402

The method, employed in this and all but one of the subsequent analyses, was as follows, the precipitates and filtrates being treated in the usual way. The lime was first precipitated with sulphuric acid and alcohol. To the filtrate, evaporated to small bulk, citrate and aqua ammonia were added in excess, by which all the magnesia and a part of the phosphoric acid were thrown down as ammonio-phosphate. In the filtrate the remainder of the phosphoric acid was precipitated with the usual sulphate of magnesia solution. The filtrate was evaporated to a small bulk, digested in a flask with nitric acid until the complete expulsion of all chlorine, evaporated to dryness in a platinum dish, and calcined to perfect whiteness. The residue (excess of magnesia from the reagent) was redissolved in hydrochloric acid, the alumina and the oxyd of iron precipitated with aqua and carbonate of ammonia, and, after weighing, the iron redissolved and determined volumetrically, if desired. In the filtrate the alkalies if present were separated from the excess of magnesia.

In another portion of material, the water was determined as already explained, the ignited residue dissolved in nitric acid, and the chlorine and sulphuric acid successively estimated by additions of nitrate of silver and of nitrate of baryta.

4. *Metabrushite.*—This new mineral has been observed to occur only with the guano as a matrix, and in two localities in the northern part of the Key; in one, studding the sides of a cavity and attached in microscopic crystals to many small rootlets within it; and in the other, as a single large group, weighing about ten grammes, some of the crystals being nearly an inch in length and half an inch in breadth. The crystals are usually short, thick, and tabular, with most of their faces uneven and lusterless, unlike the prisms of brushite. The planes of cleavage are the same as in that mineral and in selenite, but are more imperfect. No massive specimens have yet been discovered. Its characters are as follows:

H. =2·75. Sp. gr. (with large crystals and distilled water at 60° F.) =2·288, 2·356, and 2·362. Luster vitreous, inclining to pearly, splendent on cleavage faces parallel to the clino-diagonal section; somewhat resinous on surfaces of irregular fracture. Color yellowish-white, sometimes with a slight reddish tint by transmitted light. Streak and powder white. Transparent; in some crystals, however, the surface is covered with a translucent film, by which their transparency is disguised and to which their reddish tinge is probably due. Fracture uneven. Brittle. It presents the same characteristics before the blowpipe and when treated with acids as massive brushite, except that it is apparently a little more slowly soluble in the mineral acids. It also exhibits strong double refraction.

In one assay the total loss of water by ignition was at once determined, and in three others, as in brushite, the loss at successive temperatures from 110° to 400° C. was carefully ascertained.

	Temp. of expulsion.	I.	II.		III.		IV.	
Moisture,	100° C.			1·620		1·515		1·380
Water,	126°–240° C.		17·983		17·419		17·191	
Water and org. matter,	Ignition.	21·865	4·293		4·600		5·461	
Combined water,				22·276		22·019		22·652
Total,		21·865		23·896		23·534		24·032

A tendency was noticed to the expulsion of one of the constitutional equivalents of water (in the formula below) at about 126° C., of the remaining two at 175°–180° C., a fraction of a per cent of water at 235°–239° C., and the basic equivalent on ignition; but the figures were somewhat variable. The quantity of material in my hands being small, the following analyses were made from an amount altogether a little less than two grams, according to the method already explained.

	I.	II.	III.	IV.	Mean.	Oxygen Ratios.		
Moisture,	21·865	1·620	1·515	1·380	1·505			
Water and organic matter,		22·276	22·019	22·652	21·826	19·40	2·07	4
Sulphuric acid,	none.	·208	none.	none.	·052			
Phosphoric acid,	42·527	42·688	43·103	42·565	42·721	23·95	2·55	5
Lime,	32·981*	32·731	33·346	32·849	32·977	9·38	1·	2
Magnesia,	·881	trace.	·645	·571	·524			
Alumina,	1·292	trace.	·505	·930	·795			
Sesquioxyd of iron,	·454							
	100·	99·523	101·133	100·947	100·380			

* By difference.

There was no trace of chlorine present, and no examination was made for fluorine. The results may be grouped as follows:

Moisture,	1·491
Organic matter,	·717
Sulphate of lime,	·068
Phosphate of magnesia, ($3MgO, PO_5$),	1·147
Alumina, oxyd iron, and phosphoric acid,	1·049=4·472 impurities.
2CaO,	32·955
HO,	5·277
PO_5	41·844
3aq.	15·832=95·908 metabrushite.
	100·38

While, therefore, brushite corresponds to the salt of Bödeker,[2] which contains four equivalents of water of crystallization and which was found by Ples[3] in almost pure acicular crystals in the central tissue of *Tectonia grandis*, we have in metabrushite the allied salt of Ræwsky[4] and Berzelius, the analysis (1) and theoretical composition (2) of which, by Berzelius, are as follows:

[2] Ann. Pharm, lxix, 206.

[3] Rép. Chim. Appliq., xi, 324.

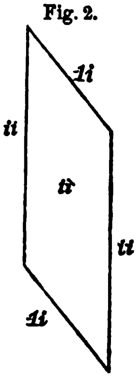
[4] Pharm. Centralbl., 848, 285.

1. 2CaO, 34·39 HO, 5·50 PO_5, 43·61 3aq, 16·50 = 100.
2. 35·42 5·67 41·90 17·01 = 100.

Its relation to brushite and succession in amount of water have therefore been designated in the prefix of its name (*μετα*, *after*). In many of its characteristics it bears a strong resemblance to gypsum, a mineral which, it may be stated, does not occur upon the Key. I am indebted to Prof. J. D. Dana for the following results of his crystallographic examination of metabrushite and its pseudomorphs.

"System monoclinic. Occurring planes the prismatic *i-i* (orthodiagonal) and *i-ì* (clinodiagonal) with a hemidome which may be lettered –1-*i*, as shown in the annexed sectional figure, (fig. 2,) *i-i* on –1-*i*=41° 30′–42° 30′, as measured with the common goniometer, but varying in some cases between 38° and 46°. . . . Large crystals, which vary from a fourth of an inch to an inch in length, often nearly half as broad as long, with commonly only *i-i* smooth, –1-*i* being crossed vertically by a few deep furrows and ridges, and rounding either side into *i-ì*, which is also uneven from a series of furrows parallel to those of –1-*i*, (see fig. 3). Cleavage clinodiagonal, easy and pearly. The crystals differ in the occurring planes from those of brushite, although like them in being of the monoclinic system, and in having a pearly clinodiagonal cleavage. They approximate moreover in the angle *i-i* : –1-*i*; for calculation gives for this angle in brushite (in which the plane –1-*i* does not occur) 41° 44′. The discovery of other crystals at Sombrero with additional planes, and those that are better for measurement, is necessary to settle positively the question of identity."

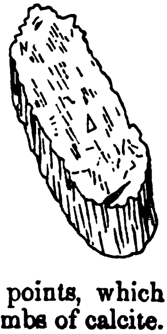
Fig. 2.



Pseudomorphs.—Pseudomorphous crystals (hollow crusts or shells) in considerable abundance have been found in certain localities in the course of quarrying. Of these at least the two following varieties may be unhesitatingly referred to metabrushite, possessing the same form and the peculiar furrowed planes.

A. *Zeugite.*—H. =3·25. Sp. gr.=2·971. Color white, and with light shades of yellow and brown. Translucent. The crust in these crystals usually very thin, brittle, and fragile. The exterior surface is tolerably smooth (on the orthodiagonal plane,) though without lustre; but the interior, on being broken open, is extremely ragged and irregular. Both surfaces are often, but not always, completely covered with glittering points, which beneath the microscope are revealed to be tiny rhombs of calcite.

3.



This rare variety of the pseudomorphs was found associated in the same cavity with the large group of crystals of metabrushite before mentioned in stellated, radiating, and irregular groups and drusy crusts, adhering to guano and also to the sides of cavities in the limestone. A few are also attached to the large group itself, showing the commencement of the pseudomorphous process. Two analyses of this material follow, of which I is the mean of two almost complete analyses of the same specimen, in which the phosphoric acid was determined by means of oxyd of mercury.

	I.	II.	Mean.
Moisture,	·08	2·97	3·02
Water and organic matter,	2·99		
Sulphuric acid,	·39	*trace.*	·19
Carbonic acid,	*trace.*	·48	·24
Phosphoric acid,	47·07	46·03	46·55
Magnesia,	3·25	3·92	3.59
Lime,	44·18	44·24	44·21
Alumina and sesquioxyd of iron,	·54	·78	·66
Fluorine,	*trace.*	*trace.*	*trace.*
Chlorid of sodium,	*undt.*	1·08	1·08
	98·50	99·50	99·54

In grouping together these results we must of course, in the first place give to the percentages of sulphuric and carbonic acids their equivalents of lime, and to that of magnesia sufficient phosphoric acid to form the salt, $3MgO, PO_5$. There only remain—

		Atomic ratios.	
Lime,	43·78	1·56	8
Phosphoric acid,	42·28	·59	3

If we calculate to the lime its proportion of phosphoric acid to form the bone phosphate, $3CaO, PO_5$, five per cent of phosphoric acid will still remain uncombined, except with the trifling quantity of alumina and oxyd of iron. From these considerations, necessary to elucidate the proper method of combining the results, (a question almost always a matter of great doubt and uncertainty,) the following arrangement seems the most probable:

Water and organic matter,	2·93
Sulphate of lime,	·41
Carbonate of lime,	·54
Phosphate of lime ($8CaO, 3PO_5$),	85·62
Fluorid of calcium,	*trace.*
Phosphate of magnesia,	7·86
Phosphates of alumina and iron,	1·10
Chlorid of sodium,	1·08
	99·54

This is therefore a new but accidental product, and although it is amorphous and there is no apparent relation between the constituents, (the phosphate of magnesia, alumina, etc., probably occurring in variable proportions, here as elsewhere,) nevertheless it differs somewhat in appearance from every other phosphatic material upon the Key, and is the only substance, I believe, in which this phosphate of lime, $8CaO, 3PO_5$, occurs in nature. This salt is the one whose occurrence in bones, as reported by Berzelius, as well as certain methods for its manufacture, were ascertained by Raewsky to be incorrect; but no evidence has been offered against its identity (with the addition, perhaps of some proportion of water) with the precipitate formed[*] by pouring chlorid of calcium, drop by drop, into a solution of phosphate of ammonia, containing an excess of ammonia, until half the phosphoric acid is thrown down. In the following table its composition, together with that of the bone phosphate of lime, are put side by side for the sake of comparison:

$8CaO$,	225·04	51·248	$3CaO$,	84·39	54·183
$3PO_5$	214·08	48·752	PO_5,	71·36	45·817
	439·12	100·		155·75	100·

In this case the salt has probably been formed by the union of an equivalent of the neutral phosphate of lime of the original crystals of metabrushite with two of the bone phosphate of lime of the pseudomorphous solution, and the old theory of its composition is thus confirmed, (rejecting the water:)

$$2CaO, PO_5 + 2(3CaO, PO_5) = 8CaO, 3PO_5.$$

To express, therefore, the junction in this mineral of metabrushite and ornithite, (soon to be described,) it may be called *Zeugite* (ζευγιτης, yoked or joined together,) and the other phosphates, etc., present, may be considered, as usual, impurities.

In the platinum forceps, before the blowpipe, it darkens with an empyreumatic odor, whitens, glows with a greenish light (though not so strongly as in the case of brushite and metabrushite,) but does not fuse, even upon thin edges. In a closed tube it darkens, with the odor, whitens, and gives off a very little hygroscopic water. With borax, microcosmic salt, and carbonate of soda, it acts like ornithite. When boiled in a test-tube with water, the latter becomes alkaline.

In cavities of the limestone in another locality of the Key, a variety of these pseudomorphs has been found in which the hemidome, $-1\text{-}\bar{i}$, is invariably covered with many small crystals. In the same cavities also occur large hollow hemispherical and botryoidal crusts. These are probably pseudomorphous crystals of metabrushite, with the angles rounded off by a concretionary tendency in the pseudomorphous process, or they may be pseudomorphs of massive specimens of the mineral. Their material resembles that just described, in color, etc., (except that it is not drusy with calcite,) and it has probably the same composition.

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[*] Berzelius, Traité de Chem., iv, 386.

B. Color straw yellow, buff, to dark shades of brown. Subtranslucent—opaque. Brittle. Crusts rather thicker than those of A, without the glittering surfaces. Found in isolated crystals, irregular groups, and drusy films, in cavities and fissures of the limestone, and in radiating groups upon stalactitic deposits of phosphate of lime. From their color and furrowed sides they often resemble little chips of wood.

C. Sp. gr. =2·988–3·030. Color straw yellow, buff, and shades of brown. Opaque. Brittle. Narrow blades, sometimes an inch and a quarter in length. In radiating or irregular groups. Crust thick; sometimes so much so that the crystals are nearly or quite solid. Occurs in limestone cavities. The following are a complete and a partial analysis of different specimens, the latter having been spoiled by an accident:

	I.	II.
Water and organic matter, - - - -	3·98	3·88
Sulphuric acid, - - - - - -	·18	—
Carbonic acid, - - - - - -	1·74	a little
Phosphoric acid, - - - - -	43·24	—
Magnesia, - - - - - - - -	·56	—
Lime, - - - - - - - -	48·87	47·15
Alumina and sesquioxyd of iron, - -	1·02	—
Fluorine, - - - - - - -	*trace.*	*trace.*
Chlorid of sodium, - - - - -	?	—
	99·59	

In grouping these results by the method employed in the case of A, if we calculate the remainder of the lime as bone-phosphate, 3 per cent of phosphoric acid will remain uncombined except with alumina and iron. It seems more judicious, therefore, to suppose the existence of a mixture of the two phosphates of lime in this case, and arrange the analysis, for example, as follows: Water and organic matter, 3·90; sulphate of lime, ·39; carbonate of lime, 3·95; phosphate of lime ($8CaO, 3PO_5$), 35·28; bone phosphate of lime, ($3CaO, PO_5$,) 52·49; phosphate of magnesia, 1·22; phosphates of alumina and iron, 2·36; fluorid of calcium, trace; =99·59. As the narrowness of the pseudomorphs of this variety is unusual among at least the known crystals of metabrushite, they may possibly be pseudomorphs of prisms of brushite, if such ever existed on the Key. In my first published statement* of the discovery of metabrushite, I erroneously attributed to its composition the results of a qualitative examination of the foregoing pseudomorphous material. No analysis has been made of the material of variety B, but it is probable that in both B and C the material always consists of zeugite, with bone phosphate of lime (as well as the phosphates of magnesia, etc.,) as an impurity, in proportions increasing until perhaps the amount of $8CaO, 3PO_5$ becomes too small to be distinguished.

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* This Jour., [2], xxxvi, 424.

5. *Ornithite.*—In the empty casts of Madrepores and other cavities of the coral limestone, in two localities of the southern part of the Key, another new mineral occurs.

System monoclinic. Small crystals, not over a fourth of an inch in length, usually very narrow, with the planes even and not polished, and $-1\text{-}i$ very slightly convex, resembling somewhat a common form of gypsum. Clinodiagonal cleavage perfect; so that this, unlike the other minerals, is easily sectile into very thin scales; two other planes, as in gypsum.

The broader crystals are arranged in radiating groups, while most of the narrower are isolated and strewn irregularly over the surfaces of the cavities. No massive specimens of the mineral have yet been found.

Hardness =2·5; (just scratches mica when rubbed upon it). Lustre pearly on clino-diagonal surfaces. Color white. Streak and powder white. Translucent. Flexible.

When heated before the blowpipe in a closed tube, it darkens with a feeble empyreumatic odor, loses water, and again on ignition becomes white, opaque, and lusterless. It is probable that the water condensed above is neutral to the test-papers. In the platinum forceps and on charcoal, the mineral undergoes the same changes and does not fuse.[7] When previously moistened with sulphuric acid, the greenish tinge of phosphoric acid is imparted to the flame. With borax and with microcosmic salt it dissolves readily, and apparently with greater effervescence than in the case of massive brushite or metabrushite, to a bead which is clear and colorless both hot and cold; when nearly saturated, the bead is yellowish while hot, clear and colorless when cold, and becomes white and opaque by flaming: with a still greater excess, the bead is transparent and yellowish while hot, and becomes white and opaque on cooling. Readily soluble in nitric and chlorhydric acids. In thin scales it exhibits a feeble display of colors under the polarizing microscope.

The amount of this mineral in my possession being very small, only about one-tenth of a gram of pure crystals could be used for analysis, and it was first carefully dried at 100° C., with but trifling loss. With this two water determinations were first made, from ·048 and .058 grame respectively of unbroken crystals, as follows:

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[7] On account of the small size of the fragments and their mobility before the blowpipe flame, I was unable to determine the effect of a long continued ignition; but at the end of a minute's ignition no trace of fusion could be perceived.

	Temperature of expulsion.	I.	II.	Mean.
Water,	250° C.	3·918		....
Water and trace of org. matter,	strong ign.	5·154	9·827	....
Total,		9·072	9·827	9·449

A tendency is thus shown to the expulsion of one equivalent (in the formula obtained below) below 250° C., and the retention of the other until ignition. The ignited residues were combined and analyzed in the usual manner, with the following results:

		Oxygen ratios.		
Water and trace of organic matter,	9·449	8·4	1·	2
Phosphoric acid, - - - -	40·139	22·5	2·67	5
Magnesia, - - - - -	*trace.*			
Lime, - - - - - -	45·768	13·2	1·54	3
Alumina and sesquioxyd of iron,	4·623			
	99·979			

No attempt was made to test for sulphuric acid and fluorine, traces of which may occur. These results correspond to the formula $3CaO, PO_5+2aq.$, so that this mineral is identical in composition with the gelatinous precipitate produced when the bone-phosphate of lime is thrown down from solution. The foregoing figures may be arranged as follows:

Organic matter, - - - - - -		*trace.*
Alumina, oxyd of iron, and their phosphates,		6·061
Phosphate of magnesia, - - - -		*trace.*
$3CaO$ - - - -	45·768	
PO_5, - - - -	38·701	
2aq., - - - -	9·762	94·231
		100·292

I do not think that I can do better than to dedicate this species to my old comrades, the sea-birds of Sombrero, (ὄρνις, *a bird,*) to whom we are mainly indebted for the contribution of the guano-deposit to American agriculture, and for the formation of this interesting companion to apatite. Its physical characteristics, even more than those of metabrushite, correspond remarkably with those of gypsum, to which species it is also allied in its formula and mode of crystallization.

Pseudomorphs.—a. White and yellowish white crystals, with glittering surfaces and a material somewhat similar in appearance to the A of metabrushite; the crystals differ usually in their greater slenderness. These occur in cavities of the limestone, intermixed with the crystals of ornithite.

b. Hemispherical stellated groups. Color white. Opaque. Friable.

No analysis has been made of the material but I suppose it to be identical with that of the original crystals, *minus* the water. They occur intermixed with the original crystals; and also as solitary white groups, dotting over in marked contrast the dark brown phosphatic lining of cavities in the limestone.

In conclusion it may be observed that specimens of all these phosphatic minerals are exceedingly rare, few having been found since my departure from Sombrero (in the spring of 1864). Good specimens of the pseudomorphs are also uncommon, though imperfect or drusy crystals have occasionally been found in tolerable abundance on the opening of some new vein. It is a universal rule, when the original or the pseudomorphous crystals occur in cavities of the limestone, that they are never in direct contact with the matrix, a film or thin layer of a common brownish material, rich in bone-phosphate of lime, always intervening. As to my theory in regard to the origin and history of these minerals and of their pseudomorphs, I must refer to the coming publication of my general observations on Sombrero.

It is sufficient here to state that they have undoubtedly been deposited from the salts of the superficial guano-deposit brought down in solution by carbonated rain-waters into the joints of the limestone.

School of Mines, Columbia College, New York. June, 1865.

Art. XXXIX.—*On Two Varieties of Sponge-Spicules;*[1] by Alexis A. Julien.

In clearing the white coral-sand out of a crevice of the rock near the east cliff of the Key of Sombrero, W. I., a loose mass of snow-white acicular fibres was found, associated with fragments of coral, shells, etc., at the height of thirty-six feet above the sea, and imbedded in the sand at a depth of about three feet from the surface. Among these fibers the two following varieties were distinguished, the former composing the bulk of the mass:

A. Acicular, tubular, and gently-curving spicules (fig. 1, *a*), tapering from the middle toward each end, at first very gradually, and then rather suddenly at a short distance from the end, to a rounded point.

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[1] My grateful acknowledgements are due to Prof. O. N. Rood of the School of Mines, Columbia College, for the use of his microscope and for general assistance and advice, and to Prof. C. F. Chandler of the School of Mines, for the facilities of his laboratory.—A. A. J.

The curvature of the ordinary spicules is in general very slight, and they are sometimes nearly straight. Among them, however, other forms occur (fig. 1, *b*) whose length is about the $\frac{1}{1000}$ of an inch, and whose curvature amount to that of a semi-circle. These I take to be rudimentary spicules and suppose that they may lose their curvature in their enlargement.

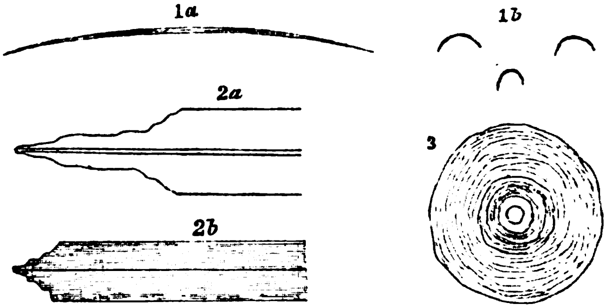


Fig. 1, magnified 14 diameters; fig. 2, 250 diameters; fig. 3, 727 diameters.

The sudden contraction near each end of a spicule occasions an irregular slope (fig. 2, *a*) or perhaps more frequently a series of from two to nine steps (fig. 2, *b*) consisting of a succession of curved planes. This slope is usually from $\frac{1}{1000}$ to $\frac{14}{1000}$ of an inch in length, and it not only varies greatly in length and form in different specimens, but sometimes at the ends of the same spicule.

The transverse sections of the spicules are generally circular, though sometimes somewhat elliptical, and the outer surface appears perfectly smooth. When the spicules are examined under a high power, a series of very fine parallel lines can be distinguished on either side of the central tube (fig. 2, *b*). In the transverse sections also (fig. 3), a series of very numerous but usually very faint concentric rings is often revealed, of which from three to five, adjacent to the center, are often most strongly marked and sometimes can alone be distinguished. We may therefore conclude that each spicule is constructed of a great number of enveloping sheaths. (They are much too remote in the figure.)

The section of the tube is generally circular, but sometimes triangular, elliptical, or oval. Its diameter diminishes, like that of the spicule, near each end, but the tube evidently extends through to the outer surface, since when an unbroken spicule is immersed in water the liquid enters the capillary tube by the orifice at each end. When magnified, a portion of a spicule bears a strong resemblance to a thermometer tube. The dimensions are:—

	Variation.	Mean.	No. of Measurem'ts.
Length, - - - - - - - -	$\frac{1}{200}-\frac{1}{5}$ inch	$\frac{1}{7}$ inch	40
Diameter at middle, - - -	$\frac{1}{6666}-\frac{1}{400}$ inch	$\frac{1}{775}$ inch	58
" " end, - - - -	$\frac{1}{20000}-\frac{1}{3333}$ inch	$\frac{1}{6000}$ inch	15
Ratio of middle diam. to length,	$\frac{1}{25}-\frac{1}{160}$	$\frac{1}{108}$	8
Ratio of middle to end diameter,	$\frac{1}{25}-\frac{1}{2}$	$\frac{1}{6}$	13
Diameter of central tube, - -	$\frac{1}{20000}-\frac{1}{1000}$ inch	$\frac{1}{3788}$ inch	45
Ratio of bore to diameter, -	$\frac{1}{40}-\frac{2}{7}$	$\frac{1}{8}$	45

The range in the variations of the measurements and of the ratios is consequently very considerable, and the same conclusion is impressed by a simple examination of a few spicules under a microscope, without measurement. Some of the longest spicules are the narrowest, and the thickness of the walls of the tube seems to bear no fixed relation to any other dimension.

A portion of the mass was carefully cleansed, by digestion with acidulated and with pure water, from the intermixed impurities, particles of sand, etc., and on analysis yielded the following results:

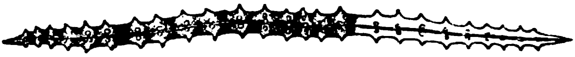
Water, - - - - - -	7·002
Silica, - - - - - -	89·84
Alumina, - - - - - -	·29
Protoxyd of iron, - - - - -	·09
Lime, - - - - - -	·66
Magnesia, - - - - - -	·22
Potassa, - - - - - -	·63
	99·63

Hardness =3. Specific gravity =2·071. Color white. Luster pearly. Powder white. Transparent. Elastic. Easily ground to a very fine powder. Fracture conchoidal. Transverse planes of fracture very distinct; so that on coarsely pulverizing the spicula, very thin and beautiful transverse sections may be readily obtained among the fragments. The material does not possess double refraction.

In the platinum forceps or on charcoal, before the blowpipe, the spicules decrepitate slightly, whiten, lose their luster, and curl together into a coherent mass. When this mass is examined beneath a microscope, the ends of the spicules are found to be fused and in many cases blown into thin bulbs, or a series of bulbs occurs along their stems. This has evidently been effected by the fusion of their ends and the expansion of the air in the tubes. In a closed tube the spicules whiten, curl, and yield water. Unaffected by the acids, hot or cold, but readily soluble in a solution of caustic potassa.

B. Acicular, tubular, and gently-curving spicules, tapering gradually from the middle toward either extremity. Only about a dozen specimens have been observed, all of which were fragments like that sketched in fig. 4. These fragmentary spicules differ from those of A in their minute size and in being marked by a series of from nine to sixteen nodes, each consisting of from four to eight sharp spines encircling the spicule. From their general correspondence to the spicules of A, in their curving and tapering outline and tubular character, as well as from an analogy (stated below) in regard to the number of the spines upon the nodes, I have ventured in fig. 4 to reproduce in lighter lines the lost portion of the spicule.

4.



Magnified 436 diameters.

The surfaces between the nodes are more or less concave, and the intervals, though nearly equal in the same spicule, vary from the $\frac{1}{6250}$ to the $\frac{1}{2300}$ of an inch, averaging the $\frac{1}{3840}$: but this variation seems to be partly occasioned by the contraction of the intervals between a few of the central nodes, and in the larger specimens the length of the intervals (as well as the distance of the extreme node from the end of the spicule,) is quite regularly the $\frac{1}{2500}$ of an inch.

In the smaller spicules the nodes are crowded nearer together, are apparently composed of minute rounded projections, and resemble rings or raised fillets; but in the larger the spines are distinct.

These spines are acute and conical, like the extremities of the spicule. Their length varies slightly and irregularly among the nodes but never exceeds the $\frac{1}{12000}$ of an inch.

The number of spines on each node diminishes from the fractured end towards the apex of the spicule, but in a progression which varies in different specimens. In the largest spicule found (fig. 4) the grouping of the spines seems to be as follows from one extremity of the perfect spicule to the other, supplying from analogy those upon the unseen side and upon the lost half of the spicule:

Groups.	3 nodes (terminal),	Spines on each node	4	total	12
	8 nodes,	" " "	6	"	48
	3 nodes (central),	" " "	8	"	24
	8 nodes,	" " "	6	"	48
	3 nodes (terminal),	" " "	4	"	12
Total,	25 nodes,				144 spines.

In the same way, another specimen had two central nodes of 8 spines each, five nodes on either side of 6 spines, and three terminal nodes of 4 spines: 100 spines in all. Again, in other specimens, no 8-spined nodes seem to be present, but my observations have been too few to determine the extent of the variation.

The central tube appears to pass uninterruptedly through the nodes, to diminish in breadth towards the extremity of the spicule, and to present an open orifice there, into which the water enters when the spicule is immersed. The dimensions of these spicules are as follows:

	Variation.	Mean.	No. of Measurements.
Length, - - - - - - - -	$\frac{1}{625}-\frac{1}{227}$ inch	$\frac{1}{357}$ inch	9
Diameter at middle, - - -	$\frac{1}{12500}-\frac{1}{3571}$ inch	$\frac{1}{5555}$ inch	9
" " end, - - - -	? $-\frac{1}{8333}$ inch	?	1
" of central tube, - -	$\frac{1}{50000}-\frac{1}{25000}$ inch	$\frac{1}{30000}$ inch	3
Ratio of bore to diam. of spicula,	$\frac{1}{4}-\frac{1}{7}$	$\frac{1}{6}$	3

The chemical and physical characteristics of this variety are apparently identical with those of the former.

Both these varieties are related to the spicules of sponges, in their general characteristics; and I learn from Prof. A. E. Verrill that a sponge secreting such spicules exists in the West Indies. They may come from species of Hyalonema, which, according to published descriptions, are Gorgonioid corals having an axis of parallel siliceous fibers; but Mr. Verrill states that the species of Hyalonema are probably sponges. However this may be, there is a decided parallel arrangement in certain portions of the original mass, and even a firm cohesion of little bundles of the parallel fibers, which may imply that they were not disseminated through their organic matrix in the irregular manner usual to the spicules of a sponge, but occurred in a coherent mass of parallel fibers.

Although the mass of spicules is so light as almost to float, nevertheless it rapidly falls to pieces when placed in water. It is probable therefore that it has not been thrown up by the sea in its present form but is the residue from the decomposition of a sponge in the sand. Many sponges have been frequently thrown up by heavy ground swells at the locality in which the mass was found. The negroes state that similar white masses have been observed on the beach of the island of St. Martin.

School of Mines, Columbia College, New York. June, 1865.

ART. XXXIX.—*Researches on the Volatile Hydrocarbons*; by C. M. WARREN.*

OF THE CALCULATED BOILING-POINTS OF HYDROCARBONS BY SCHRÖDER'S THEORY.

THE subjoined tables exhibit the theoretical boiling-points of the above mentioned hydrocarbons,[1] as calculated according to Schröder's last theory, in comparison with the boiling-points actually found. By this theory, as already stated, each double atom of carbon (C_2) contained in a body is supposed to influence the boiling-point by 30°, and each double atom of hydrogen (H_2) to influence the same—10°; from the sum of these influences the number 70° is in all cases to be deducted, in order to find the boiling-point.

1. *Hydrocarbons from Pennsylvania Petroleum.*

1ST SERIES.

Formula.	Determined Boiling-point.	Calculated boiling point by Schröder's theory	Difference between Calculated and Determined boiling point.
	°	°	°
C_8H_{10}	0·0 (?)	0	
$C_{10}H_{12}$	30·2	20	10·2
$C_{12}H_{14}$	61·3	40	21.3
$C_{14}H_{16}$	90·4	60	30.4
$C_{16}H_{18}$	119·5	80	39.5
$C_{18}H_{20}$	150·8	100	50.8

2D SERIES.[2]

Formula (?)	Determined Boiling-point.	Calculated boiling point by Schröder's theory.	Difference between Calculated and Determined boiling-point.
	°	°	°
C_8H_{10}	8–9	0	8–9
$C_{10}H_{12}$	37·0	20	17·0
$C_{12}H_{14}$	68·5	40	28·5
$C_{14}H_{16}$	98·1	60	38·1
$C_{16}H_{18}$	127·6	80	47·6

3D SERIES. (Not completed.)

Formula.	Determined Boiling-point.	Calculated Boiling-point by Schröder's theory.	Difference between Calculated and Determined boiling-point.
	°	°	°
$C_{20}H_{20}$	174·9	130	44·9
$C_{22}H_{22}$	195·8	150	45·8
$C_{24}H_{24}$	216·2	170	46·2

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* Concluded from p. 232.

[1] To avoid useless repetition, the hydrocarbons from Albert coal oil will be omitted in this series of tables, they being considered identical with the corresponding bodies from petroleum.

[2] See foot-note on page 227.

2. *Hydrocarbons from Coal-tar Naphtha.*

Name of substance.	Formula.	Determined boiling point.	Calculated boiling-point by Schroder's theory.	Difference between calculated and determined boiling-point.
		°	°	°
Benzole,	$C_{12}H_6$	80·0	80	0·0
Toluole,	$C_{14}H_8$	110·3	100	10·3
Xylole,	$C_{16}H_{10}$	139·8	120	19·8
Isocumole,	$C_{18}H_{12}$	169·9	140	29·8

3. *The Homologous Hydrocarbons from Oil of Cumin and Cuminic Acid.*

Name of substance.	Formula.	Determined boiling-point.	Calculated boiling-point by Schroder's theory.	Difference between calculated and determined boiling-point.
		°	°	°
Cumole,	$C_{18}H_{12}$	151·1	140	11·1
Cymole,	$C_{20}H_{14}$	179·6	160	19·6

It appears, therefore, that the theory of Schröder finds no support from any one of the different series of hydrocarbons presented in these tables. The discrepancy between the observed and calculated boiling-points, as shown, varies from about 10° to 50° C. This discrepancy is found to increase pretty uniformly by about 10° as we rise from one member to the next higher in the ascending series. In the series of the formula $C_n H_n$, however, the discrepancy is nearly a constant one, viz. about 46°. I would not overlook the fact, that the calculated boiling-point of benzole is absolutely identical with that found by experiment; nor the remarkable coincidence, that the agreement is almost perfect between the probable boiling-point, and that obtained by calculation, for the body of the probable formula $C_8 H_{18}$ in the 1st Series from petroleum. It is obvious, however, that these are merely accidental circumstances, to which no importance can attach.

OF THE CALCULATED BOILING-POINTS OF HYDROCARBONS BY LÖWIG'S THEORY, VIZ. THAT ONE ATOM OF CARBON (C) RAISES THE BOILING-POINT 38°·4, AND ONE ATOM OF HYDROGEN (H) LOWERS IT 29°·2.

Hydrocarbons from Pennsylvania Petroleum.

1ST SERIES.

Formula.	Determined boiling-point.	Calculated boiling-point by Lœwig's theory.	Difference between calculated and determined boiling-point.
	°	°	°
$C_8 H_{10}$	0·0 (?)	15·2	
$C_{10}H_{12}$	30·2	33·6	3·4
$C_{12}H_{14}$	61·3	52·0	9·3
$C_{14}H_{16}$	90·4	70·4	20·0
$C_{16}H_{18}$	119·5	88·8	30·7
$C_{18}H_{20}$	150·8	107·2	43·6

2d Series.[3]

Formula. (?)	Determined boiling-point.	Calculated boiling-point by Lœwig's theory.	Difference between calculated and determined boiling-point.
	°	°	°
$C_8 H_{10}$	8–9	15·2	6·7
$C_{10}H_{12}$	37·0	33·6	3·4
$C_{12}H_{14}$	68·5	52·0	16·5
$C_{14}H_{16}$	98·1	70·4	27·7
$C_{16}H_{18}$	127·6	88·8	38·8

3d Series. (Not yet completed.)

Formula.	Determined boiling-point.	Calculated boiling-point by Lœwig's theory.	Difference between calculated and determined boiling-point.
	°	°	°
$C_{20}H_{20}$	174·9	184·0	10·9
$C_{22}H_{22}$	195·8	202·4	6·6
$C_{24}H_{24}$	216·2	220·8	4·6

A cursory examination of the last three tables will suffice to show that, so far as regards the hydrocarbons of the formulæ $C_n H_n$ and $C_n H_{n+2}$, the theory of Löwig also has no foundation in fact. That his theory did not hold good with the hydrocarbons of the formula $C_n H_{n-6}$ was observed by Löwig himself, who found that it would place the boiling-point of benzole at 285°·6, i. e. 205° above its actual boiling-point.

Of the Calculated Boiling-points of Hydrocarbons by Gerhardt's Theory.

We come finally to test the law of Gerhardt, above mentioned. Inasmuch as this law was especially designed to apply exclusively to the hydrocarbons,—upon the observed boiling-points of some of which it was indeed founded,—we should naturally expect to find this more in accordance with facts than either the hypothesis of Schröder or that of Löwig, both of which claimed to be of more general application, and were framed more especially with reference to other classes of substances. The facts presented in the following tables will show, however, that this is

1. *Hydrocarbons from Pennsylvania Petroleum.*

1st Series.

Formula.	Determined boiling-point.	Calculated boiling-point by Gerhardt's theory.	Difference between calculated and determined boiling-point.
	°	°	°
$C_8 H_{10}$	0·0 (?)	− 8·0	
$C_{10}H_{12}$	30·2	12·5	17·5
$C_{12}H_{14}$	61·3	33·0	28·3
$C_{14}H_{16}$	90·4	53·5	36·9
$C_{16}H_{18}$	119·5	74·0	45·5
$C_{18}H_{20}$	150·8	94·5	56·3

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[3] See foot-note on page 227.

far from being the case; and that the theory of Gerhardt, as well as those of Schröder and Löwig, so far as these relate to the hydrocarbons, was by no means legitimately drawn from nature, but is altogether artificial.

2D SERIES.[4]

Formula. (?)	Determined boiling-point.	Calculated boiling-point by Gerhardt's theory.	Difference between calculated and determined boiling-point.
C_8H_{10}	8–9°	–8°	16°·5
$C_{10}H_{12}$	37·0	12·5	24·5
$C_{12}H_{14}$	68·5	33·0	35·5
$C_{14}H_{16}$	98·1	53·5	44·6
$C_{16}H_{18}$	127·6	74·0	53·6

3D SERIES.

Formula.	Determined boiling-point.	Calculated boiling-point by Gerhardt's theory.	Difference between calculated and determined boiling-point.
$C_{20}H_{20}$	174°·9	130°·0	44°·9
$C_{22}H_{22}$	195·8	150·5	45·3
$C_{24}H_{24}$	216·2	171·0	45·2

2. *Hydrocarbons from Coal-tar Naphtha.*

Name of substance.	Formula.	Determined boiling-point.	Calculated boiling-point by Gerhardt's theory.	Difference between observed and calculated boiling-point.
Benzole,	$C_{12}H_6$	80°·0	93°·0	13°·0
Toluole,	$C_{24}H_8$	110·3	113·5	3·5
Xylole,	$C_{16}H_{10}$	139·8	134·0	6·0
Isocumole,	$C_{18}H_{12}$	169·9	155·5	15·5

3. *Hydrocarbons from Oil of Cumin and Cuminic Acid.*

Name of Substance.	Formula.	Determined boiling-point.	Calculated boiling point by Gerhardt's theory.	Difference between observed and calculated boiling-point.
Cumole,	$C_{18}H_{12}$	151°·1	154°·5	+3°·4
Cymole,	$C_{20}H_{14}$	179·6	175·0	–4·6
Cumo-oil of turpentine,	$C_{20}H_{16}$	155·4	160·0	+4·6

The chief conclusions deduced from the foregoing facts and considerations may be briefly summed up as follows:

1. That the boiling-point difference for the addition of C_2H_2 in homologous series of hydrocarbons is generally 30° C., which is a much larger difference than has been commonly supposed.

2. That of the five series of hydrocarbons examined, only one series was found exceptional to the rule just stated, and this presented the boiling-point difference of about 20°, which is but

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[4] See foot-note on page 227.

little larger than the number 19°, which Kopp found so common with other classes of substances.

3. That certain series of derivatives from the benzole series of hydrocarbons present boiling-point differences corresponding to the elementary difference of C_2H_2, considerably smaller than the number 19° of Kopp.

4. That the formulæ of Schröder, Löwig, and Gerhardt, for the calculation of boiling-points, so far as these may be supposed to relate to the hydrocarbons, are incorrect and purely artificial.

5. That the custom of taking boiling-points with the bulb of the thermometer in the vapor is more liable to lead to an erroneous determination, at least in certain cases, than if the bulb be placed in the liquid.

SCIENTIFIC INTELLIGENCE.

1. *Iron regions of Arizona;* by W. P. Blake (from a letter to J. D. Dana, dated San Francisco, Cal., Aug. 31, 1865).—Your note in the Journal (xxxix, 358,) upon the probable Azoic age of the iron ores of Mexico, described by Mr. Manross, induces me to send you a notice of the great abundance of specular iron ore in the upper part of Arizona, near Williams fork of the Colorado, observed by me in 1863. The ore is chiefly the micaceous variety, and occurs in thick beds and in thin sheets in a ferruginous limestone or dolomite, evidently metamorphic, and tilted at a high angle. The whole forms a belt of peculiar appearance, which may be traced by the eye for miles across the country in a direction a few degrees south of west, so as to pass over the Colorado, and into the limits of California. It is interstratified with chloritic and talcose slates, gneiss and granite, and the series has yielded both copper ores and gold. The iron ore is found in large boulders or blocks in most of the ravines; and as it resists decomposition by the weather better than the limestone, it stands out upon its surface in rough sharp ridges. Numerous specimens of similar ore were collected in the ranges north, and on the Cerbat and Aquarius mountains, by Lt. Whipple's party in 1863, of which Mr. Marcou was the geologist. It would thus appear that the ferruginous formation extends over a considerable area north and south, and it may possibly be connected with the Mexican series, and be pre-silurian. The antiquity of the Aztec and the Aquarius ranges and the metamorphic region about them, as compared with the Sierra Nevada and the Rocky mountains, was commented on by me in vol. iii, p. 59 of the Pacific R. R. Reports. At Williams fork a heavy white sand-rock and a conglomerate, of ancient appearance, overlie the granitic and metamorphic series unconformably. [If the above iron-bearing rocks are true pre-silurian, they afford, we believe, the first example yet published of such rocks being auriferous. The beds of iron ore to be of value as evidence of age should be of extraordinary thickness.—J. D. D.]

2. *On an oil-well boring at Chicago;* by Geo. A. Shufeldt, Jr. (From a letter to Prof. Erni, dated Chicago, Sept. 16, 1865, and communicated

for this Journal.)—I have recently bored a well at Chicago 711 feet in depth. The surface rock is the Upper Silurian; for the first forty feet it is porous and much decomposed; and it is so filled with petroleum that when exposed to the sun the oil oozes out, and the rock will burn almost like coal. From this rock, when the well was forty feet deep, I pumped 100 gallons of oil, but the fountains or sources of the oil were evidently much deeper, as the drill proved. The next stratum, 200 feet thick, is what we call here Joliet marble, from the name of the place where it outcrops; it is a yellowish-white stone and very compact, and affords no signs of oil. Next below, for 200 feet, there are bands of grey limestone with oil in the seams; then a band of shale, 156 feet thick, saturated with petroleum. This shale I understand separates the Upper and Lower Silurian, and where it (the shale) rests upon the underlying rock, oil was found in considerable quantities. Beneath this shale we entered the first band of sandstone rock of a reddish color, abounding in oil, and having a thickness of 71 feet.

At a depth of 711 feet we penetrated a stream of water which stopped further progress. This water is perfectly limpid, and is free from all traces of sulphur or other disagreeable minerals, and flows 500,000 gallons of water per day through a 3½ inch orifice. Its head is 120 feet above Lake Michigan.

3. *On the Drift in Brazil, and on decomposed rocks under the Drift;* from recent observations by Prof. AGASSIZ. Communicated for this Journal by ALEXANDER AGASSIZ.—At Tijuca, a cluster of hills about 1,800 feet high, and about seven or eight miles from Rio, there is a drift hill with innumerable erratic boulders as characteristic as any ever seen in New England. Prof. A. had before seen unmistakable traces of drift in the Province of Rio and in Minas Geraes; but there was everywhere connected with the drift itself such an amount of decomposed rocks of various kinds, that it would have been difficult to satisfy any one not familiar with drift that there is here an equivalent of the northern drift. There is found at Tijuca the most palpable superposition of drift and of decomposed rocks, with a distinct line of demarcation between the two.

This locality afforded an opportunity of contrasting the decomposed rocks, which form a characteristic feature of the whole country, with the superincumbent drift, so as to be able hereafter to distinguish both, whether found in contact or separately. These decomposed rocks are quite a new feature in the structure of the surface of the country. Granite, gneiss, mica slate, clay slate, in fact all the various kinds of rocks usually found in old metamorphic formations are reduced to the condition of a soft paste, exhibiting all the mineralogical elements of the rocks as they may have been before they were decomposed, but now completely disintegrated, and resting side by side as if they had been accumulated artificially. Through this loose mass there were here and there larger or smaller veins of quartz rock, or of granite or other rocks, equally disintegrated; but they retain the arrangement of their materials showing them to be disintegrated veins in large disintegrated masses of rocks. The whole passes unmistakably to rocks of the same kind in which the decomposition or disintegration is only partial, or no trace of it is visible, and the whole mass exhibits then the appearance of a set of ordinary metamorphic rocks. I tis plain that such masses forming everywhere the surface of the country must be a great obstacle to the study of erratic phenomena, and it is not wonderful that those who seem familiar with the country should entertain the idea that the surface rocks are everywhere decomposed, and that there is no erratic formation or drift here. But upon close examination it is easy to see that, while the decomposed rocks consist of the small particles of the primitive rocks, which they represent, with their veins and all other characteristic features, there is not a trace of larger or smaller boulders in them; while the superincumbent drift consisting of similar parts does not show the slightest sign of the indistinct stratification characteristic of the decomposed metamorphic rocks below it, nor any of the decomposed veins, but is full of various kinds of boulders of different dimensions. The boulders have not yet been traced to their origin; the majority consist of a kind of greenstone, composed of nearly equal amounts of a greenish black hornblende and feldspar. This greenstone is said by mining engineers to be found in the Entre Rios on the Parahyba, where iron mines are worked in a rock like these boulders. Thus far evidence has been furnished of the action of glaciers only in the extensive accumulations of drift similar in its characteristics to northern drift, but no trace has been found of glacial action, properly speaking, such as polished surfaces, scratches and furrows.

The decomposition of the surface rocks to the extent to which it takes place here is very remarkable, and points to a geological agency thus far not fully discussed in our geological theories. It is obvious that the warm rains falling upon the heated soil must have a very powerful action in accelerating the decomposition of rocks; it is like torrents of hot water falling for ages in succession upon hot stones, and instead of wondering at the amount of decomposed rocks, we should rather wonder that there are any rocks left in their primitive condition.

4. *Mining Statistics of Great Britain for* 1864.—

	Produce of mines.	
Coal,	92,787,873	tons.
Iron ore,	10,064,890	"
Pig iron,	4,767,951	"
Copper ore,	214,604	"
Metallic copper,	13,302	"
Lead ore,	94,433	"
Metallic lead,	91,283	"
Silver (from lead ore),	641,088	ounces.
Zinc ores (nearly all sulphuret),	15,407	tons.
Metallic zinc,	4,040	"
Iron pyrites (for sulphuric acid and soda works),	94,458	"
Tin ore,	15,211	"
Metallic tin,	10,108	"
Gold (from Merionethshire),	2,887	ounces.

The gross value of the above mineral products was 39,979,837*l*. There were 2,351,342 tons of coal taken to London in 1864 and 1,786,713 tons in 1863. Eight and a half millions of tons of coal were exported in 1864, and nearly half a million of tons of iron.—*Athen.*, *Aug.* 19.

5. *Cretaceous Reptiles of the United States*, by JOSEPH LEIDY, M.D. 136 pp. 4to, with 20 plates. From the Smithsonian Contributions to Knowledge, vol. xiv, 1865.—Dr. Leidy has here described in detail and figured with great beauty and perfection, the Saurian remains of the American Cretaceous. The species included are the following *Thoracosaurus :*

Bottosaurus Harlini, undetermined species of *Crocodiles*, *Hyposaurus Rogersii*, *Discosaurus vetustus*, *Cimoliosaurus magnus*, *Piratosaurus plicatus*, *Mosasaurus*, *Macrosaurus lævis*, *Polygonodon vetus*, *Hadrosaurus Foulkii*, undetermined reptiles allied to *Hadrosaurus*, *Astrodon Johnstoni*, *Tomodon horrificus*, *Pliogonodon priscus*, *Chelonia*, *Chelone sopita*, *Chelone ornata*, *Emys firmus*, *Emys beatus*, *Emys pravus*, *Platemys sulcatus*, *Bothremys Cookii*, *Trionyx priscus*.

6. *Note on the Discovery of Rhizopods in the Azoic;* by JAMES D. DANA.—It may be thought that the recent discovery of the Eozoön in the Laurentian of Canada is at variance with the writer's Manual of Geology, in which the Laurentian is styled *Azoic*. But in truth the book and facts are in more perfect harmony for the discovery made since its publication. The author suggests on page 147 that Rhizopods may have existed in the Azoic along with lime-secreting marine vegetation, as the sources of the material of the Azoic limestone strata; and on page 596, after alluding to marine locomotive (animal-like) Protophytes with other Algæ as probably commencing in the Azoic, and as the *first* of comprehensive types, it is added:

"The Protozoans (Rhizopods, etc.) may have been the *associated* species of the *superior* group, as remarked on page 147. The two are alike in extreme simplicity of organization. In Algæ the Radiate type of structure, characteristic of the typical plant, is not brought out; and in Protozoans neither of the four great Animal types appears,—the Radiate, Molluscan, Articulate, or Vertebrate. If, therefore, these simple species existed in the Azoic era, they were *systemless* life, and only foreshadowed the great systems of life which were afterwards displayed according to their respective types in the true *Zoic* ages."

The reader will apprehend the full bearing of this sentence if he will re-peruse the chapter on the Progress of Life from which it is taken.

7. *Report on the Mines of New Mexico;* by Prof. R. E. OWEN, Geologist, and E. T. Cox, Geologist and Chemist. 60 pp. 8vo. Washington, 1865.—This pamphlet gives a brief notice of the general geology of the region examined. It mentions a five-foot bed of bituminous coal of Cretaceous age in the Raton Mountain close to the stage road; and two miles N.E. of Santa Fe, a thin bed of coal with coal-plants and other fossils (*Productus Cora*, *P. semireticulatus*, &c.) of the Carboniferous formation. At the Organ Mts., in the southern part of the territory, the Carboniferous limestone is converted into crystalline marble; and at the Hannover copper mines, it is partly fossiliferous, and partly so crystallized as to resemble the adjoining granite rock. In the same region there is a bed of magnetic iron ore, "forming a belt 75 to 100 feet wide," and near the junction with the stratified rocks there is an iron breccia. The carboniferous limestone, with *P. Cora*, was found in the Jemez valley and Cañon of San Diego. Near the Placer Mtn., a bed of true anthra-

cite, nearly 5 feet thick, exists, due to a porphyritic dike intersecting the Carboniferous formation which is much tilted. The authors state that this metamorphism and disturbance must have taken place after the Carboniferous age.

8. *Defense des Colonies:* III, Etude Générale sur nos Etages G–H, avec application spéciale aux environs de Hlubocep, pres Prague; by JOACHIM BARRANDE. 368 pp. 8vo., with a chart and a sheet of profiles. 1865. Prague and Paris.—Barrande has discussed in this work the subject of "Colonies" as connected with the distribution of Paleozoic fossils, in a way to command attention and excite an interest in his views. The volume is also very valuable for the review it gives of the paleontology of Bohemia. No justice to the work can be done in a brief notice—which is all that our space in this number will allow; and we commend the volume entire to all interested in geological science.

9. *Supplement to the Ichnology of New England.*—A Report to the Government of Massachusetts in 1863. By EDWARD HITCHCOCK, D.D., LL.D., late Professor of Geology in Amherst College. 96 pp. 4to, with 20 plates. Boston, 1865.—This Report, the last work of the late Professor Hitchcock, contains descriptions and illustrations of new species of tracks, among which are some strange forms, well termed enigmatical. A number of the plates are fine photographs, a style of illustration especially important for the minute tracks, like those of insects, and excellent for all. The bones of the right foot of the Saurian remains found near the water-shops of the Armory at Springfield, and partly described and commented upon by Professor J. Wyman at p. 187 of the Ichnology, are figured on one of the plates of this Supplement. The opinion of Professor Owen of London is given that they belonged to a Saurian with an unusually thin and compact wall of bone in the limb bones, which, however, might have been occupied by unossified cartilage, as in the young crocodile and turtle; if they were filled with oil or light marrow, it would indicate a relation to Pterodactyles or Birds. The species is named *Megadactylus polyzelus.*

10. *New Dinosaurian from the Wealden Formation, Isle of Wight.*—A new Dinosaurian, for which Prof. Owen has proposed the name *Polyacanthus* (from *πολυς many* and *ακανθος spine*), has been found by Rev. W. Fox. The animal was over 6 feet long from the shoulder to the rump, had a massive tail 5 feet long, legs about 4 feet long, and a broad short foot. It had a bony armor made of plates from ½ in. to 4 in. broad, and ½ in. thick, excepting along the back, over which there was a great bony shield; and along the sides of the body and tail there were spine-like bones, some of which are 15 in. long and weigh 7 pounds.—*Athenæum, Aug.* 5.

11. *Seaside Studies in Natural History;* by ELIZABETH C. AGASSIZ and ALEXANDER AGASSIZ. Marine Animals of Massachusetts Bay. RADIATES. 156 pp. 8vo. Boston, 1865. (Ticknor & Fields.)—The tendency of summer travel to the seashore is creating a demand for popular works on the life of the waters. The "Seaside Studies" will be welcomed as a beautiful and attractive beginning in this department of literature. We say beginning, because it takes up but a single branch—that of the Radiates, and is confined mainly to the species of Massachu-

setts Bay. The work is exact in its science as well as popular in style, and is in fact a contribution to science which the most learned may read with profit. The text is mainly from the graceful pen of Mrs. Agassiz, and the work is dedicated to her husband, Prof. L. Agassiz. The illustrations are numerous and excellent, and the printing and paper all that could be desired.

12. *Entomological Society of Philadelphia.*—The publication committee of this Society propose to publish, and issue gratuitously, an occasional bulletin, under the title of "The Practical Entomologist," in which papers on the insects injurious and beneficial to vegetation will be given for the benefit of the American farming interest. They express in their circular (dated September, 1865,) the hope that the information imparted through this medium will be of use to the agriculturists of the country, by leading them to study critically the Entomological fauna which surrounds them, and to derive, from their knowledge thus acquired, the power to increase the production of their crops and develop the interest which they represent. It is a singular fact that some of our commonest and most injurious insects are least known. Our Cotton, Hop, and various other crops suffer at times immensely from insects whose habits we little understand. The Society aims by its publication to bring out the true history of each and all of our noxious insects; and it is believed that, this attained, simple remedies will suggest themselves, which in certain cases may be merely burning the stubble and vigorous fall ploughing.

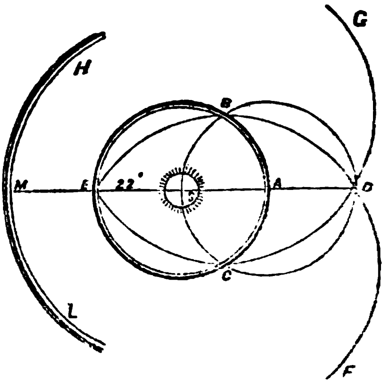
They ask, in order that their plan may be carried out, that those having information will send it by letter to the Secretary of the institution, at No. 518 South Thirteenth Street, Philadelphia, along with alcoholic phials containing specimens of the noxious insects of the region. They also desire papers from scientific observers for the "Practical Entomologist," as only the active coöperation of all interested parties will insure success in an undertaking which is a labor of public benefit, without moneyed recompense.

13. *A classification of Mollusca based on the Principle of Cephalization;* by EDWARD S. MORSE. 20 pp. 8vo. From the Proceedings of the Essex Institute.—A very valuable original paper, based on researches in Molluscan development and bearing on the general subject of zoological classification. We may cite from the paper in a future number.

14. *Natural History: A Manual of Zoology for Schools, Colleges, and the General Reader;* by SANBORN TENNEY, A. M., author of "Geology," Prof. Nat. Hist. in Vassar Female College. 540 pp. 12mo; illustrated with 500 woodcuts. New York. 1865. (C. Scribner.)—The American student in zoology will find Prof. Tenney's Manual a very convenient and useful work. It goes over the animal kingdom, commencing with the highest class, that of Mammals, and illustrates the various subdivisions with descriptions and figures, mentioning and figuring particularly North American species. Each of the prominent groups is thus presented to the eye by means of excellent wood-cuts of animals accesible to the American student, and with a large number of them considering the size of the work. The Manual is well calculated to make the study of zoology popular and easy. It fails of any introductory chapters on the structure of animals.

15. *On a solar halo, seen at Crawfordsville, Ind.*, (lat. 40° 03′); by Prof. J. L. Campbell. (In a letter to the Editors, dated Wabash College, Crawfordsville, May 27, 1865.)—Herewith I submit a report of the remarkable appearances in the atmosphere on May 26th, 1865.

The morning was hazy and cool—the thermometer at 5 ranging about 48° to 50°.



S (Sun), Primary center; A, Secondary center; B and C, Tertiary centers; Radius, SE=22°=AD, SM=44°; MD, Grand axis =88°; Barometer 29·72 in.; Thermometer 70°·F.

About nine o'clock a series of halos or rings began to form about the sun, and continued to increase in brilliancy until near noon—then slowly disappeared during the afternoon—traces being visible at six o'clock in the afternoon. The accompanying diagram is a correct representation of the phenomenon as it appeared at twelve o'clock, noon.

The most brilliant ring was described about the sun as a center with an angular radius (SE) of twenty-two degrees by careful measurement.

The bands were *white*, *yellow*, *red* and *green*. The white was exceedingly brilliant. The yellow and red were also very bright.

The breadth of this series of bands forming the primary ring was about one and a half degrees.

The arc MLG described about the sun as a center with a radius of forty-four degrees presented less brilliant, but well defined colors in the *reversed* order,—the white band being on the outer circumference, and the green within,—with another inner band of violet.

At A on the circumference of the primary circle, the center of the second circle was located. The radius of this circle was the same as that of the primary—its *circumference passing directly through the sun.*

The ring was chiefly white light, of less brilliancy than the primary, yet very sharply marked throughout its entire circumference.

From D the arcs DF and DG branched off symmetrically with reference to the axis MSD, indicating the centers of curvature at B and C, the intersections of the secondary circle with the primary.

These arcs were of white, well defined, mellow light.

The long continued and well defined manifestation of the phenomenon enabled us to take all our observations very carefully, and to repeat them for further accuracy.

The geometrical relations of the centers of these curves are curious and interesting, and the record of these positions may be of some value in connection with future observations of the same character.

In explanation of these appearances there are some difficulties not hitherto presented in discussing halos. For the present the record of the facts only is made.

16. *British Association at Birmingham.*—The meeting of the British Association opened at Birmingham on the 6th of September, Professor Phillips being the President. The number of members at the meeting during the week following was about 1,400, and besides these there were present 508 ladies and 23 foreigners. The funds received amounted to 2227*l.*; and nearly this amount was set apart for various scientific investigations in astronomy, physics, zoölogy, geology, &c., including 35*l.* for oyster culture. The number of papers read was two hundred and sixty. We have no space in this number for a notice of the Proceedings. The next meeting will be held at Nottingham.

17. *Systema Naturæ of Linnæus.*—By letter from Prof. A. E. Nordenskïold of Stockholm we learn that the Academy of Sciences of Sweden is about to have printed a new photo-lithographic edition of the first edition of Linnæus's Systema Naturæ; and he adds, that "To enable us to judge how many copies may be sold it would be of some interest to have an idea of the probable extent of its sale in the United States." This first edition of Linnæus consists of only 13 to 14 leaves in folio and still contains the rudiments of all his great productions.[1]

18. *New Planet.*—Planet No. (84) was discovered on Aug. 26, by Dr. Luther, of the Observatoray at Bilk, near Düsseldorf. On the day of discovery at 10h. 46m. 28s., Bilk mean time, its A. R. was 323° 37′ 49·1″, and its declination N. 14° 20′ 47·1″. It has been named Clio.

19. *Italian Society of Natural Sciences.*—An extraordinary meeting of this Society was held at Spezzia on the 17th of September.

20. *Prof. Berthelot.*—A chair of Organic Chemistry has been founded at the College of France, and Prof. Berthelot appointed to fill the place.—*Reader*, *Sept.* 2.

21. *Statue of Arago.*—An inauguration of the statue of Arago, erected at his birthplace, Estagel, in the Basses Pyrenees, took place on the 31st of August. His son, dissatisfied that he had not received a special invitation, and because there was no commemoration of his father's political services, declined to be present.—*Reader*, *Sept.* 9.

21. *Chambers's Encyclopedia.*—Parts 94, 95, 96, 97 of this valuable Encyclopedia, extending to RID., have been issued by the American publishers, J. P. Lippincott & Co.

OBITUARY.

Admiral WILLIAM HENRY SMITH.—Admiral Smith died at his residence near Aylesbury, Sept. 9th, at the age of 77. He had acted as President of the Astronomical Society, and of the Geographical Society, Vice President of the Royal Society, and Director of the Society of Antiquarians. He retired in 1825 from naval life, and published in 1844 the "Cycle of Celestial Objects," including the "Bedford Catalogue," an excellent hand-book of practical Astronomy, a result of a series of astronomical observations in his Observatory at Bedford. He also published many works or memoirs on Geographical and other subjects.—*From a notice in Reader*, *Sept.* 16.

HUGH CUMMING, died in London, on the 10th of August last, at the age of 74. He commenced his journeyings in behalf of natural history, especially conchology, in 1826, when he built a yacht for the purpose, and undertook a cruise of more than a year's length among the islands of the South Pacific. Returning to Valparaiso, he prepared for explorations along the western coast of southern South America, where he continued for two years collecting in all departments of zoology. In 1835 he started on a new expedition of four years to the Philippine Islands, and made there vast collections in botany and zoology. During the twenty-five years since his return from the East Indies, he was occupied mainly with the arrangement and extension of his collections. He had long been subject to chronic bronchitis and an asthmatic affection, and these were finally the occasion of his death.—*From a notice in Athen., Aug.* 19.

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[1] If persons desiring the book will so write the Editors of this Journal they will communicate the result to the Swedish Academy.

Johann Franz Encke.—Encke, the Director of the Berlin Observatory, died at Spandau, on the 26th of August, 1865. He was born on the 23d of September, 1791, at Hamburg, where his father was a clergyman.

William Rowan Hamilton.—Sir Wm. Rowan Hamilton, Astronomer Royal for Ireland, died on the 2nd of September, at the age of sixty. He became known as a mathematician of extraordinary genius when he was about twenty years old.

His papers on systems of rays, on the methods of dynamics, on algebra looked at as the science of pure time, on discontinuous functions, on equations of the fifth degree, and his *new algebra*, the *Quaternions*, cannot be popularized. But there is one little result of which an idea can be given, one of the earliest of Hamilton's discoveries, and one which alone would carry down his name to posterity. Hamilton found, from optical theory alone, by reasoning on the properties of light, that under certain circumstances a ray, instead of being refracted as a ray, should, if the theory were true, split into a cone of rays. This *conical refraction*, on being looked for under the proper circumstances by Prof. Lloyd, was actually found to exist. No such phenomenon had ever been even imagined; and it may be justly said that no more remarkable triumph of theoretical prediction had then occurred in the history of science. If we must add to it, as a match, the prediction of Neptune by Leverrier and Adams, each of these brilliant feats does honor to the other. Sir W. R. Hamilton was a man of very wide pursuits, and very varied talents; he was a scholar, a metaphysician, and a poet. He was beloved for the kindness of his heart, and respected for the integrity of his character.—*From a notice in Athen., Sept.* 9.

John T. Plummer, M.D.—Dr. Plummer died on the 10th of April, at his residence in Richmond, Indiana, aged 58 years. He was a graduate of the Medical School of Yale College in 1828. While not making chemistry and natural science his special study, he made collections and observations in different departments, and formerly contributed short papers to the pages of this Journal, and to those of the Journal of Pharmacy. He was a member of the Society of Friends (Quakers), and one of the best of men. He was averse to all public show, and quiet and unobtrusive to an extreme. He has left numerous manuscripts, the larger part of which relate to the Society of Friends, of which he was one of the most influential members.

GENERAL INDEX

OF

VOLS. XXXI—XL, SECOND SERIES.

Note.—In the statement of the number of the volume, the xxx (thirty) is omitted. The names of minerals are inserted only under the word MINERAL. The references to articles on Botany, Geology and Zoology, are grouped under these words, but at the same time are in general inserted in their places elsewhere.

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APPENDIX.

New Planet.—Prof. WATSON, of Ann Arbor, Mich., announces the discovery by him of another planet, on the 9th of October. In a second letter to the editors, dated Oct. 16th, he adds: "I have just received a letter from Prof. Ferguson, from which it appears that the planet discovered by me on the 9th inst. was discovered by Dr. Peters at Hamilton College on the 20th of September. I did not know of this prior discovery until to-day. It seems that Dr. Peters communicated his discovery to the Observatory at Washington, but so far as I know no further announcement was made.

"From a little examination which I have made I feel pretty sure that this planet is identical with *Sappho* ⊙, discovered by Pogson at Madras on May 2d, 1864, but subsequently lost. The circumstances of the motion of the new planet, as far as I have observed it, agree precisely with the hypothesis of identity with Sappho, according to Pogson's observations from May 3d to May 12th, 1864."

On Prairies.—On the subject of Prairies, some excellent observations are made by H. Engelmann, in volume xxxvi of this Journal, in an article, commencing on p. 384, on the causes producing the different characters of vegetation known as Prairies, Flats and Barrens in Southern Illinois.—J. D. D.

Meteorites.—In a pamphlet on Meteorites (Aërolites) by R. P. GREG of Manchester, England, the author presents the following System of Arrangement:

CLASS I. AËROLITES.

ORDER A. Sp. gr. mostly between 1·7—3·2, containing little or no metallic iron.

Trachytic!

Group *a*. *Carbonaceous;* blackish, and containing carbon.

Group *b*. *Howarditic;* ash-gray, fine-grained matrix, somewhat resembling trachyte, and containing imbedded crystals of olivine, anorthite, or augite; outside crust highly resinous and pitch-black.

Group *c*. *Feldspathic;* containing, or consisting of, a mixture of anorthite and augite; crust pitch-black and highly resinous. "Eukrite" of Rose.

Group *d*. *Crystalline;* peridotic, shalkitic, chladnitic; (magnesia-silicates).

Group *e*. *Vesicular.*

Order B. Sp. gr. mostly between 3·25 and 3·9.

Group *a*. *Variolitic ;* after the manner of the mineral called Variolite.

Group *b*. *Sommitic ;* consisting of finely mixed crystalline minerals; something after the nature of the ejected masses of Mt. Somma, Vesuvius.

Group *c*. *Tufaceous ;* mixed; spherules numerous. } "Porphyritic"
c_1; composite, crude, &c. } of Shepard.
c_2; brecciated, marbled, &c.

Group *d*. *Psammitic ;* arenaceous, like sandstone, granular; color yellowish or brownish white; particles chiefly olivinoid; metallic particles freely and visibly interspersed, occasionally with the addition of magnetite and graphite, or veined and stained with rust; Bachmut may be taken as the type. Polished surfaces show more or less of spherules.
d_1; ditto, very fine-grained texture.
d_2; ditto, grayish; texture more compact or tough; New Concord and Lixna may be taken as types.

Group *e*. *Chondritic ;* structure coarse-grained, grayish.
e_1; oolitic or pisiform.
e_2; containing small angular crystalline fragments.

Group *f*. *Blanskitic ;* after the manner of the Blansko meteorite; color grayish blue, occasionally running into *c* or d_2; probably contains more Labradorite or augite than *d* or d_2. Texture rather uniform.

Group *h*. *Erxlebenetic ;* texture fine-grained, tough, and gritty; highly peridotic and metalliferous, with a slight bronze-like luster; this represents quite a peculiar group, of which the Erxleben meteorite may be taken as the type.

Class II. SIDEROLITES.

Order C. Sp. gr. 4·0—7·0 meteoric iron, containing, or mixed with, stony matter and silicates.

Group *a*. *Pallasites ;* olivine, coarsely crystalline.
a_1; ditto, finely crystalline.

Group *b*. Partially or irregularly mixed with silicates.

Group *c*. Containing aërolithic fragments, imbedded in iron showing Widdmannstättian figures.

Class III. METEORIC IRONS, or AËROSIDERITES.[1]

Order D. Sp. gr. between 7·0 and 8·0.

Group *a*. *Agrammic ;* without line-markings, not lettered when etched with acid.
a_1; nacreous, pearly lustered.
a_2; spotted, or dotted.

Group *b*. *Microgrammic ;* minutely marked.

Group *c*. *Eugrammic ;* distinctly marked; lines parallel to the sides of the octohedron.

Group *d*. *Spora-grammic ;* scattered lines, finely marked.
d_1; ditto, coarsely marked.

Group *e*. *Nephelic ;* convoluted, or clouded markings.

Group *f*. *Undetermined ;* markings doubtful, or altered by artificial heat.

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[1] Frequently containing imbedded portions of pyrites, magnetite, graphite, and schreibersite.

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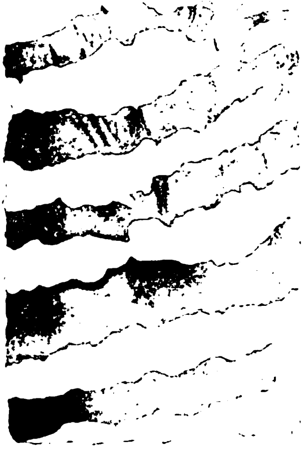

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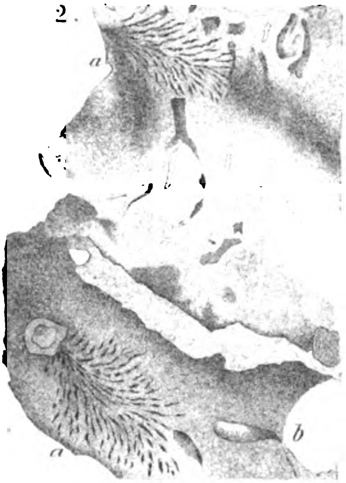

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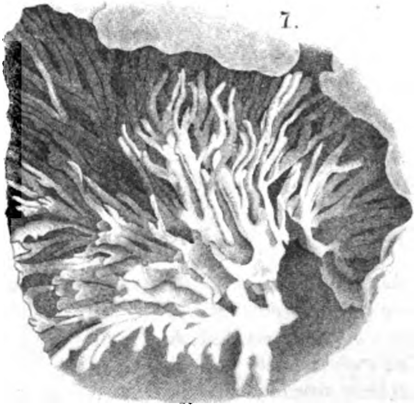

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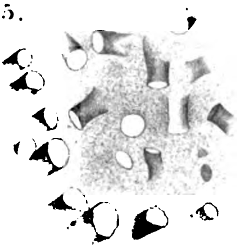

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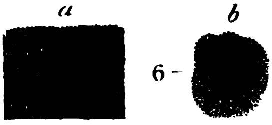

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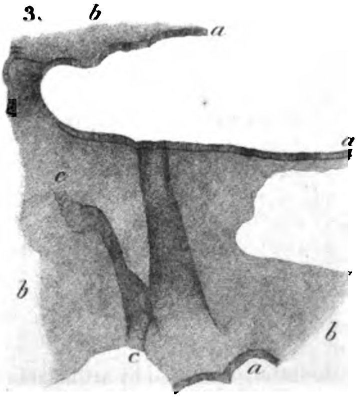

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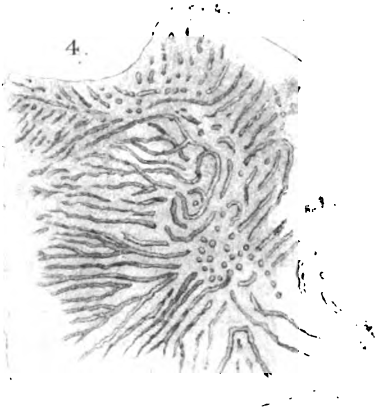

EOZOON CANADENSE, Dawson.